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Of Nature trusts the mind which builds for aye."—WORDSWORTH

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Of Nature trusts the mind which builds for aye."—WORDSWORTH.

THURSDAY, MAY 1, 1890.

THE APPLICATION OF THE MICROSCOPE TO PHYSICAL AND CHEMICAL INVESTIGATIONS.

Molekularphysik, mit besonderer Berücksichtigung mikroskopischer Untersuchungen und Anleitung zu Solchen, sowie einen Anhang über mikrochemische Analyse. Von Dr. O. Lehmann, Professor der Electrotechnik am kgl. Polytechnikum zu Dresden. 2 Volumes, pp. 852, 697, with 624 Woodcuts and 10 Plates. (Leipzig: W. Engelmann, 1888–89.)

VERY soon after the first invention of the microscope, attempts were made to apply the new instrument to solve some of the remarkable problems of crystallogenesis. The early volumes of the Royal Society Transactions contain in the papers of Boyle, Hooke, and Leeuwenhoek, published between the years 1663 and 1709, many records of attempts of this kind; and the works of Henry Baker, which appeared between 1744 and 1764, are also largely concerned with the study of the process of crystallization under the microscope.

In Germany, Ledermuller in 1764 and Gerhardt in 1780 showed the value of the microscope in studying the internal structure of crystals; while in France a long succession of enthusiastic investigators, Daubenton, Dolomien, Fleurian de Bellevue, Cordier, and others, were busily engaged in laying the foundations of the science of microscopical petrography.

Early in the present century, we find the English investigators once more taking a leading part in applying the microscope to the study of crystallized bodies. Between the years 1806 and 1862, Brewster published a long series of memoirs, dealing with the microscopical characters of natural and artificial crystals, and the inclusions which they contain. About the year 1850, too, Mr. Sorby commenced his important investigations on the subject, availing himself of the method of preparing transparent sections of rocks and minerals which had been, shortly before this time, devised by William Nicol. Mr. Sorby's epoch-making memoir "On the Microscopical Structure of Crystals, indicating the Structure of Minerals and Rocks" made its appearance in 1858.

While one group of investigators, following the lines of the early work of Brewster and Sorby, have sought to make the microscope an efficient instrument for the determination of minerals, even when present in rocks as the minutest crystals or fragments; others have no less diligently pursued the methods which the same pioneers in this branch of research have initiated for solving physical and chemical problems connected with the formation of crystallized bodies.

In the hands of Des Cloizeaux, Tschermak, Zirkel, Von Lasaulx, Fouqué and Michel-Lévy, Rosenbusch, and other workers, the microscope has gradually been developed into a splendid instrument of mineralogical research; and the determination of the minutest particles of a mineral is now becoming no less easy and certain than that of the largest hand-specimens.

But, at the same time, Brewster and Sorby's early attempts to solve physical and chemical problems by the aid of the microscope have not failed to exercise an important influence on subsequent workers in these branches of science. Link, Frankenheim, Klocke, Harting, and especially Vogelsang (whose early death was a very severe loss to this branch of science), have done much towards establishing the science of crystallogenesis upon a firm basis of accurate observation; and their labours have been continued in more recent times by H. Behrens and Dr. Otto Lehmann, the author of the work before us.

As the well-known treatises of Rosenbusch, and of Fouqué, Michel-Lévy, and Lacroix, give us an admirable *résumé* of the present state of determinative mineralogy, as improved by the application of the microscope, so does the work before us contain a perfect summary of the contributions of the microscopist to the sciences of physics and chemistry.

It will only be possible, within the limits of an article like the present, to indicate briefly the plan of the very comprehensive, and, indeed, almost exhaustive work, in which Dr. Lehmann has embodied the observations of himself and his predecessors in this field of inquiry.

The first division of the book deals with the construc-

tion and use of the microscope; especial attention being given to forms of the instrument, like those devised by Nachet and by the author of this work, for the special purpose of studying crystallization and other physical and chemical processes.

The second division of the book treats of those physical properties of matter which are presented by all bodies, whether in the solid, liquid, or gaseous state. Such questions as the polarization and absorption of light, the conduction of heat, and the electric and magnetic relations of various substances are here dealt with by the author.

The next division relates to the peculiar properties presented by solids. Elasticity and plasticity are considered, and, under the latter head, the remarkable phenomenon of the production of twinned structures in crystals by mechanical means is fully discussed. Under the head of cleavage we find a treatment of such phenomena as the production of mathematical figures in certain crystals by pressure, percussion, &c.; while under the heads of "Enantiotropie" and "Monotropie" are classified the consequences which follow from heteromorphism among crystalline substances, and the tendency of the heteromorphous forms to pass one into the other.

The division dealing with liquids and their peculiar properties contains discussions on fluidity, surface-tension, diffusion, capillarity, and crystal-growth, with the origin of structural anomalies. The problems of solution and precipitation, with those of solidification and fusion, are also treated of in this part of the treatise.

The second volume of the work commences with the discussion of the properties of gases and their relations to solids and liquids. This division of the subject, which is very exhaustively treated, extends to 335 pages.

The work concludes with critical remarks upon different molecular theories. The chapters dealing with the theories of crystal structure, of allotropy, of heteromorphism, and of isomerism, with several others, in the same division of the book, are full of interest and suggestiveness.

A supplement of about 150 pages is devoted to what the author calls "crystal-analysis," or what is generally known to geologists and mineralogists as "microchemical analysis." Very minute particles of an unknown substance may often be determined by being treated with appropriate reagents and studied under the microscope; in this way they are made to yield crystals of various compounds which can be recognized by their characteristic forms and habit. An admirable summary is given by the author of the work of Bôricky, Streng, Behrens, Haushofer, and others, who have gradually perfected this branch of research, and made the method one which is of the very greatest service to the students of microscopical mineralogy and petrography.

While the physicist and chemist will find in this work a perfect mine of interesting and ingenious experiments (many of which are suited to class-demonstrations by projection methods), the mineralogist and geologist will hail the appearance of the book as one that completes and supplements the well-known treatise of Vogelsang—a work that has exercised the most important influence on the development of petrological theory.

In conclusion, it may be pointed out that, not only are the numerous observations of the author on crystallogenesis that are described in memoirs in *Groth's Zeitschrift* included in the work before us, but many others that have never before been published find a place in these volumes. The work is very fully illustrated both with woodcuts and coloured plates, and constitutes a complete synopsis of all that is known on a number of questions of great importance and interest to workers in many different branches of science.

BERTRAND ON ELECTRICITY.

Leçons sur la Théorie Mathématique de L'Électricité, professées au Collège de France. Par J. Bertrand. (Paris: Gauthier-Villars.)

THIS book contains lectures on electricity given by M. Bertrand at the Collège de France. In his preface the author states that he has confined himself to the mathematical principles of the subject; but this hardly expresses the limitation he has imposed upon himself, for a great many results which English students of electricity are accustomed to find in text-books on this subject are omitted from this work. A brief description of the contents of the book will suffice to show this. The first chapter contains an investigation of the attractions of spheres and spherical surfaces when the law of attraction is inversely as the square of the distance; the second and third are devoted to the properties of the potential; the fourth contains an investigation of the conditions under which the method of lines of force can be used; the fifth, which has the comprehensive title "Électricité Statique," contains a short discussion of the electrical distribution on two spheres which mutually influence each other, the reciprocal theorems, and a discussion of the properties of the Leyden jar so far as they can be discussed without introducing the idea of specific inductive capacity; the sixth chapter contains some remarks upon magnets; the seventh treats of Ohm's law, and contains Kirchhoff's equations for the distribution of currents amongst a network of conductors, without, however, any applications even to such an important case as that of Wheatstone's bridge; the eighth, ninth, and tenth chapters contain, respectively, investigations of the magnetic forces produced by linear currents, the laws according to which such currents act on each other, and simple applications of these laws; the eleventh chapter contains some account of the induction of currents, and, amongst other things, some well-founded reasons for not deducing the laws of induction from the principle of the conservation of energy alone, but no hint is given of the possibility of regarding a system of currents as a dynamical system, though the introduction of this idea by Maxwell has thrown new light over the whole subject and enabled many of the properties of currents to be recognized at once as those belonging to any dynamical systems; the twelfth chapter contains some account of the application of the results of the previous chapters to dynamo-electric machines; and the thirteenth and last chapter discusses units.

There are two views which have been taken as to the relation between the mathematics and the physics, which ought to exist in a text-book on mathematical physics: the one is, that it is the province of physics to supply the

laws of action between particles charged with electricity, elements of current, and the like; then its function ceases, and the rest is a mere matter of mathematical symbols; by this method the physics and the mathematics are sharply divided—the physics occurs in the first few lines of the chapter, the rest of which is mathematics. In the other method the physics and mathematics are kept as closely connected as possible, so that by knowing from physics the kind of results we may expect errors in the mathematical investigations may be detected; while, on the other hand, our physical conceptions may be extended, and perhaps even the point of view changed, by the results of mathematical transformations. Thus, as Maxwell points out, the two sides of the equation which expresses Green's theorem might have suggested the two ways of regarding electrical phenomena—the one when we confine our attention to the electrified bodies; the other when we look upon the dielectric as the seat of the phenomena. In the department of physics in which mathematical analysis has won perhaps its greatest triumphs, that of gravitational attraction, the first method is perhaps the most natural; but in an intricate subject like electricity, where so much remains to be discovered, and which it is so important to regard from as many points of view as possible, the second method seems infinitely the more likely to lead to an extension of our knowledge.

M. Bertrand's work is a most favourable example of the first method: it is clearly and gracefully written, and the mathematical part often extremely elegant; yet, in spite of all this, we must confess to a feeling of disappointment on reading the book. We had thought from the publication of Mascart and Joubert's "Leçons sur l'Électricité et le Magnétisme," and the excellent translation of Maxwell's "Electricity and Magnetism" by MM. Seligman-Lui, and Cornu, that the ideas introduced by Maxwell, von Helmholtz, and others, were spreading in France; yet here we have a work written by one of the first scientific men of that country, in which the subject is treated in fundamentally precisely the same way as that in vogue twenty or thirty years ago; and in fact, with the exception of some results due to M. Marcel Deprez, in the chapter on electro-magnetic machines, there is no reference to any investigation made within the last twenty years. The names of Maxwell and von Helmholtz are not even mentioned in the book itself—though, to be quite accurate, that of Maxwell occurs in the table of contents in connection with a particular case of Green's theorem.

M. Bertrand seems to exact more from the science of electricity, before he deems it worthy to be discussed mathematically, than is exacted from any other science; thus, for example, he omits all consideration of the effect of the dielectric because there is no satisfactory molecular theory of specific inductive capacity, such as Mossotti[1] attempted by supposing the dielectric to contain conducting spheres, the specific inductive capacity depending on the ratio of the volume of the spheres to that of the rest of the dielectric. It seems to us that if M. Bertrand were to write a book about optics, he would, if he were consistent, leave out everything connected with either refraction or reflection, since no complete molecular theory of these phenomena have been given. The way in which the dielectric affects the lines of force is as definite and simple as the way in which a refracting medium affects the rays of light, and the one is quite as capable of receiving mathematical treatment as the other.

Again, M. Bertrand, in treating of magnetism, points out that on the theory of magnetic fluids the behaviour of a magnetized body will depend upon the shape of the molecules, and as this is not known he refuses to investigate the magnetic properties of bodies; he never mentions magnetic permeability, the idea of which, by introducing a new property of bodies, enables us to investigate mathematically their magnetic properties, and express the results of the investigation in terms of quantities which can be measured in a physical laboratory.

In spite of the clearness and elegance of this book, we are afraid that a student who learnt his electricity from it would think, if he read any modern memoirs on the subject, that they dealt with some new and unknown science; for the mode of regarding the phenomena would probably be entirely different, and many quantities would be introduced of whose existence M. Bertrand had given him no hint.

OUR BOOK SHELF.

Sundevall's Tentamen [*Methodi naturalis avium disponendarum tentamen*]. Translated into English, with Notes, by Francis Nicholson, F.Z.S., &c. (London: R. H. Porter, 1889.)

THE practice of translating into English memoirs by leading foreign naturalists that may be considered classical is to be highly commended. English ornithologists who are not conversant with German may thus study such important works in their branch of science as Nitzsch's "Pterylographie" and Johannes Müller's "Voice Organs of Passeres," of both of which excellent English translations exist.

It is, however, a question whether Sundevall's "Tentamen" comes into the category of classical memoirs, or is worth translating if it does. In our opinion it might have been allowed to drop peacefully into oblivion in the obscurity of the original Latin. No particular object is gained by helping to perpetuate a scheme of bird-classification like that of Sundevall, with the details of which no one nowadays can agree. Even the translator has nothing to say for it, except the very general statement that "every serious scheme of classification contains some items of progressive knowledge towards the attainment of a complete natural arrangement of the class of birds." It would be very difficult, however, to say what these items are, and the translator gives us no help in the matter. On the other hand, if ornithologists believe that this, the last work of Sundevall, is really important, then it can be certainly said that Mr. Nicholson's translation is good and accurate.

The introduction, which occupies the first twenty-five pages, is interesting, and so are the notes interspersed through the volume; but it is clear that the book must be entirely judged by the merits or demerits of the scheme of classification. Prof. Newton (article "Ornithology," in "Encyc. Brit.," ninth edition) has subjected Sundevall to a searching criticism, which seems to us to be perfectly justified. Some of the worst features of the classification—in addition to those mentioned by Prof. Newton—are to associate *Serpentarius* with any other birds of prey, to place the American vultures near the American kites (an error which is constantly cropping up in spite of the obvious anatomical differences), *Glareola* among the goatsuckers, &c. Prof. Sundevall's classification is, in fact, most reactionary in every particular; it is difficult to believe that it was published in the year 1872—after the appearance of so many important papers upon bird classification and structure, such as those of Profs. Huxley and Parker.

Mr. Nicholson very justly remarks in a footnote to p. 43, that since the publication of the "Tentamen," much has been done in the way of improvement in the classification of birds. In order to assist the student a few references are added to recent publications.

These do not seem to be very well chosen; for example, it is probably much better to arrange the Turdidæ in two sub-families, as has been suggested later, than to retain Sundevall's arrangement. But this seems a very trifling matter in comparison with such serious errors as we have referred to, about which there can be no question, and which are left altogether unnoticed by the translator. F. E. B.

The Flowering Plant: as illustrating the First Principles of Botany. By J. R. Ainsworth Davis, B.A. (London: Charles Griffin and Co., 1890.)

DIFFERENT opinions may be held as to what constitutes an elementary science text-book dealing with first principles, and we are inclined to think that Mr. Davis has given the work before us too modest a title. This little book, of 160 pages, contains enough facts and "hard words" to fill a small Encyclopædia, although "no previous knowledge is assumed"; and we fear that any beginner who limited his studies to this work would run more danger of developing into a kind of living abridged botanical dictionary than of mastering the first principles of the science.

The introduction, which deals with "the scope and subdivisions of the subject," "differences between plants and animals," and "differences between living and non-living matter," is condensed into 5½ pages. The following 137 pages are devoted to morphological and physiological botany; these are succeeded by an appendix on practical work, in which directions for the description of flowering plants, a summary of the various classes and orders, and directions for the study of anatomy, histology, and physiology, are condensed into 15 pages. One cannot help being struck by the author's power of *précis*-writing.

We cannot, therefore, recommend Mr. Davis's book to beginners, for whom a judicious selection of facts from which main principles may be deduced is, in our opinion, necessary. It is no easy task to write a book on "first principles," and this can hardly be accomplished by anyone who has not devoted much time to actual observation in the subject in question.

In his preface the author states that "no attempt has been made to 'write up' (or 'down') to any syllabus;" but the title-page informs us that the book is "especially adapted for the London Matriculation, South Kensington, and University Local Examinations in Elementary Botany." This, we take it, explains the real object of the work, which is also indicated by an appendix, consisting of 153 questions selected from South Kensington and London University examination papers. The appearance of the present work is, in fact, a natural result of our present system of examinations.

Looked upon as a set of condensed notes, recapitulating what has been learnt in lectures which (as doubtless many at the present time *have* to be) are "specially adapted for the requirements" of various examinations, the book may certainly prove useful to many, and from this point of view much might be said in its favour. Moreover, as no specific types are taken, it will probably (for examining bodies do fortunately change their "types" occasionally) have a longer life than the author's "Text-book of Biology."

It is impossible here to criticize the work in detail, and we will only call attention to the insufficient account of growth contained in the introduction: such condensation cannot but result in inaccuracy.

Sixty figures are included in the text, most of which are very well known; some half-dozen are original, but most of these might have been omitted with advantage.

Cycles of Drought and Good Seasons in South Africa. By D. E. Hutchins, Conservator of Forests, Knysna. With Cyclical Diagrams. Pp. 137. (London: William Wesley and Son, 1889.)

MR. HUTCHINS'S little book consists of two lectures (subsequently amplified) which were delivered at King William's Town and Grahamstown in 1886 and 1887. Their subject-matter is fairly indicated in the title, and the author's views are succinctly set forth in the opening words of his second lecture:—"We know that the climate of South Africa varies in cycles, that the climates of other countries similarly placed, such as Australia, South America, and India, also vary in cycles. This cyclical variation is probably due to more causes than one."

Of these cycles, one only, that of the sun-spot period, is already familiar to meteorologists. The others are—a cycle of 9 or 10 years, or, more accurately, 9·43 years as a mean, which Mr. Hutchins terms the "storm cycle," and appears to have been suggested to him by the rainfall register of Cape Town Observatory, extending over 48 years; and one of 12 or 13 years, which he terms the "cyclical mitigation" of the droughts which otherwise prevail in the intervals of the maxima of the two previous cycles. The physical cause of this last is not indicated. Allowing for an occasional delay of a year in the occurrence of the sun-spot rainfall maximum, the vicissitudes of the Cape Town Observatory rainfall are thus fairly reduced to rule. For other stations some modifications are found necessary, and it appears that at certain inland stations and on the east coast a wet year occurs two or three years after that of maximum sun-spots, which Mr. Hutchins terms the "lag rain" of sun-spot maximum. In the register of the Karoo rainfall we also notice a year of "irregular mitigation," and another year of high rainfall not reducible to any cycle, but which is not so annotated.

Perhaps, indeed, we are wrong in assuming that some of the above cycles are new and unfamiliar, since Mr. H. C. Russell, in a paper communicated to the Royal Society of Sydney in 1876, tells us that cycles of 2, 3, 5 or 6, 6 or 7, 9, 10, 11, 12, 13, 17, 19, 30, and 56 years, have been advocated as regulating the rainfall of different places, and we might, of our own knowledge, add others to the list. But with the exception of the sun-spot cycle, all of them seem to be evolved from the rainfall statistics dealt with in each case, and to have no other physical meaning.

It does not seem to have occurred to Mr. Hutchins that, however ingenious as an arithmetical exercise, such analyses of a series of statistics have no more claim to rank as physical inquiry than the solving of acrostic puzzles. He has evidently no misgiving on this head, and is certainly not open to the reproof conveyed in Montrose's well-known lines. He does not fear the fate of his system too much to put it to the touch of a definite and detailed forecast; and under its guidance he has constructed tables showing year by year the occurrence of drought or of average or excessive rain, in some cases for the next half-century. Those therefore who may live to the year 1938 will be in a position to form a definitive judgment on the merits of the system. H. F. B.

Science in Plain Language. By William Durham, F.R.S.E. (Edinburgh: A. and C. Black, 1890.)

MR. DURHAM thinks that there are many intelligent persons who have not time, and may not have the inclination, to read regular scientific works, but who would be glad to know the general results of scientific investigation if these results could be set forth in plain language without too much detail. For this class he has written the present volume, which consists of articles that were originally printed in the *Scotsman*. The subjects are divided into four groups—natural selection, protoplasm, colour, and movement. Under "Natural Selection" there are essays on the origin of species, evolution, the evolution of man, the origin of man's higher nature, the

antiquity of man, primæval man, and ancient lake-dwellings. The section on "Protoplasm" includes papers on the origin of life, the basis of life, bacteria, disease germs, and fermentation. Under "Colour" we find articles on the colour of flowers, the colour of animals, and warning colours and mimicry. "Movement" takes in essays on movements in plants, the sleep of plants, climbing plants, and carnivorous plants. Discussing so many subjects, the writer is, of course, obliged to content himself with the statement of very wide views; but his expositions are so clear and fresh that the book ought to be of considerable service to the readers to whom he specially appeals. It will give them at least a general conception of the nature and direction of some of the lines of modern research, and may induce them to seek elsewhere for fuller knowledge.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Panmixia.

I REGRET that I was led to doubt the sincerity of Mr. Romanes when he professed to have formed the conclusion that my words meant the reverse of their plain significance. I had not supposed that there was any one capable of making such a mistake.

I should be glad to terminate this discussion by a brief statement of the divergence of view between Mr. Romanes and myself as to the original matter in question, from which Mr. Romanes has led the correspondence by raising a variety of collateral issues. At the same time I should like to take the opportunity of saying what I believe Mr. Romanes would reciprocate, viz. that there is no ill-feeling but only a divergence of opinion between us.

Mr. Romanes definitely states that when an organ has become useless it will decrease in successive generations as a result of "cessation of selection" to about half its original size, without the co-operation of any such cause as economy of growth. He has repeated in effect this statement in his last letter. The result attributed by him to mere cessation of selection is, it must be noted (because he shows a tendency to waver and to substitute "degeneration" for "decrease in size"), a *decrease of size*: a mere failure in the exact adjustment of the parts of a complex organ is *not* the result in question. Of this I have a few words more to say below.

Mr. Romanes not only attributes the decrease in size of a useless organ to the cessation of selection pure and simple, but he calls that condition "a causal principle," and claims to have discovered it.[1] He has also stated that, whilst (to use his own words) "inherited decrease" of an organ *must* be due to this principle, it is "remarkably strange" that Mr. Darwin had overlooked it, and that it was unfortunate that he (Mr. Romanes) only gained the idea of this novel principle just after the appearance of the last edition of the "Origin of Species."

On the other hand, I consider that Mr. Romanes, by these contentions, obscures the theory of organic evolution, and that he presumes to censure Mr. Darwin without cause. There is nothing unfortunate in the date of Mr. Romanes's idea, because the idea is entirely erroneous; and it was no strange oversight of Mr. Darwin not to attribute the decrease of useless parts to "the principle of cessation of selection," or, in other words, to their uselessness alone—for the simple reason that he would have made a blunder had he done so. It is this blunder which Mr. Romanes places before us as his own contribution to the theory of panmixia: it is this blunder which Mr. Darwin not only did not make, but rendered *almost* impossible for others by his discussion of the matter ("Origin of Species," p. 401).

It is an incontrovertible mathematical fact that *the only effect of promiscuous breeding or panmixia* (considered apart from all other influences) *upon an organ or part which presents variations round an average mean will be to increase the number of individuals near the average mean, in proportion to the number of generations in which the panmixia is operative.* The notion that the haphazard interbreeding of "variations about a mean," must by itself lead to a shifting of the mean in the direction of diminished size—without the assistance of any special cause favouring reduction in size—is, to put it plainly, absurd.

It is, I believe, a mistake on the part of Mr. Romanes to say that Galton, Weismann, and Poulton agree with him in this astonishing fancy. But, were this the case, the mathematical fact would remain as it is.

Given a race of organisms in which a part has become useless, it is only (as Mr. Darwin pointed out) when some cause (such as economy of growth) favouring diminished size is operative, that the average mean of the size of the part will in successive generations shift in the direction of decrease. Mr. Darwin saw this, and explained it. Mr. Romanes not only failed to appreciate the considerations advanced by Darwin, but actually now charges him with oversight for not having made the blunder which he carefully avoided.

In conclusion, I have a few words to say in regard to the possibility of an organ consisting of several nicely adjusted parts losing that adjustment in a state of panmixia without the co-operation of economy of growth. Mr. Romanes erroneously declares that if we admit this we must also admit that decrease in size must similarly result. I am not surprised to find that he thinks so, and do not doubt his sincerity. But really the two cases present very different problems. Suppose the organ in question to be represented by fifty independent variables; then we have to consider not the probability of the average mean of each kind of variable being maintained but the probability of the production of the necessary *combinations* of fifty of them with the specific initial proportions of each of the fifty elements. Whether it is or is not probable that the complex adjustment and interaction of parts would be maintained in the absence of all interfering causes in a state of panmixia is a difficult question. It is one which is hardly worth further discussion, since it is impossible that the results of panmixia without such interfering causes should ever present themselves in organic nature.

It is, moreover, quite certain that any conclusion we may adopt in regard to that matter will not alter the mathematical fact that, given a numerous race and a long series of generations, the average mean round which the variations in size of a useless organ are distributed will not ultimately shift in the smallest degree either towards increase or decrease of size, as the result of the promiscuous interbreeding of the variations.

April 26. E. RAY LANKESTER.

[1] The erection of a negative condition—a cessation—into the position of a causal principle is an artifice which is very likely to obscure the view of the related facts. The "causal principle of non-existence" and "the reversal of being," would be worthy of the author of the artifice who professes also to have extracted an essence from an idea—the idea of promiscuous breeding, or panmixia!

The Inheritance of Acquired Characters.

IT surprises me to find that anyone who has looked into the evidence can doubt that acquired characters, as distinct from congenital ones, may, like congenital characters, become hereditary, and produce physiological effects. The instance mentioned in Herbert Spencer's letter in NATURE (vol. xli. p. 511), of domestic varieties of animals losing the power of erecting the ears, appears as nearly conclusive on the subject as such an instance can be.

On the habits or instincts of domesticated varieties, Darwin says:—"It may be doubted whether anyone would have thought of training a dog to point, had not some one dog naturally shown a tendency in this line. . . . When the first tendency to point was once displayed, methodical selection and the inherited effects of compulsory training in each successive generation would soon complete the work" ("Origin of Species," 4th edition, p. 256).

I quote another instance from Carpenter's "Comparative Physiology" (p. 987):—"Sir C. Lyell mentions that some Englishmen, engaged in conducting the operations of the Real del Monte Company in Mexico, carried out with them some greyhounds of the best breed to hunt the hares which abound in that country. It was found that the greyhounds could not support the fatigues of a long chase in this attenuated atmosphere, and before they could come up with their prey they lay down gasping for breath; but these same animals have produced whelps which have grown up, and are not in the least degree incommoded by the want of density in the air, but run down the hares with as much ease as do the fleetest of their race in this country."

Mr. Gulick's letter in NATURE (vol. xli. p. 536), insisting that the first and only absolutely essential factor in the

production of new varieties or species is the isolation of a portion of the race, appears very luminous. On this subject, let me again quote from Darwin:—

"Youatt gives an excellent illustration of the effects of a course of selection, which may be considered as unconsciously followed, in so far that the breeders could never have expected, nor even have wished, to produce the result which ensued—namely, the production of two distinct strains. The two flocks of Leicester sheep kept by Mr. Buckley and Mr. Burgess, as Mr. Youatt remarks, 'have been purely bred from the original stock of Mr. Bakewell for upwards of fifty years. There is not a suspicion existing in the mind of anyone at all acquainted with the subject that the owner of either of them has deviated in any one instance from the pure blood of Mr. Bakewell's flock, and yet the difference between the sheep possessed by these two gentlemen is so great that they have the appearance of being quite different varieties'" ("Origin of Species," 4th edition, pp. 37, 38). JOSEPH JOHN MURPHY.

Belfast, April 24.

THE fifth caudal vertebra of a tortoiseshell cat at the Sussex County Hospital is dislocated and attached at right angles to the long axis of the fourth. The sixth and last vertebra is also affixed at right angles to the fifth. The cat is able to wag the terminal phalanx of the tail, and the distortion has always been considered to be due to an accident when the animal was a kitten. Within the last week the cat has had a litter of several kittens, two of which were born almost tailless, one possessing (as far as I could ascertain by external manipulation) two caudal and the other three caudal vertebræ only. Whether the original distortion is due to accident or not, I think these facts may interest some readers of NATURE. W. AINSLIE HOLLIS.

Brighton, April 28.

P.S.—Since writing the above note I have had an opportunity of examining the two remaining kittens of the litter, and I find that only one of these has a normal tail. The other is docked of one or two of the terminal vertebræ, and the tail has a slight twist on itself towards the end. W. A. H.

April 30.

Variation in the Nesting-habits of Birds.

IN considering the interesting question of instinct, one naturally turns to the nesting-habits of birds as affording an apparently good instance of habit acquired and perpetuated so as to become fixed, and, as we say, instinctive. It would be interesting, however, to find exactly how far the art of nest-building is really inherited, and how much uniformity exists among the nests of birds of identical specific characters.

The "blackbird" of this region, *Scolecophagus cyanocephalus*, is rather noteworthy in this connection. Goss, in his "Birds of Kansas," says this bird breeds in trees and bushes, from three to thirty feet from the ground. In Colorado, as observed by Mr. Morrison and myself, it breeds sometimes on the ground, and sometimes in low trees or bushes. In Custer County, Colorado, I find it breeding on the ground, sometimes at the very edge of creeks, in places where arboreal nests might have been made, and also better concealed ones. Captain C. E. Bendire, who inclines to the opinion that this bird breeds diversely in all parts of its range, where opportunities offer, writes (*in litt.*):—"I have found them nesting abundantly both on the ground and in bushes in the same locality and close together in Oregon. One thing struck me as peculiar: the nests when placed on the ground were almost always to be found on the extreme edge of a creek bank, when they could have selected far more suitable places, better concealed ones at any rate, a few feet away from the bank." This selection of creek banks, noticed both in Colorado and in Oregon, is remarkable. It had occurred to me that in Colorado the habit might have been formed to lessen the risk of being trampled upon by the herds of buffalo which used to inhabit this region, but Captain Bendire tells me the habit is observed also in regions where there never were any buffalo, which throws doubt upon my explanation.

Captain Bendire, who has so excellent a knowledge of the nesting-habits of American birds, kindly gives me a few notes on the subject, which it may be permissible to quote.

"Birds in the selection of their nesting-sites will adapt themselves to circumstances, as is well known, but as in the case just mentioned [*Scolecophagus*] it is hard to arrive at an entirely satisfactory conclusion. It is, for instance, easy to account for, why the *Archibuteo ferrugineus* should breed on the ground in Dakota, in many cases at any rate, and why *Falco peregrinus anatum* in trees in Kansas, but there are a number of other such departures from the old established rules, which cannot be so easily accounted for" (C. E. Bendire, *in litt.*, January 21, 1890).

Captain Bendire also cites *Buteo swainsoni* and *Archibuteo ferrugineus* as birds which sometimes nest on the ground in places where there is plenty of suitable timber, which one might have expected them to make use of.

These variations in habit are certainly puzzling: probably the important factors in deciding the terrestrial or arboreal nesting-habits of a bird are four:—

(1) *Ability to build arboreal nests.*—If this varied in a locality where arboreal nests were not greatly preferable to terrestrial ones, we can see how a minority of clever birds might build in trees, and a majority of duffers on the ground. The slight disadvantage to the ground-builders might be counterbalanced by their numbers.

(2) *Danger of falling.*—In regions where the trees are not suitable for holding nests, or where very high winds prevail, a terrestrial nest might be preferable; though the same species in another part of its range might do well to build arboreally.

(3) *Dangers of nesting on the ground.*—Such dangers would arise from terrestrial enemies, floods, &c., and would vary greatly no doubt in different regions. Where things were otherwise fairly balanced, a slight difference in this respect might decide the nesting of a bird.

(4) *Means of defence.*—Some birds, with special means of defence or of escaping observation, might build on the ground where others would take to trees. T. D. A. COCKERELL.

West Cliff, Custer Co., Colorado.

Russian Transliteration.

I AM afraid the authors of the "new system" of transliteration have misunderstood my letter in yours of April 10 (p. 534), advocating "the tabulation of the system of transliteration which has been so long in use in this country" in preference to the adoption of the unnecessary novelties they propose to introduce. By the "system in use" I meant that for transliteration from Russian into *English*, and certainly did not include the transliterations from Russian into *German* which have been copied from books or memoirs in that language into English catalogues or journals. As practically all the examples the authors adduce in defence of their "new system," including both the atlases and the works with which they associate my name, are of this kind—*i.e.* merely copies of transliterations from Russian into *German*—I fail to see what bearing they have on the question of transliteration into *English*, however useful they might be in constructing a system for transliteration from Russian into German.

Another misapprehension is, they seem to imagine that I have propounded a system of transliteration of my own. I sincerely hope I shall never be guilty of doing anything so rash. I merely offered some friendly criticisms on the new system which the authors had devised, and I may supplement my remarks by here giving in tabular form the principal points in which this system differs from that which I conceive to be the English use:—

	English Use.	New System.
в	*v*	*v*
въ	*ff*	*v*
г	*h* before *e* or *i*, otherwise *g*	*gh*
ж	*j*	*zh*
кс	*x*	*ks*
у	*ou*	*u*
х	*ch*	*kh*
ч	*tch*	*ch*
щ	*shtch*	*shch*
ѣ	*é*	*ye*
ій	*y*	*iĭ*
ю	*u*	*yu*

I have already given a few examples of names which look uncouth when transliterated according to the new system, and I here add one more. It is

SKRZHIPSKIĬ.

When I wrote it down and observed its hieroglyphic appearance, there arose somehow in my mind a vision of a new system of chemical nomenclature devised many years ago by Laurent,

and his proposal to give to "alum" the name *atolan-telmin-ajafin-weso*.

Charles E. Groves.

Chemical Society, April 14.

P.S.—I need scarcely say how cordially I concur with Mr. W. F. Kirby's exceedingly apposite remark that no system of transliteration should be adopted offhand without full discussion.

With reference to the scheme of Russian transliteration propounded on p. 397 of Nature (vol. xli.), I should be obliged if the editor of Nature would allow me the opportunity of suggesting that different principles of respelling foreign languages in English might possibly be adopted with advantage for different purposes. The scheme referred to is one of strict transliteration; in other words, the aim is to represent the letters of a foreign alphabet uniformly by the same letters or combinations of letters in the English alphabet. For the purpose of drawing up lists of titles of books and papers in a foreign language—the purpose obviously kept in view by the propounders of the new Russian scheme—this principle is no doubt the best. It is the only one that makes it easy to consult a Russian dictionary. But it does not follow that the principle of strict transliteration is the best to adopt for foreign proper names occurring in a language different from that to which they belong. The third of the rules adopted by the Council of the Royal Geographical Society for geographical orthography is as follows: "The true sound of the word as locally pronounced will be taken as the basis of the spelling" (Proc. Roy. Geog. Soc., 1885, p. 535). This rule is inconsistent with any scheme of strict transliteration. I can imagine that two views may be held as to its propriety. Unquestionably there are difficulties in applying it, but surely for the purpose for which the rule was adopted it is at least defensible and worthy of serious discussion.

Even if it should be recognized, however, that it is desirable that one principle of conversion into a foreign alphabet should be adopted for one purpose, another for another, it will, I think, be generally admitted to be a matter of the greatest importance that an agreement should be come to among all concerned in such conversions as to those points which might be held in common on either system of conversion. All schemes of transliteration in the strict sense of the term are based on phonetic rules. The aim in all is to render the letters of one alphabet by the letters and signs most appropriately representing their normal sounds in another. It is the departures from the normal sounds that are disregarded. Now a uniform system of representing sounds, so far as it is at all desirable to represent foreign sounds in English, if devised with sufficiently wide regard to the requirements of different languages, would be of great use as a system to be followed for every word or name on the principle of phonetic respelling and to be adopted as the basis of every scheme of transliteration.

Geo. G. Chisholm.

April 22.

On some Decomposed Flints from Southbourne-on-Sea.

The curiously decomposed flint-pebbles which occur in the cliffs between Boscombe and Southbourne-on-Sea have not, so far as I have been able to ascertain, yet received the attention they deserve, and, with a view of obtaining other opinions before the completion of a paper on the subject, I venture briefly to offer mine.

I will not now deal generally with all the pebbles in the horizon alluded to, but specifically with some of unusual interest which occur at a certain point in the cliff, as these represent an extreme type of decomposition to which most of the less-altered pebbles may be found gradating. These type-pebbles occur in the cliff a short distance to the east of the pier at Southbourne-on-Sea, and present all the characteristic features of a littoral deposit.

A section of the cliff at this point shows:—

Blown sand	8 feet.
Brown loam, passing down into lighter-coloured sandy gravel containing angular and sub-angular yellow and brown flints without any definite mode of deposition	14 feet.

At the base of this, and resting on pure quartzose sand, free from flints, is a definite and more or less horizontal layer of rounded and decomposed flint-pebbles of about one pebble in thickness, partially embedded in the white sand on which they rest, and covered by the clastic matter of the bed above.

While some of these pebbles are apparently unaffected, most of them are eroded in a remarkable manner, having large portions of their substance removed; and others, though retaining their original form, are completely changed throughout their mass into a soft, white substance (crystalline silica) macroscopically like chalk, and as easily cut or sawn through. The largest wholly-decomposed specimen I have been able to procure measures 14 inches around its greatest circumference.

It is remarkable that these flint-wrecks preserve their original form and detail to such a degree of perfection that in most cases the soft surfaces retain the crescentic markings (mastoid) of incipient conchoidal fracture which resulted originally from the percussion due to wave-action.

As far as I am at present able to judge, the silica originally composing these pebbles was of two distinct kinds—a bluish-black, or more stable form (superior crystalline development), and a light-coloured, or less stable form (inferior crystalline development); for, in the specimens I have procured, the bluish-black variety does not appear to be abnormally affected, while the lighter-coloured variety is nearly always partially or completely decomposed. The wholly-decomposed pebbles would, therefore, have been formed of the unstable variety, while those eroded only would have been formed of a combination of the two, the stable portion now remaining.

My supposition seems to be strengthened by the evidence obtained from the banded flints, which are very plentiful here. These banded flints are formed of alternating zones of the two varieties, and in many cases the unstable form has been so decomposed as to leave only successive zones of the more stable form fitting loosely one into the other like a nest of boxes, and as easily separable. Notwithstanding this fact these unstable zones —before decomposition—are apparently as well able to withstand *mechanical* erosion as the stable zones, a conclusion arrived at through having some of these banded flints subjected to the action of the sand-blast for 15 minutes without any "ridging" taking place.

That the decomposition of these particular flint-pebbles must have taken place prior to the deposition of the superincumbent bed of clastic material is proved, I think, by the fact that none of the flints composing this bed appear to be decomposed, even the smallest chips being comparatively unaffected.

From this and other facts observed, I gather that the decomposition of these pebbles must have taken place when they were exposed to the air, but I do not think atmospheric influences alone would be sufficient to account for the evident rapidity and effectiveness of the process; we must seek a special cause for an unusual effect.

I venture to suggest that the solvent which has in this case removed the colloidal silica was derived from decaying sea-weed, and other organic matter, cast up from time to time by the waves upon this (then) pebbly beach. Large masses of sea-weed cast up by storm-waves take a considerable time to decompose, and during such period is it not possible that they might produce alkaline solutions, or—as has been suggested to me by Dr. Irving—combinations of ammonia and organic acids? Either of these is a well-known solvent of colloidal silica. The action of such solvents might have been accelerated by the mechanical process through which most of these pebbles passed prior to their final state of rest, viz. the action of the sea-waves in producing the mastoid structure already alluded to, this molecular disruption no doubt facilitating the penetration of the solvent to the very heart of the pebble. It is worthy of note, too, that in some of the eroded specimens procured, the remaining unaffected parts are almost entirely free from these incipient fractures, a fact which—if we ignore the supposed variation in the stability of the silica—suggests the necessity for a combination of the chemical and mechanical causes to produce the effects observed.

I have dealt here with a special case only, in the hope that my suggestions may be found applicable to the many in which we see abnormal decomposition occurring in the flint-pebbles of littoral deposits, and which appears to be distinct from the "weathering" so frequently seen occurring to considerable depths in the exposed flints of deposits other than littoral.

Bournemouth, April 16.

Cecil Carus-Wilson.

Doppler's Principle.

As a student I should be much obliged to any reader for an explanation of the following difficulty. In considering Doppler's

principle as applied in acoustics, we find four cases: (1) approach of observer, source and medium being at rest; (2) recession of observer, source and medium at rest; (3) approach of source, observer and medium at rest; (4) recession of source, observer and medium at rest.

I have consulted all the standard authorities which have occurred to me, and find they all agree in the 1st and 2nd cases. In (3), Doppler, Lord Rayleigh, Prof. Everett (1st method in "Deschanel"), Jamin, and Ganot have the same result as in (1). Lord Rayleigh in his "Theory of Sound," vol. ii. p. 142, says, "In the case of a periodic disturbance a velocity of approach v is equivalent to an increase of frequency in the ratio $a : a + v$," a being the velocity of sound. In another place the same author says that it is the *relative* velocity of source and observer alone that is important. The above-mentioned authorities appear to hold the same views.

But Prof. Everett has a more rigorous demonstration than the above, which leads to the result—old pitch : new pitch :: $a - v : a$. This result is the same as that given by Mach, "Ton u. Färberänderung durch Bewegung" (1874), and as that used by Balfour Stewart, "Treatise on Heat."

In the 4th case the first-mentioned authors again agree, giving as a result—new pitch : old pitch :: $a : a - v$. Prof. Everett's and E. Mach's results agree in giving $a + v : a$ as the ratio.

It will be readily admitted that the first two cases are simpler problems to attack than the last two. The results of the minority for the cases (3) and (4) seem to me to come from looking at the change in wave-length first, those of the majority from taking into account the number of waves met by the observer. In any case the disagreement among such authorities is naturally beyond me to explain. The motion of the medium does not appear to offer any special difficulty.

G. H. WYATT.

The Relative Prevalence of North-east and South-west Winds.

THE direction of the wind has been noted twice daily at this Observatory (9 a.m. and 9 p.m.) during the past 6 years, with the following mean results:—

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
56	48	30	25	23	65	45	60	13

The period under consideration is not sufficiently long to make the series of observations of any great value, but as Mr. Ellis asks for comparisons, I am happy to give them for what they are worth.

C. E. PEEK.

Rousdon Observatory, Lyme Regis, April 26.

The London Mathematical Society's List of Papers.

IN NATURE (vol. xli. p. 594) it is stated that "a complete index of the papers printed in the Proceedings of the London Mathematical Society has been issued." It will be in the recollection of some that a previous issue of the Index to the papers contained in the first 17 volumes was announced in NATURE (vol. xxxvi. p. 42): it is a re-issue of this list completed for the first 20 volumes that is now noted. The former edition of 3000 copies was soon dispersed, and resulted in warm expressions of thanks from mathematicians, and also in an increased sale of the Proceedings. If other Societies would, in like manner, issue lists of the titles of papers printed in their Proceedings, they would no doubt meet with a like reward. All mathematicians, and others who are interested in mathematical research, can have a copy on application to the Secretaries (22 Albemarle Street, W.), or to the publisher (Francis Hodgson, 89 Farringdon Street, E.C.).

April 26. R. TUCKER, Hon. Sec.

THE UNITED STATES SCIENTIFIC EXPEDITION TO WEST AFRICA, 1889.

AS the work of the Expedition approaches conclusion, I venture to hope that a brief partial recital of results may be worth notice in NATURE, particularly as, in many of the ports we have visited, English courtesy and English hospitality have contributed in large measure to the facilities for prosecuting our work, not to say also very greatly to the delight of doing it.

I find it a trifle difficult to say just where to begin, but Dr. David Gill, H.M. Astronomer at the Cape, comes first to mind, and surely no one could have devoted himself more unsparingly to the interests of the Expedition than he did during our stay of a fortnight and more at Cape Town: and through his liberal provision for every requirement of the observers, it became possible to swing the pendulums in the Royal Observatory buildings, the same spot occupied in previous gravity-research at the Cape. Had it been expedient to delay the *Pensacola* longer, Dr. Gill's suggestion would gladly have been acted upon, and an additional gravity-determination made at the Kimberley diamond fields, 650 miles in the interior, at an elevation of about 4000 feet; but there was time only for members of the Expedition not engaged in exact measures to proceed as far inland as that; and the movements and operations of the naturalists and others who desired to visit the Cape Colony country as far as Kimberley became feasible through the kind offers of Mr. Difford, the Secretary of the Colonial Government Railways.

Not only at Cape Town had we much occasion to thank His Excellency Sir H. B. Lock, the Governor of the Colony, but two months later, at Ascension Island, through his courteous intervention, and the obliging civilities of Admiral Wells, R.N., all possible preparation had been made; while, on our arrival, Captain Napier, R.N., in charge of Ascension, most thoughtfully smoothed the way by arranging to our entire satisfaction all matters which could in any way facilitate the work we had planned for that interesting island.

Nor am I forgetting the multitude of courtesies at the hands of Governor Antrobus of St. Helena, where all desired assistance was afforded, and where work similar to that at Ascension was undertaken and completed.

In this connection, I must not omit mention of the American Navy, for neither the Expedition in its present form nor its work could have become an accomplished fact but for the enlightened policy of Secretary Tracy, who assigned a man-of-war for its transport to Africa and home again; of Admiral Walker, and later, Commodore Dewey, Chiefs of Naval Bureaux, who devoted their energies ungrudgingly to the regulation of all matters official affecting the welfare of the Expedition; and of Captain Yates, the commander of the U.S.S. *Pensacola*, who has done everything in his power to forward the prosecution of the scientific work.

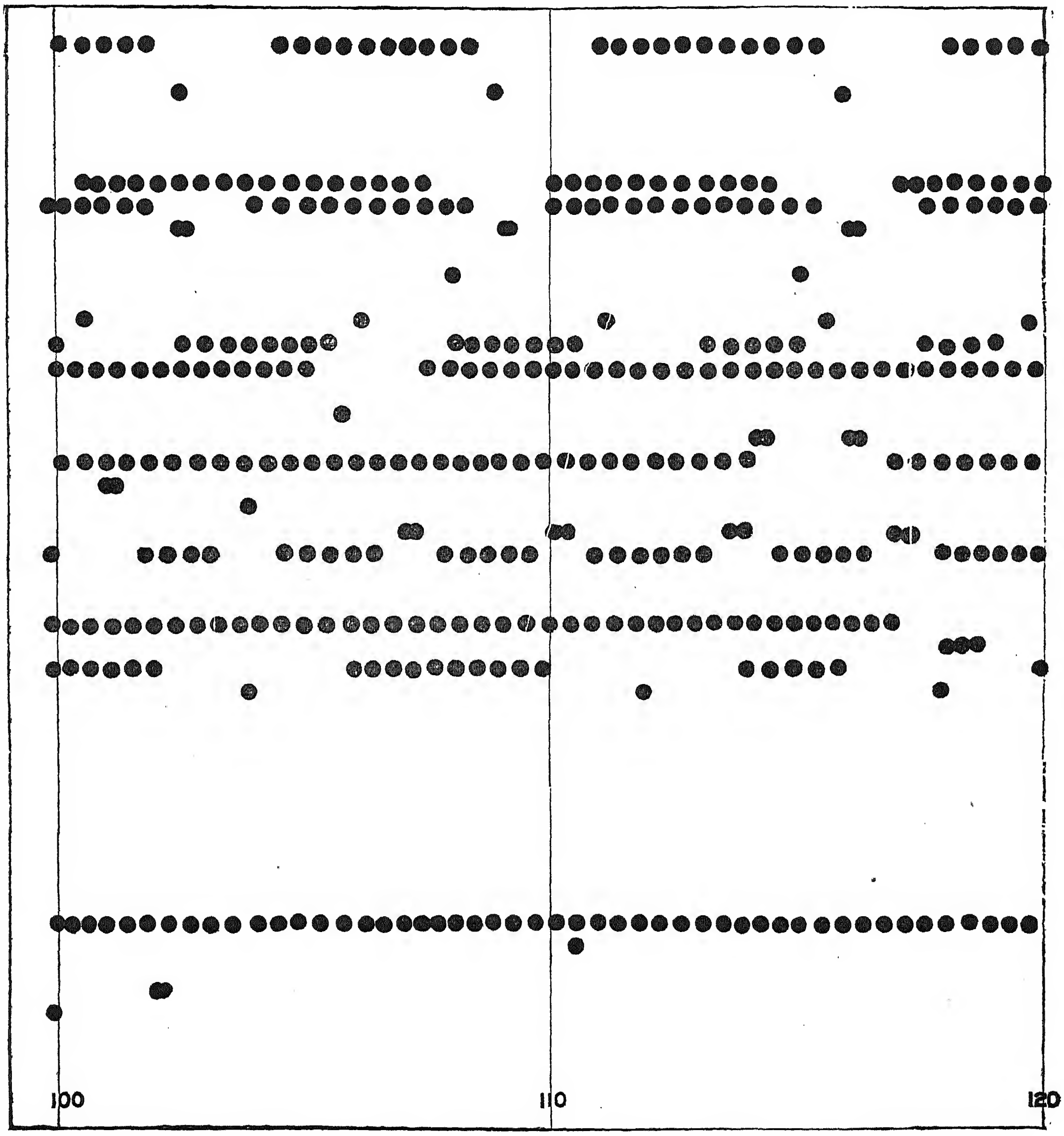
The *Pensacola* left New York on October 16 last; called at the ports of Horta, Fayal, Azores, November 2–3; San Vicenti, Cape Verdes, November 10–12; St. George's Parish, Sierra Leone, November 18–20; Elmina, Gold Coast, November 26–28; São Paolo di Loanda, December 6–7; Eclipse Bay, Cape Ledo, December 8–27; again at Loanda, December 28–January 6; Cape Town, January 17–February 6; St. Helena, February 20–March 10; and arrived at Ascension six days later, which port she will probably leave about April 10.

Now to some of the results.

At all these stations except Cape Ledo, the magnetic elements have been carefully investigated by Mr. Preston, of the U.S. Coast and Geodetic Survey. Also he had an additional magnetic station at Cabiri, about 45 miles interior from Loanda, whither he went for the immediate eclipse period.

The short time available before the eclipse made it impracticable to begin the gravity-determinations until Loanda; there Mr. Preston swung the Peirce pendulums, and again at the Royal Observatory, Cape Town. At St. Helena two complete swings were obtained, the one at a sea-level station near the Castle, Jamestown, and the other at Longwood, elevation 1750 feet. It was not

thought practicable to re-occupy Foster's station at Lemon Valley. Here at Ascension the sea-level station at Garrison is already complete; and, as I write, Mr. Preston and Prof. Bigelow are taking quarters near the summit of Green Mountain for the second station, near the spot occupied by Foster sixty years ago, elevation between two and three thousand feet. Auxiliary magnetic work is undertaken at both these upper-level island stations. Between Ascension and New York but one prolonged stop is at present contemplated—at Bridgetown, Barbados—where magnetics will be done, and gravity-work, if practicable. Also, Bermudas may be included, but that is perhaps unlikely. In addition to the bearing of this work on terrestrial physics and geologic theories, it is worthy of note, in passing, that all these stations, including Washington, where swings are made both before the departure of the Expedition and after its return, lie within a narrow great-circle belt, which can at



Control-sheet of the Pneumatic Commutator between the 100th and 120th seconds of Totality.

any time be continued on through the United States and Canadas and Alaska, forming an extraordinary stretch of gravimetric survey.

Regarding the eclipse and the stay of the astronomers at Cape Ledo, it has to be said, to our great regret, that the direct photo-heliograph of 40 feet focal length was the only instrument with which eclipse-records could be secured. These were photographs of the partial phases, over 100 in number, obtained between clouds. The instrument was built under the immediate supervision of Prof. Bigelow, and has, among other peculiarities, a skeleton tripod-mounting which will be fully figured in the definitive report of the Expedition. It has been proven practicable to dispense with the heliostat mirror, always the weak point in the horizontal photo-heliograph; and to manipulate readily a camera long enough to produce a $4\frac{1}{2}$-inch solar image direct: and this, too, by means of a mounting easy to transport and to set in position. The photographs were taken in groups of ten, on circular plates of 22 inches diameter. The apparatus auxiliary

to these rotating plates made the whole automatic, the driving power being compressed air under electric control. A form of sand-clock was found most efficient for counteracting the diurnal motion.

For the total phase our preparations were even more elaborate. What I attempted was nothing short of the complete automatic operation of all the photographic instruments, whether photometers, spectroscopes, cameras, or polariscopes. Over a score of these instruments were securely adjusted upon an immense polar axis, split, and mounted on the English plan. Powerful clock-work with a Repsold governor carried the whole with great accuracy. All such mechanical movements were specially invented and constructed as were necessary to work the exposing-shutters, to change exposed plates for fresh ones, and to perform all other operations, as rotating Nicols, varying apertures of objectives, trailing plates, and the like. Each movement, of whatever sort, took place as a result of the thrust of a small, collapsible, pneumatic bellows. The precise instants of collapse of these bellows were controlled through the intervention of the Gally pneumatic valve, a most ingenious device whereby any required number of very small air-currents (exhaust) are made to control the motion of an equal number of large air-currents (also exhaust). This principle has been very successfully employed in the automatic playing of musical instruments; and anyone familiar with the modern forms of these, in which a perforated paper sheet takes the place of the music, will readily comprehend how the whole thing was done. In the pneumatic commutator actually used at the African station forty-eight half-inch currents were under absolute control of a small paper sheet, about nine inches wide, suitably perforated, and unwinding at an invariable rate from a chronograph barrel. Thence it passed over the series of minute apertures through which flowed the lesser exhaust-currents, each of which controlled the action of its own valve, and consequently of its appropriate large exhaust-current, through suitable pipes leading to the individual pneumatic bellows. A portion of the commutator-sheet is represented in the illustration.

I do not need to specify here the detail of astronomical apparatus which this pneumatic commutator operated; but in the collection of totality-instruments were two 8-inch silver-on-glass mirrors, four spectroscopes, and a variety of objectives for a variety of purposes, ranging all the way from a ¾-inch aperture in one of the polariscopes to the Harvard 8-inch doublet of 13 feet focal length. The whole number of plates, or separate exposures, was in excess of 300, totality being 190 seconds in duration; and when once started, the whole affair looked out for itself absolutely, so long as the necessary power was supplied at the main exhaust-bellows.

But totality was completely clouded under; and instead of a fine accumulation of photographic data, I have only the gratification of having shown it to be practicable in the future for one eclipse observer to operate an indefinite amount of photographic apparatus quite as readily as, and with greater certainty than, he would have attended to only two or three cameras by hand heretofore. In converging all this apparatus toward readiness for eclipse-day, I had of course much valued assistance, which will be fully acknowledged elsewhere; and I need only mention here Prof. Bigelow, Mr. Davis, and Mr. Van Guysling, who were specially helpful in devising required movements and practically constructing them.

The totality-area in West Africa appears to have been unusually overcast. Auxiliary observers at Cabiri had clouds; at Cunga, clouds; at Dondo, clouds; while at Cazengo, Oeiras, Muxima, Kakulu, and Bom-Jesus it was cloudy too. Also, about 15 miles out at sea, in the path of central eclipse, whither the *Pensacola* had gone in the hope of additional results, the sky-conditions were perhaps slightly better, but still so bad that it is doubtful whether the true corona was seen at all.

Lest I weary anyone who may be reading this with too long a statement of our work, I omit here all account of the natural history of the Expedition, only saying for the present that Messrs. Brown, sent out by the U.S. National Museum, have been actively engaged in collecting at all the ports made by the *Pensacola*, and their materials will, I dare say, be competently discussed on the return of the Expedition. More about this later. At Ascension, opportunity for trawling is now for the first time available, and so far with fair success. While the main eclipse party was established at Cape Ledo, naturalists and anthropologists were in the interior about three weeks, at Cunga and at Dondo, His Excellency the Governor of Loanda, and the Directors of the Caminho de Ferro Trans-Africano, having courteously afforded them every facility for the prosecution of their work there. Physical measurements were taken among the Umbundus, Cabindas, Bailundas, Quissamas, and others; collections of folk-lore, fetishes, and mind-products made; and general information gathered concerning a variety of subjects indicated in the manual of the Anthropological Society of Great Britain. On reaching the Cape, both naturalist and anthropologist found the outlook so promising that they applied for discharge from the Expedition there, in order to continue their work in the Cape peninsula. The opportunities were indeed rare: a great exploring Expedition was about organizing, under the auspices of the English Syndicate, to which the King of the Matabele has granted unusual privileges and concessions, in a region for the most part untravelled by white men, and represented as very rich in material for natural history and other research. The Expedition is particularly indebted to the Rev. G. H. R. Fisk, of Cape Town, for a very valuable collection of tortoises, embracing *Testudo pardalis*, *T. angulata*, *T. trimeni*, and *T. tentoria*; *Homopus areolatus*, *H. femoralis*, and *H. signatus*, the representatives of these latter forming a perfect series of the South African *Homopus*.

The progress of M. Heli Chatelain's researches in the West African tongues is gratifying, and bids fair to constitute a valuable section of the work of the Expedition. He remains in Angola for some months yet, to gather linguistic material for various works he has in hand—among them his "Grundzüge des Ki-mbundu," in which the elements of this language are compared with those of Kixi-kongo, Luba, Lunda, N-mbundu, Oshi-ndonga, and Otyi-herero. The results will enable one to form an idea of the mutual relations of the languages of Central West Africa.

I may say here that Prof. Bigelow, in addition to assisting in the pendulum-work at St. Helena and Ascension, has been diligently engaged upon theoretic researches on the corona and terrestrial magnetism, the beginnings of which are outlined in his paper already published by the Smithsonian Institution. As yet he inclines to speak of this work with much reserve; but if the key to the solution of these complex phenomena has not actually been found, it surely looks strongly that way. By Dr. Gill's kindness, the resources of the excellent library of the Cape Observatory were freely and gladly drawn upon in this work.

Of the Bulletins, or preliminary publications of the Expedition, thirteen are already issued—one each relating to general matters, to terrestrial physics, to philology, and to localities of scientific interest in St. Helena; two to meteorology and to natural history; and five to the total eclipse.

I reserve for another occasion all account of the important researches which Prof. Cleveland Abbe, Meteorologist of the Expedition, has been sedulously prosecuting for the past five months and over, with improved means, and under rare conditions at sea and on land.

David P. Todd.

U.S.S. *Pensacola*, Ascension, March 27.

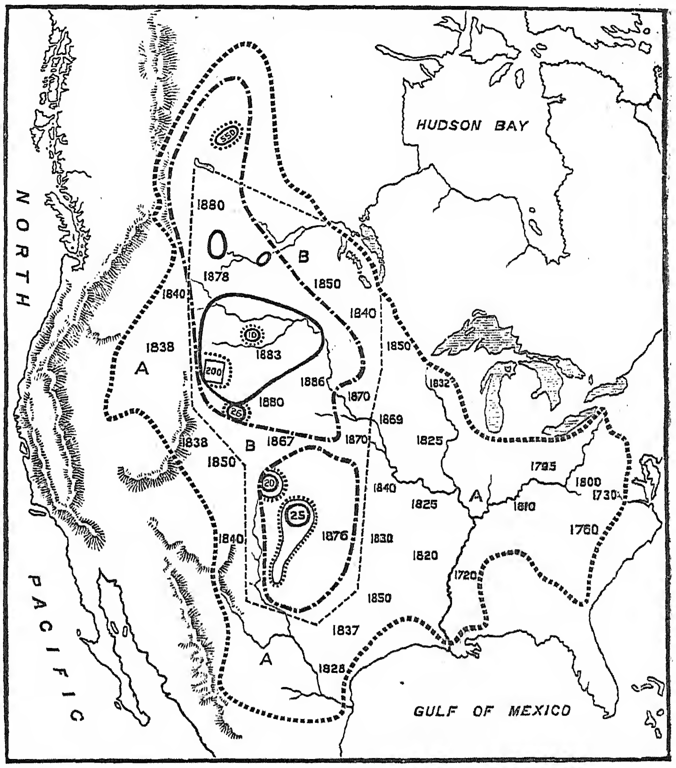
THE EXTERMINATION OF THE AMERICAN BISON.[1]

IN the whole course of the history of man's relations with the lower animals, no sadder chapter will ever be written than that which tells of the practical extinction of the bison, which, only a short twenty years since, wandered in countless thousands over the vast prairies of the northern half of the American continent. This mournful story—mournful alike to the naturalist, to the sportsman, and to the trader—the author of this memoir recounts in such a full and lucid manner as to have practically exhausted the subject. Indeed, this memoir, in conjunction with Mr. J. A. Allen's monograph of the recent and extinct American bisons, does all that can be done in the way of literature to atone for the loss of the animal itself as a feature of the North American continent.

The memoir before us—which, we should say, is issued as a separate volume—is divided into three parts. The first of these deals with the life-history of the bison, the second with its extermination, while the third gives the history of the Expedition despatched by the Smithsonian Institution, in 1886, to procure specimens for the National Museum before it became too late. Of this Expedition the author was a prominent member, and the results of his labours are now exhibited in the magnificent case of stuffed specimens set up by his own hands in the National Museum at Washington. An excellent illustration of this group is given in the frontispiece to the volume.

After briefly alluding to the earliest records of a knowledge of the existence of the American bison by Europeans, Mr. Hornaday proceeds to notice its geographical distribution. In illustration of this important part of the subject a map is given, showing not only the original distributional area, but also the division by the Union Pacific Railway into the great northern and southern herds, and the gradual contraction and isolation of their areas, finally ending in the few spots where scattered individuals still linger on. For the benefit of our readers we give a reduced reproduction of that portion of this map comprising the bison area. Our author states that the bison originally ranged over about one-third of the entire North American continent. Thus, "Starting almost at tide-water on the Atlantic coast, it extended westward through a vast tract of dense forest, across the Alleghany Mountain system to the prairies along the Mississippi, and southward to the delta of that great system. Although the great plain country of the West was the natural home of the species, where it flourished most abundantly, it also wandered south across Texas to the burning plains of North-Eastern Mexico, westward across the Rocky Mountains into New Mexico, Utah, and Idaho, and northward across a vast treeless waste to the bleak and inhospitable shores of the Great Slave Lake itself."

About a century and a half ago, when the greater part of North America was still an unknown region to the white races, it would appear that the bison had about attained its maximum development; and the author suggests that if it had been left undisturbed it would probably have crossed the Sierra Nevada and the Coast Range to reach the fertile plains of the Pacific slope. This enormous range would also in course of time have probably given rise to local races, of which there is an actual example in the so-called "wood-" or "mountain-buffalo"; and in the opinion of the author it is probable, if things had been left to themselves, that, while the bisons in the neighbourhood of the Great Slave Lake would have developed an extra amount of hair, and thus tended to resemble the musk-ox of the Arctic regions, those in the warm regions of the south would tend to lose their hair, and attain a condition resembling that of the Cape buffalo and the Indian gaur. The appearance of the white man on the scene soon, however, put a stop to Nature's processes.



Boundary of the area once inhabited by the bison.

Approximate boundary between the area of desultory extirpation (A) and that of systematic destruction for robes and hides (B).

Range of the two great herds in 1870.

Range of the herds in 1880.

Range of the scattered survivors of the southern herd in 1875, after the great slaughter of 1870–73.

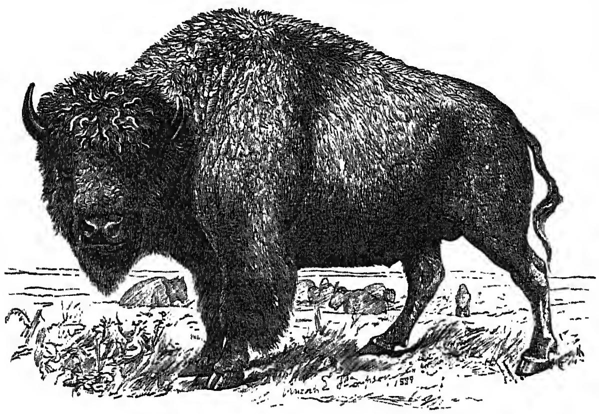
Range of the northern herd in 1884, after the great slaughter of 1880–83.

The third section of the first part is devoted to the consideration of the former numerical abundance of the bison. Here the author considers that the current accounts of the extraordinary number of these animals are not in the least exaggerated. Thus he observes that "it would have been as easy to count or to estimate the number of leaves in a forest as to calculate the number of buffaloes [the author frequently employs this American misnomer for the bison] living at any given time during the history of the species previous to 1870. Even in South Central Africa, which has been exceedingly prolific in great herds of game, it is probable that all its quadrupeds taken together on an equal area would never have

[1] "The Extermination of the American Bison." By W. T. Hornday. From the Report of the U.S. National Museum for 1886–87. Pp. 369–548, Pls. i.–xxii. (Washington: Government Printing Office, 1889.)

more than equalled the total number of buffalo in this country forty years ago." As an instance of these enormous numbers, it appears that, in the early part of the year 1871, Colonel R. I. Dodge, when passing through the great herd on the Arkansas, and reckoning that there were some fifteen or twenty individuals to the acre, states from his own observations that it was not less than 25 miles wide and 50 miles deep. This, however, was the last of the great herds; and Mr. Hornaday estimates that the number of individuals comprising it could not be reckoned as less than four millions. Many writers at and about the date mentioned speak of the plains being absolutely black with bison as far as the eye could reach; and Mr. W. Blackmore tells of passing through a herd for a distance of upwards of 120 miles right on end, in travelling on the Kansas Pacific Railroad. Frequently, indeed, trains on that line were derailed in attempting to pass through herds of bison, until the drivers learned that it was advisable to bring their engines to a standstill when they found the line blocked in this manner. Plate III. gives a graphic illustration of a train halted as it reaches the border of a herd of bison.

In the fourth section of the part under notice, we have a full description of the general characters of the American bison, and the points by which it is distinguished from its European congener, the Lithuanian aurochs. In this connection we reproduce,



Bull Bison in the National Museum at Washington.

on a smaller scale, the author's figure of the bull bison mounted in the United States National Museum, since he tells us that many of the figures to be met with do not give by any means a fair idea of the grand proportions of the animal, being taken either from domesticated or from badly-mounted specimens. The height of this bull is upwards of 5 feet 8 inches at the withers. The author remarks, however, that the specimens obtained by the Smithsonian Expedition were above the average height, since they were the fleetest and strongest examples of the race, which had escaped from the slaughter of the great herds by their endurance and speed. It is also remarked that these bison were of extreme muscular development, and showed no traces of the large amount of fat so characteristic of the members of the great herds when they were comparatively undisturbed upon the open plains.

The following sections treat of the habits, food, and disposition of the bison—subjects into which we need not enter on this occasion. In the eighth section we have a full discussion as to the economic value of the bison, in the course of which it is shown what a severe financial loss the States have sustained in permitting its extermination. Some very interesting observations then follow as to the number of herds or individuals of bison—either pure or half-bred—now existing in captivity in various parts of the States, and in other countries. From this it appears that on January 1, 1889, there were 256 pure-bred specimens known to be kept in captivity; while the herd of wild ones, protected by the United States Government in the Yellowstone National Park, numbered about 200.

With the second and most interesting part we come to the proper subject of the memoir—the actual extermination of the bison. The primary cause which has led to this sad result is, of course, the spread of civilization—and more especially railways—over the area formerly sacred to the bison and a few Indians. But as secondary causes the author mentions the utterly wanton and reckless way in which the unfortunate animals were shot down for the sake merely of their hides or tongues; the want of protective legislation on the part of the Government; the preference for the flesh and skin of cows; the marvellous stupidity and indifference to man of the animals themselves; and the perfection of modern firearms.

Among the methods of slaughter the so-called "still-hunt," where the hunter creeps up to a herd and shoots one after another of its members, appears to be one of the most deadly, owing to the crass stupidity of the animals themselves. The plan adopted was first to shoot the leader, when the remainder would come and stupidly smell round the body, till another animal assumed the post of leader, and was shot down when it was about to make a move; the same process being repeated almost without end. Riding down, surrounding, impounding, or hunting in snow-shoes, were, however, other equally effective methods of destruction.

It is stated that, in spite of the merciless war which had been in a desultory manner incessantly waged against the bison, both by whites and Indians, for over a century, and the consequent gradual restriction of its area, it is certain that there were several million head alive as late as 1870. The period of desultory destruction may be roughly reckoned as extending from 1730 to 1830. During that time the bison had been completely driven away from the Eastern United States, and also from the districts lying to the west of the Rockies (where it had never been very numerous); and the area had thus become practically restricted to that inclosed by the broken line on the map.

From 1830 to 1888 is reckoned as the period of organized and systematic slaughter for the sake of the skin and flesh; and the author does not measure the terms he employs with reference to the supineness of the Government during this period. He gives a detailed account of the various expeditions which were steadily playing upon the great herd occupying the area indicated on the map; and the gradually increasing demand for "buffalo-robes." The real beginning of the end was, however, the completion in 1869 of the Union Pacific Railway, which completely cut the bison area in twain, and divided the great herd into a southern and a northern moiety.

The history of the southern herd is very short. Its central point was somewhere about the site of the present Garden City in Kansas; and although its area was much less than that occupied by the northern herd, it probably contained twice as many animals, the estimated number of individuals in 1871 being not less than three millions, and probably nearer four. The completion of the Kansas branch of the Union Pacific in 1871, which ran right through the head-quarters of the southern herd, was the immediate cause of its destruction; and we are told that the chief slaughter, which began in 1871, attained its height in 1873. So wanton and wasteful, indeed, was the destruction during this period that it is said that every single hide sent to market represented four individuals slain; and the description given by the author on p. 496 of the condition of the country owing to this frightful slaughter is almost sickening. The author observes that "it is making a safe estimate to say that probably no

fewer than 50,000 buffaloes have been killed for their tongues alone, and the most of these are undoubtedly chargeable against white men, who ought to have known better." Over three and a half million individuals are estimated to have been slaughtered in the southern herd between 1872 and 1874. In the latter year the hunters became alarmed at the great diminution in the number of the bison, and by the end of 1875 the great southern herd had ceased to exist as a body. The main body of the survivors, some 10,000 strong, fled into the wilder parts of Texas, where they have been gradually shot down, till a few years ago some two or three score remained as the sole survivors of the three or four millions of the great southern herd. Bison-hunting as a business definitely ceased in the south-west in 1880.

Almost equally brief, and equally decisive, is the history of the great northern herd. The estimated number in this herd in 1870 is roughly put at a million and a half, ranging over a much wider area than the southern herd. The portions of the herd in British North America appear to have been exterminated first. Previously to 1880, the Sioux Indians had made an enormous impression on the numbers of this herd in the States of Dakota and Wyoming; but the beginning of the final destruction of the herd may be said to date from that year, which was signalized by the opening of the Northern Pacific Railway, running right through their area. In that year the herd was hemmed in on three sides by Indians armed with breechloaders, who enormously reduced its numbers. A rising market for "buffalo-robes," in 1881, stimulated a rush on this herd, till "the hunting-season which began in October 1882 and ended in February 1883 finished the annihilation of the great northern herd, and left but a few small bands of stragglers, numbering only a very few thousand individuals all told." It was long thought that a large section of the herd was still surviving, and had escaped into British territory, but this proved to be a mistake.

"South of the Northern Pacific Railway, a band of about three hundred settled permanently in and around the Yellowstone National Park, but in a very short time every animal outside of the protected limits of the Park was killed; and whenever any of the Park buffaloes strayed beyond the boundary, they too were promptly killed for their heads and hides. At present the number remaining in the Park is believed by Captain Harris, the Superintendent, to be about two hundred, about one-third of which is due to the breeding in protected territory."

It is curious to notice that even the bison hunters themselves were unaware of the extinction of the northern herd in the spring of 1883; and costly expeditions were actually fitted out in the autumn of that year to arrive at the bison country and find that the "happy hunting-grounds" existed no longer.

Such very briefly is the mournful history of the extermination of the two great herds of American bison. Scattered individuals or small droves still exist here and there in the more secluded parts of the country, in addition to those preserved in the Yellowstone. The pursuit of them is, however, unremitting, and the author considers that the final disappearance of every unprotected individual is but a question of time. In 1889 some twenty bison were seen grazing in the Red Desert of Wyoming, which narrowly escaped destruction. We have already mentioned the survivors of the southern herd still lingering in Texas; but there is strong evidence of the existence in the British district of Athabasca of a herd of "wood-buffalo," estimated at upwards of 550 in number. Exclusive of those in the Yellowstone, the number of wild bison existing in the United States on January 1, 1889, is given as 85. Finally, the whole census of living examples of the American bison—both wild and tame—at the date mentioned, gives only 1091 individuals.

That the Government of the United States will do all they can to increase and preserve the herd in the Yellowstone Park goes without saying; but the warning of the author that without great care, and unless (if this be possible) crossed, they will gradually deteriorate in size, should not be overlooked.

The account of the Smithsonian Expedition into Montana, which forms the concluding portion of the volume, although well told, is not of sufficient general interest to need further notice here.

In conclusion, we have to congratulate the author on having brought together such a number of facts in relation to the extermination of the bison, which, if they had not been recorded while they were fresh in men's memories, would probably have been entirely lost.

R. L.

DICE FOR STATISTICAL EXPERIMENTS.

Every statistician wants now and then to test the practical value of some theoretical process, it may be of smoothing, or of interpolation, or of obtaining a measure of variability, or of making some particular deduction or inference. It happened not long ago, while both a friend and myself were trying to find appropriate series for one of the above purposes, that the same week brought me letters from two eminent statisticians asking if I knew of any such series suitable for their own respective and separate needs. The assurance of a real demand for such things induced me to work out a method for supplying it, which I have already used frequently, and finding it to be perfectly effective, take this opportunity of putting it on record.

The desideratum is a set of values taken at random out of a series that is known to conform strictly to the law of frequency of error, the probable error of any single value in the series being also accurately known. We have (1) to procure such a series, and (2) to take random values out of it in an expeditious way.

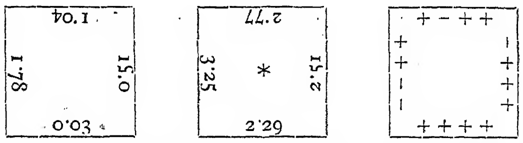
Suppose the axis of the curve of distribution (whose ordinates at 100 equidistant divisions are given in my "Natural Inheritance," p. 205) to be divided into n equal parts, and that a column is erected on each of these, of a + or a − height as the case may be, equal to the height of the ordinate at the middle of each part. Then the values of these heights will form a series that is strictly conformable to the law of frequency when n is infinite, and closely conformable when n is fairly large. Moreover the probable error of any one of these values irrespectively of its sign, is 1.

As an instrument for selecting at random, I have found nothing superior to dice. It is most tedious to shuffle cards thoroughly between each successive draw, and the method of mixing and stirring up marked balls in a bag is more tedious still. A tee-totum or some form of roulette is preferable to these, but dice are better than all. When they are shaken and tossed in a basket, they hurtle so variously against one another and against the ribs of the basket-work that they tumble wildly about, and their positions at the outset afford no perceptible clue to what they will be after even a single good shake and toss. The chances afforded by a die are more various than are commonly supposed; there are 24 equal possibilities, and not only 6, because each face has four edges that may be utilized, as I shall show.

I use cubes of wood $1\frac{1}{4}$ inch in the side, for the dice. A carpenter first planed a bar of mahogany squarely and then sawed it into the cubes. Thin white paper is pasted over them to receive the writing. I use three sorts of dice, I., II., and III., whose faces are inscribed with the figures given in the corresponding tables. Each face contains the 4 entries in the same line of the table. The diagram shows the appearance of one face of each of the 3 sorts of dice; II. is distinguished from I. by an asterisk

in the middle; III. is unmistakable. It must, however, be understood, that although the values are given to the second place of decimals both in the tables and in this diagram, I do not enter more than one decimal on the dice. The use of the second decimal is to make multiplication more accurate, when a series is wanted in which each term has a larger probable error than 1.

I. II. III.



In calculating Table I., n was taken as 48. This gives 24 positive and 24 negative values in pairs, but I do not enter the signs on the dice, only the 24 values, leaving the signs to be afterwards determined by a throw of die III. It will be observed that the difference between the adjacent values in Table I. is small at first, and does not exceed 0·2 until the last three entries are reached. These, which are included in brackets, differ so widely as to require exceptional treatment. I therefore calculated Table II. on the principle of dividing that portion of the curve of distribution to which those entries apply, into 24 equal parts and entering the value of the ordinate at the middle of each of those parts in that table. Moreover, instead of entering the three bracketed values on die I. I leave blanks. Then whenever die I. is tossed and a blank is turned up, I know that I have to toss die II., and to enter the value shown by it.

The precise process I follow is to put 2 or 3 of dice I. into a small waste-paper basket, to toss and shake them, to take them out and arrange them on a table side by side in a row, squarely in front of me, but by the sense of touch alone. Then for the first time looking at them, to write down the values that front the eye. If, however, one of the blank spaces fronts me, I leave a blank space in the entries. Having obtained as many values as I want from die I., I fill up the blank spaces by the help of die II.

Lastly, the signs have to be added. Now as $24 = 16 + 8 = 2^4 + 2^3$, it follows that 16 of the edges of die III. may be inscribed with sequences of 4 signs in every possible combination, and the remaining 8 with sequences of 3 signs. Then when die III. is thrown, the several entries along its front edge, which are 4 or 3 in number as the case may be, are inserted in an equal number of successive lines, so as to stand before the values already obtained from the other dice.

The most effective equipment seems to be 3 of die I., 2 of die II., 1 of die III., making 6 dice in all.

Values for Die I.

0·03 ...	0·51 ...	1·04 ...	1·78
0·11 ...	0·59 ...	1·14 ...	1·95
0·19 ...	0·67 ...	1·25 ...	2·15
0·27 ...	0·76 ...	1·37 ...	(2·40)
0·35 ...	0·85 ...	1·50 ...	(2·75)
0·43 ...	0·94 ...	1·63 ...	(3·60)

Values for Die II.

2·29 ...	2·51 ...	2·77 ...	3·25
2·32 ...	2·55 ...	2·83 ...	3·36
2·35 ...	2·59 ...	2·90 ...	3·49
2·39 ...	2·64 ...	2·98 ...	3·65
2·43 ...	2·68 ...	3·06 ...	4·00
2·47 ...	2·72 ...	3·15 ...	4·55

Values for Die III.

+ + + +	+ − − +	− − + +	+ − +
+ + + −	+ − − −	− − + −	+ − −
+ + − +	− + + +	− − − +	− + +
+ + − −	− + + −	− − − −	− + −
+ − + +	− + − +	+ + +	− − +
+ − + −	− + − −	+ + −	− − −

FRANCIS GALTON.

THE ROYAL SOCIETY SELECTED CANDIDATES.

THE following fifteen candidates were selected on Thursday last (April 24) by the Council of the Royal Society to be recommended for election into the Society. The ballot will take place on June 5, at 4 p.m. We print with the name of each candidate the statement of his qualifications.

SIR BENJAMIN BAKER, Mem. Inst. C.E.,

Hon. Mem. of the American Society of Mechanical Engineers, and of the Society of Engineers. Hon. Mem. of the Manchester Lit. and Phil. Soc. Has been engaged as an Engineer during the last twenty-five years, in the design and construction of many important works at home and abroad, including the Forth Bridge, and has carried out numerous investigations relating to the strength of materials and of engineering structures generally, and has contributed papers thereon to various Scientific Societies, viz., Proc. Inst. Civil Eng., Trans. Amer. Soc. Mech. Eng., Brit. Assoc. Reports, &c. Author of "A Theoretical Investigation into the Most Advantageous System of Constructing Bridges of Great Span," upon which plan the Forth Bridge and six of the largest bridges in the world have been built.

ROBERT HOLFORD MACDOWALL BOSANQUET, M.A.,

Fellow of St. John's College, Oxford. Barrister. Long and successful devotion to scientific inquiry, as shown by the following list of papers, and the printed copies sent herewith for the use of the Council:—"On an Experimental Determination of the Relation between the Energy and Apparent Intensity of Sounds of Different Pitch" (*Phil. Mag.*, xliv., 381–387); "On Just Intonation in Music; with a Description of a New Instrument for the Easy Control of all Systems of Tuning other than the Ordinary Equal Temperament" (Roy. Soc. Proc., xxi., 131–132); "Note on the Measure of Intensity on the Theories of Light and Sound" (*Phil. Mag.*, xlv., 215–218); "The Theory of the Division of the Octave, and the Practical Treatment of the Musical Systems thus obtained" (Roy. Soc. Proc., xxiii., 390–408); On the Polarization of the Light of the Sky" (*Phil. Mag.*, l., 497–520); "On a New Form of Polariscope and its Application to the Observation of the Sky" (*Phil. Mag.*, ii., 20–28); "On the Hindoo Division of the Octave, with some Additions to the Theory of Systems of the Higher Orders" (Roy. Soc. Proc., xxv., 540–541, xxvi., 372–384); "On the Relation between the Notes of Open and Stopped Pipes" (*Phil. Mag.*, vi., 63–66); "On the Present State of Experimental Acoustics" (*ibid.*, viii., 290–305); "Notes on Practical Electricity" (*ibid.*, xiv., 241–258); "On a Uniform Rotation Machine, and on the Theory of Electromagnetic Tuning Forks" (Roy. Soc. Proc., xxxiv., 445–447); "On Magneto-motive Force" (*Phil. Mag.*, xv., 205–217); "On Permanent Magnetism" (*ibid.*, 257–259, 309–316); "On Self-regulating Dynamo-electric Machines" (*ibid.*, 275–296); "On a Standard Tension Galvanometer" (*ibid.*, xvii., 27–30); "On a Determination of the Horizontal Component of the Earth's Magnetism at Oxford" (*ibid.*, 438–447); "On Electro-Magnets," No. I. (*ibid.*, 531–536); No. II., "On the Magnetic Permeability of Iron and Steel, with a new Theory of Magnetism" (*ibid.*, xix., 73–94); No. III., "Iron and Steel: a New Theory of Magnetism" (*ibid.*, 333–340); No. IV., "Cast Iron, Charcoal Iron, and Malleable Cast Iron" (*ibid.*, xx., 318–323); "Permanent Magnets," No. I. (*ibid.*, xviii., 142–153), No. II., "On Magnetic Decay" (*ibid.*, xix., 57–59); "On the Supposed Repulsion between Magnetic Lines of Force" (*ibid.*, 494–495). With a further list of twenty-seven papers.

SAMUEL HAWKESLEY BURBURY, M.A.,

Barrister-at-Law. Formerly Fellow of St. John's College, Cambridge. Second Classic, and Chancellor's Medallist, and fifteenth Wrangler in the year 1854. Has done much work in Mathematical Physics, espeially in the theories of Electricity and Magnetism and the Kinetic Theory of Gases. Joint author of Watson and Burbury's "Generalized Co-ordinates"; also of Watson and Burbury's "Electricity: Part I. Electrostatics." Author of sundry papers on physical science; for example, the following: Paper in *Phil. Mag.*, January 1876, "On the Second

Law of Thermodynamics in Connection with the Kinetic Theory of Gases"; *ibid.*, 1877, "On Action at a Distance in Dielectrics"; *ibid.*, 1881 (joint author), "On the Law of Force between Electric Currents"; *ibid.*, 1882, "A Theorem on the Dissipation of Energy"; *ibid.*, 1886, "Remarks on Prof. Tait's Paper 'On the Kinetic Theory of Gases'"; "Encycl. Brit." (joint author) Article, "Molecule." Attached to Science, and anxious to promote its progress.

WALTER GARDINER, M.A. (Cantab.),

F.L.S., Fellow of Clare College, Cambridge. University Lecturer in Botany. Rolleston Prize, 1888. Author of numerous papers containing original observations and discoveries in Vegetable Physiology, of which the following are the more important:—"The Development of the Water-glands in the Leaf of *Saxifraga crustata*" (*Quart. Journ. Micros. Sci.*, 1881); "On the Continuity of Protoplasm through the Walls of Vegetable Cells" (Phil. Trans., 1883, and Sachs, *Arbeit. d. Bot. Inst. in Würzburg*, Bd. iii.); "On the General Occurrence of Tannin in the Vegetable Cell, and a possible View of its Physiological Significance" (Camb. Phil. Soc. Proc., 1883); "On the Changes in the Gland-cells of *Dionæa muscipula* during Secretion" (Roy. Soc. Proc., 1883); "On the Phenomena accompanying Stimulation in the Gland-cells of *Dionæa dichotoma* (*ibid.*, 1886); "On the Power of Contractibility exhibited by the Protoplasm of certain Plant-cells" (*ibid.*, 1887); "On the Structure of the Mucilage Secreting Cells of *Blechnum occidentale* and *Osmunda regalis*" (*Ann. of Bot.*, 1887).

JOHN KERR, LL.D.,

Mathematical Lecturer in the Free Church Training College, Glasgow. Discoverer of the optical effects of Electrostatic Stress in transparent solids and liquids; and of the optical effects of magnetism on light reflected from iron.

ARTHUR SHERIDAN LEA, D.Sc. (Cantab.),

Fellow, Lecturer in Physiology, and Assistant Tutor of Gonville and Caius College, Assistant Lecturer of Trinity College. University Lecturer in Physiology. Author of the following papers:—"Ueber die Absonderung des Pancreas" (Heidelberg, 1876); "Some Notes on the Urea Ferment" (*Journ. of Physiol.*, vol. iv., 1883); "On a Rennet Ferment contained in the Seeds of *Withania coagulans*" (Proc. Roy. Soc., 1883); "On the Comparison of the Concentration of Solutions of Different Strengths of the same Absorbing Substance" (*Journ. of Physiol.*, vol. v., 1884); "Some Notes on the Isolation of a Soluble Urea Ferment from the Torula Ureæ;" "On the Digestion of Carbohydrates" (Physiol. Soc., May, 1886, *Journ. of Physiol.*, vol. vi., 1885). Author of the Appendix to Foster's "Physiology." Is distinguished for his acquaintance with Physiology. Is attached to Science, and anxious to promote its progress.

PERCY ALEXANDER MACMAHON, Major, R.A.,

As author of numerous papers in the *Quart. Journ. Math.*, vols. xix.-xxi., Proc. Lond. Math. Soc., vols. xv.-xix., *Amer. Journ. Math.*, vols. vi.-xi., on various subjects in Pure Mathematics, connected with Invariants, Semivariants, Perpetuants, Reciprocants, Partitions, Distributions, and Symmetric Functions. Associate Member of the Ordnance Committee. Instructor in Mathematics at the Royal Military Academy, Woolwich, 1882-88.

ALFRED MERLE NORMAN, M.A. (Oxon.),

Hon. Canon of Durham, D.C.L. (Durh.), F.L.S. Eminently distinguished for his researches in Marine Invertebrate Biology, carried on continuously for thirty-seven years. In 1880, Dr. Norman, by the special invitation of the French Government, took part in the deep-sea exploration in the Bay of Biscay, on board *Le Travailleur*, and for his services received, in 1884, the commemoration medal of the Institute of France. He edited, with additions, vol. iv. of "Monograph of British Spongiadæ," by the late J. S. Bowerbank, for the Ray Society. Author, along with T. R. Stebbing, of Crustacean Isopoda of the *Lightning*, *Porcupine*, and *Valorous* expeditions in the Zool. Soc. Trans., 1886; along with G. S. Brady, F.R.S., "Monograph of the Marine and Fresh-Water Ostracoda of the North Atlantic and North-West Europe," Roy. Dubl. Soc. Trans., 1889; "Report on the Crustacea of the Faroe Channel—H.M.S. *Knight Errant*" (1880). Author of over forty other reports published in the Brit. Assoc. Reports, *Ann. and Mag. Nat. Hist.*, *Journ. Conchol.*, *Journ Micros. Sci.*, &c., &c. Chairman of the Jury on Natural History at the Fisheries Exhibition, 1883. Possessor of Collections of the Invertebrate Fauna of the North Atlantic and Arctic Oceans, which are probably unequalled, and are always at the disposal of authors, as may be seen in every work published in Britain on the subject for the last twenty years.

WILLIAM HENRY PERKIN, Jun., Ph.D.,

F.I.C., F.C.S. Professor of Chemistry in the Heriot Watt College, Edinburgh. Formerly Privatdocent and Assistant in the Chemical Research Laboratory of the University of Munich. Distinguished as an Investigator, especially in devising new synthetic methods for the preparation of organic compounds containing closed carbon chains and in studying the properties of this important class of substances. This work has attracted great attention, both in this country and on the Continent. Author, and joint author, of upwards of fifty papers, published partly in the Journal of the Chemical Society, and partly in the *Berichte* of the German Chemical Society. Amongst others—"Condensation Products of Oenanthol," "Condensation Products of Isobutylaldehyde," "Benzoylacetic Acid and some of its Derivatives," "Synthetical Formation of Closed Carbon Chains," "Action of Trimethylene Bromide on Ethylic Aceto-acetate, Benzoyl-acetate and Malonate," "Action of Ethylene Bromide on Ethylic Aceto-acetate and Benzoyl-acetate," "Action of Ethylene Bromide on Ethylic Malonate," "Trimethylene Derivatives," "Some Derivatives of Tetramethylene," "Pentamethylene Dicarboxylic Acid," "Some Derivatives of Hexamethylene," "Derivatives of Hydrindonaphthene," "New Synthesis of Naphthalene Derivatives," "Dehydracetic Acid," "Phenylenediacrylic Acid," "Paranitro-benzoylacetic Acid," "Ethylic Diacetyladipate," "On Kamala," and "On Berberine." As a teacher he has been especially successful in suggesting and directing research work, as evinced by the number of papers he has published in conjunction with his students.

SPENCER UMFREVILLE PICKERING, M.A.,

F.C.S. Professor of Chemistry at Bedford College. Distinguished as as investigator of the thermal changes attending dissolution of salts. Author of papers on "The Action of Sulphuric Acid on Copper," "The Action of Hydrochloric Acid on Manganese Dioxide," "Sodium Thiosulphate and Iodine," "Basic Sulphates of Iron," "Sulphides of Copper," "The Constitution of Molecular Compounds," "Modifications of Sodium Sulphate," "Heat of Dissolution of Potassium and Lithium Sulphates," "Calorimetry of Magnesium Sulphates," "Modifications of Double Sulphates," "Multiple Sulphates," "Influence of Temperature on the Heat of Chemical Combination," "Water of Crystallization," "Heat of Hydration of Salts," and others, in all about forty, published in the Journ. Chem. Soc., the *Phil. Mag.*, and the *Chem. News*.

ISAAC ROBERTS, F.R.A.S.,

F.G.S., V.-P. of the Literary and Phil. Soc. of Liverpool. Discovery and publication, by aid of photographic methods, of Nebulæ in Andromeda, Orion, the Pleiades, and Vulpecula. Charting by photography a considerable portion of the stars of the northern hemisphere. Rediscovery of a minor planet by photography. Improvements in the apparatus and methods for giving long exposures in stellar photography. Invention of a machine for accurately charting the stars in a permanent manner by engraving them upon metal plates directly from the photographic negatives. The machine is also adapted for measuring the positions and magnitudes of the stars (*Monthly Notices*, Roy. Astron. Soc.). Determination of the Vertical and Lateral Pressures of Granular Substances (Proc. Roy. Soc., 1884); Investigation of the Movements of Underground Waters in Porous Rocks. Various papers on astronomical and geological subjects (see "Cat. of Sci. Papers, Roy. Soc."). Often finding opportunities of rendering valuable aid to those engaged in scientific research.

David Sharp, M.B., C.M. (Edin.),

President of the Entomological Society of London. Hon. Memb. Inst. New Zealand, &c. Distinguished as an Entomologist, especially for his knowledge of the order Coleoptera, many of the more intricate groups of which he has studied with reference to their structure, classification, geographical distribution, &c.; is attached to Science, and anxious to promote its progress. Author of the following memoirs:—"On Aquatic Carnivorous Coleoptera or Dytiscidæ," forming Vol. II. (Ser. 2) of the Scient. Trans. Roy. Dubl. Soc., 1879–82; "Memoirs on the Coleoptera of New Zealand" (*ibid.*, 1886); and, with the Rev. T. Blackburn, "Memoirs on the Coleoptera of the Hawaiian Islands" (*ibid.*, 1885); besides upwards of one hundred minor contributions to the Transactions of various Societies in England and on the Continent. Has also just completed a memoir on the Dytiscidæ, Staphilinidæ, &c., of Mexico and Central America, being Coleoptera, Vol. I., Part 2, of Messrs. Godman and Salvin's "Biologia Centrali-Americana" (pp. 824, pls. 19), and is now engaged in studying the Clavicornia and Rhynchophora for the same work. Since 1885 he has written the whole of the Insecta (except the Neuroptera) for the *Zoological Record.*

J. J. Harris Teall, M.A.,

F.G.S. Has taken a leading place among the petrographical geologists of this country, having enriched the literature of the science with important original contributions. Among these, special mention may be made of the following:—"The Patton and Wicken Phosphatic Deposit" (Sedgwick Prize Essay, 1875); "Petrological Notes on some North of England Dykes" (*Quart. Journ. Geol. Soc.*, 1884, p. 209); "On the Chemical and Microscopical Characters of the Whin Sill" (*op. cit.*, p. 640); "The Metamorphism of Dolerite into Hornblende-schist" (*op. cit.*, 1885, p. 133); "The Lizard Gabbros" (*Geol. Mag.*, 1886, p. 481); "On the Origin of certain Banded Gneisses" (*op. cit.*, 1887, p. 484). In 1888 he published a valuable treatise on "British Petrography," containing the results of much original research, and presenting for the first time a general review of the microscopic characters of all known British rocks. In the same year he was appointed to the Geological Survey, where he is specially charged with the investigation of the petrography of the crystalline schists.

Richard Thorne Thorne, M.B. (Lond.),

F.R.C.P. Assistant Medical Officer to H.M. Local Government Board. Has made numerous original observations in regard to the spread of disease, and especially on an epidemic of typhoid fever, and its dissemination by water at Caterham and Redhill. Author of "The Use and Influence of Hospitals for Infectious Diseases" (Proc. of the Internat. Sanit. Conference at Rome); and of a large number of Reports on Public Health to the Privy Council and Local Government Board. He was appointed along with Sir W. G. Hunter to represent Great Britain at the International Sanitary Conference of Rome, 1885. Is distinguished for his acquaintance with Sanitary Science, as shown by his being President of the Epidemiological Society, Lecturer on Public Health at St. Bartholomew's Hospital, Examiner in Public Health to the University of Oxford, the University of London, and the English Conjoined Board.

Walter Frank Raphael Weldon, M.A.,

Fellow of St. John's College, Cambridge. University Lecturer on the Advanced Morphology of Invertebrates in the University of Cambridge. Author of: (in the *Quart. Journ. Micros. Sci.*, 1883–88) "Note on the Early Development of *Lacerta muralis*"; "On the Head-kidney of *Bdellostoma*"; "On the Supra-renal Bodies of Vertebrata"; "*Dinophilus gigas*"; "*Haplodiscus piger*"; (in the Proc. Zool. Soc., 1884) "On some Points in the Anatomy of *Phœnicopterus* and its Allies"; "Note on the Placentation of *Tetraceros quadricornis*"; "Notes on *Callithrix gigot*"; (in the Proc. Roy. Soc.) "Note on the Development of the Supra-renal Bodies of Vertebrates"; "Preliminary Note on a Balanoglossus Larva from the Bahamas"; Note on the last paper; and a Report of Investigations into the Crustacean Fauna of Plymouth Sound, carried on in the laboratory of the Marine Biol. Assoc., in accordance with instructions from a Committee appointed by the Royal Society.

NOTES.

M. Eugène Peligot, the eminent French chemist, died at Paris on April 15. He was born on March 24, 1811. In 1832 he was admitted to the laboratory of J. B. Dumas, and three years afterwards he became Professor of Chemistry at the École Centrale. In 1846 he succeeded Clément Desormes at the Conservatoire des Arts et Métiers; and here, until recently, he continued to deliver courses of lectures on general chemistry. He also lectured at the National Agricultural Institute on analytical chemistry applied to agriculture. For more than 40 years he was connected with the French Mint, and at the Hôtel des Monnaies he lived and died. M. Peligot was elected a member of the Paris Academy of Sciences in 1852, and in 1885 he received the dignity of a Grand Officer of the Legion of Honour.

The death of Dr. F. Soltwedel, Director of the Botanical Station at Semarang, in Java, is announced. He was a very energetic botanist, especially in the direction of applied botany.

We learn from the *Botanisches Centralblatt* that Mr. Thomas Hanbury, of Mortola, near Mentone, has offered to defray the expense of the erection of a building in the Botanic Garden at Genoa, to provide a laboratory, lecture-rooms, and space for botanical collections. The building is to become the property of the University of Genoa, and will be erected under the direction of Prof. Penzig, the Director of the Botanic Garden; and it is hoped that it will be completed by the time of the International Botanical and Geographical Congress to be held in Genoa at the time of the great Columbus Festival in 1892. It is intended that the new Institute shall bear the name of the "Hanbury Botanical Institute."

During his visit to the Canaries, in 1889, made for the purpose of taking observations on the atmospheric absorption of the solar spectrum, Prof. O. Simony, of Vienna, landed upon the lonely rock of Zalmo, near the Island of Ferro, and discovered a very curious lizard, which was subsequently described by Prof. Steindachner (*Anz. k. Ak. Wiss. Wien*, 1889, p. 260) as *Lacerta simonyi.* At the request of Lord Lilford, Canon Tristram has also recently visited the same spot, and obtained some examples of this lizard, which Lord Lilford has presented to the Zoological Society's collection. Simony's Lizard is a fine large species, very dark in colour, but obviously allied to the well-known *Lacerta ocellata* of Southern Europe.

The fifth of the series of photographic exhibitions at the Camera Club, will be open for private and press view on Monday, May 5, at 8 p.m., and on and after Tuesday, May 6, it will be open to visitors on presentation of card. It will consist of photographs by the late Mrs. Julia Cameron.

The French Exhibition, which is about to be opened at Earl's Court, will illustrate the arts, inventions, products, and resources of France and her colonies, and will, it is said, include many of the best objects shown at the Paris Exhibition of last year.

An archæological museum has been established in connection with the University of Pennsylvania. *Science* says it contains—in addition to the American specimens—a fine collection of flints, bronze implements, and pottery from Europe, as well as objects from Asia, Africa, and the South Sea Islands. At the same University a museum of economic botany is about to be formed. It will consist of all kinds of woods, vegetable fibres, grains and drugs, arranged so as to illustrate the processes of manufacture from the raw product, and the various uses to which each material may be put.

The Marine Biological Laboratory at Wood's Holl, Massachusetts, will hold its third session during the approaching summer. The Institution has been so successful that a library, a lecture-room, and six private laboratories have lately been added to it.

The following are the arrangements for the science lectures to be given at the Royal Victoria Hall during May:—May 6, birth and death of mountains, W. W. Watts; May 13, London water supply, Prof. Bonney; May 20, how a photograph is taken, Dr. J. A. Fleming.

Dr. H. Ross has been appointed Lecturer on Botany at the University of Palermo, and Dr. G. B. De Toni Lecturer on Botany at the University of Padua.

Morot's *Journal de Botanique* for March 1 contains an interesting biographical sketch of the late M. E. Cosson, together with a bibliography of his numerous contributions to botanical literature.

At the last meeting of the Scientific Committee of the Royal Horticultural Society, the Rev. C. Wolley Dod gave an account of several diseases of plants in his garden, and commented on the difficulty of finding curative means, or of hearing of other suggestions than burning. He first alluded to a species of smut (Ustilago) on *Primula farinosa*, which appeared to be indigenous, as the plants were collected in Lancashire; and although it was grown with *P. denticulata*, the smut was confined to the former species. *Æcidium ficariæ* had attacked his hellebores. In this case, a drier soil was suggested as likely to prove effective in ridding the plants of the fungus. The "Lily spot," due to *Polyactis cana*, usually appearing late in summer, had been seen in April upon tulips, and apparently the same species on daffodils. It was suggested that a mixture of sulphate of copper and quicklime would prove effective, as in the case of vines. *Puccinia Schrateri* had occurred on daffodils from Portugal, and also upon the common double sorts.

At the meeting of the Society of Arts on April 23, Mr. W. Whitaker read a paper on "Coal in the South-East of England." Afterwards some remarks were offered by Mr. Topley, Prof. Rücker, Prof. McKenny Hughes, Dr. Archibald Geikie, who presided, and the author of the paper. Dr. Geikie said he thought everyone present must share his feeling of surprise and pleasure at finding that a number of geologists could come together and discuss a question like this with so little difference of opinion, and it might be taken as strong evidence that on this particular question there was nothing to fight about. He knew of no recent instance where a true scientific induction had been followed with such brilliant success as the one now brought forward. It had been discussed more or less academically by geologists for some sixty years, bit by bit evidence had accumulated as they went further and further below the surface, and at last it had been definitely proved that coal existed in the south-east of England. An ordinary observer would have found it almost impossible to imagine, when standing on a sunny day in the south of Kent, that coal was to be found there hundreds of miles from the great coal-fields, and it would be difficult to make such a person understand why geologists should pitch upon such a spot as a likely place for a colliery. Mr. Whitaker had gone over the evidence, and everyone must have realized how the conclusion had been arrived at, and how admirably the inference had been proved by experiment. But, as Prof. Hughes had said, they were very far from having reached a complete picture of the geography of the rocks that underlie the Secondary rocks of the south-east of England. They were groping their way by degrees, and in the process coal had been discovered. He did not imagine there could be any large continuous coal-field there; it could only exist in detached basins (even allowing for overthrusts), separated by uprises of older rocks. Further to the west they knew nothing by actual borings, and in no other way could anything like a map of the subterranean geology be obtained. It might be surmised with some probability that, between Bristol and the areas where borings had been made, there might be more extensive coal-fields than were at all likely to be found in the extreme south-east. They had heard of the wonderful plication of the Carboniferous strata in the west of France, but it must be remembered that not only had the Coal Measures undergone these movements, but the secondary rocks which overlay them had also been crushed, folded, and pushed over each other in the manner which any one might see on the south coast of Dorsetshire; and this process must have considerably thickened the Secondary rocks, the consequence of which was that you might bore through the same stratum sometimes a very long way. It was absolutely necessary that, in the prosecution of this matter, the practical man should go hand in hand with the man of science, otherwise a great deal of time, money, and labour would be wasted.

The Norwegian Government has laid before the Storthing a proposal to the effect that two-thirds of the cost of the Norwegian Polar Expedition under the command of Dr. Frithjof Nansen shall be defrayed by the State: the conditions being—that the expenses do not exceed 200,000 kroner (about £10,000); that if the expedition proves successful the vessels and scientific instruments used during the voyage shall become the property of the State; and that the Christiania University shall receive such specimens from the scientific collections as the senate shall select.

The Director of the Observatory at Tusa, in Sicily, noted two short but severe shocks of earthquake at noon on April 15. No damage was caused.

A shock of earthquake was felt at Lisbon on the morning of April 28.

M. E. Leyst, Superintendent of the Observatory of Pawlowsk, near St. Petersburg, has contributed to vol. xiii. of the *Repertorium für Meteorologie* an important investigation upon the influence of the times of reading the maximum and minimum thermometers upon the results deduced from them.

The Administration Report of the Meteorological Department of the Government of India for the year 1888–89 gives an account of some important changes in the working of the service since January 1, 1889. The change of the hour of morning observations from 10 a.m. to 8 a.m. has accelerated the publication of the Daily Weather Reports, and this result is much appreciated in Calcutta and Bombay. A uniform system of rainfall observations throughout India, and the telegraphing of rainfall information to Simla, enable the Department to prepare comprehensive rainfall charts and tabular statements for each week during the monsoon season. A local Daily Weather Report and Chart is now prepared at Bombay, in order to give early information to the commercial community, in a form similar to the Reports published at Calcutta. The Bombay Chamber of Commerce has contributed liberally towards the expenses of this service. The collection of information from ships in the Arabian Sea and Bay of Bengal is to be extended. This is essential for the investigation of the causes of the origin of storms; and, if sufficient material be collected, charts will be prepared for each day for two or three years. The charts must necessarily appear about three months after date. The work of observation with regard to storms is acknwledged to have been hitherto very defective. A small payment will be made in future for this service, and several valuable series of observations during dust-storms, &c., have already been received. The staff in India being insufficient to discuss the mass

of material which has accumulated during the last 13 years, the Government has wisely given a grant for the discussions of the more important series to be carried out by distinguished meteorologists in Europe. Several important investigations by the Indian staff are in a more or less advanced state of preparation, including an account of the cyclonic storms of August 1888, and of September 13–20, 1888; a paper on the relation of sunspots to weather, as shown by meteorological observations in the Bay of Bengal from 1855–78; and an account of the storm in the Arabian Sea in June 1887. At the commencement of the year under report, there were 161 observatories contributing regular observations.

M. P. Lafourcade, in a paper on the great bustard (*Revue des Sciences Naturelles Appliquées*), says that this bird is becoming very scarce in France, as it can flourish only in large uncultivated spaces. In Champagne and Provence it is never found. The small bustard is less rare.

Some observations on the brain-weight of new-born infants are given by Herr Mies in a Vienna medical paper. From 203 weighings he found the brains of male children to weigh on the average 339·3 grammes (say 11·9 oz.), and those of females 330 grms. (say 11·6 oz.). The lightest was 170 grms., and the heaviest 482 grms. The brain-weight of the new born infant is to the body-weight as 1 : 7 to 8·5. Only children living at the time of birth were considered.

At the meeting of the Royal Society of Queensland on February 17, Mr. W. Saville-Kent presented some interesting notes on the embryology of the Australian rock oyster (*Ostrea glomerata*). He mentioned that in connection with the investigation of this subject he had been carrying on a series of experiments with the view of accurately determining the influence upon the embryonic brood that is exercised by the advent of fresh-water floods or other sudden changes in the salinity of the water. Some important results had been obtained. From a series of oysters recently purchased in the market a fully matured male and female were selected for experiment. Portions of milt and ova from these two individuals were abstracted and commingled under precisely the same conditions, and placed respectively in water of three different degrees of salinity. The first admitted was placed in sea-water of the full ordinary strength. In the second there were equal proportions of salt and fresh water, and in the third one part of salt water to three of fresh. As a result, the ova placed in the equal admixture of salt and fresh water exhibited active vitality, and were quickly speeding in their developmental career. Of the ova placed in pure sea-water, but few were fructified, and these developed very slowly. Those, finally, placed in the water containing only a one-fourth proportion of sea-water were entirely deprived of life, and soon began to disintegrate. To this last circumstance Mr. Saville-Kent called special attention. It indicated, he said, the pernicious effect upon breeding oysters that might be exercised by heavy floods, and opened out a wide field for further inquiry.

A paper on the fossil butterflies of Florissant, Colorado, by Mr. Samuel H. Scudder, is included in the eighth Annual Report of the Director of the United States Geological Survey, and has now been reprinted separately. The specimens were found "in presumably Oligocene beds." There are altogether seven species, and they all belong to extinct genera. Their general aspect is "distinctly sub-tropical and American, while the Tertiary butterfly fauna of Europe is derived in the first place rom the East Indies, in the second from sub-tropical America, and in the third from home." With regard to one interesting point Mr. Scudder writes as follows:—"In living butterflies, as we ascend the scale of families we find an increasing atrophy of he front legs. In the two lower families, *Hesperidæ* and *Papilionidæ*, they are similar in structure to the other pairs, being normally developed. In the *Lycænidæ* (including in this the sub-families *Lemoniinæ* and *Lycæninæ*) they are atrophied in the male to a greater or less extent, with the loss of the terminal armature, while still perfect in the female. In the highest family, *Nymphalidæ*, with the single exception of the little group *Libytheinæ*, which agrees with the *Lycænidæ*, they are aborted in both sexes, often to an excessive extent. Now, in *Prolibythea* we have the forelegs of the female preserved, and in *Nymphalites* the foreleg of the male; in both cases they agree in all essential points with what we should expect to find in living forms belonging to the same groups, showing that at the earliest epoch at which butterflies are yet known these peculiar differences, marking the upward progression of forms, were already in existence. We must therefore look for the proofs either of great acceleration in development when butterflies first appeared, or of the existence of butterflies at a far earlier period than we yet know them."

In the yearly report of the East Siberian branch of the Russian Geographical Society, it is shown that the Miocene deposits in the middle parts of the provinces of Tomsk and Yeniseisk are much greater in extent and thickness than has hitherto been supposed. They contain, besides thin layers of coal, a rich flora, samples of which have been secured by M. Klementz. Leaves and needles of *Acer*, *Betula*, *Pinus Lopatini*, *Segusia*, *Sternbergi*, *Glyphostrobus*, *Magnolia*, *Ulmus*, *Populus*, and so on, are found in great quantities, and it seems probable that the Miocene flora of Siberia will prove as abundant and as suggestive of changes of climate as that of Switzerland.

An interesting and successful experiment in technical education is described in a resolution of the Indian Education Department, granting an increase of over 16,000 rupees in expenditure on schools in Sind. Appended to the resolution is an extract from a letter of Mr. Jacob, Inspector of Schools, in which he gives some details of the practical system of technical education which has been instituted in the Naushahro schools by Khan Bahadur Kadirdad Khan. The industries taught embrace Sind embroidery, tailoring, joining, and cabinet work, smith's work in iron and brass, electro-plating, mason's work, pottery, &c., and the attendance at all the classes is continually increasing. The boys in the workshops are divided into "gangs," each headed by a senior boy who has displayed exceptional skill. The schools are in close touch with the market; and, as orders come in, they are distributed among the gangs, and the profits of the work are divided among the members of the gang in proportions fixed by the teacher, and regulated by the degree of skill possessed by each individual. The industrial school for girls is most popular, and suggests new possibilities in the extension of female education; for it is found that the opportunity of earning money keeps the girls at school up to a later age than has hitherto been usual. Mr. Jacob says that the schools have created an extraordinary interest among the industrial classes, both Mahomedan and Hindu.

In a paper on the aborigines of Australia, printed in the current number of the Journal and Proceedings of the Royal Society of New South Wales, Mr. W. T. Wyndham speaks of the skill with which the natives use stone implements. "They turn out work," he says, "that you would hardly believe possible with such rough implements. They show great ingenuity, particularly in making their harpoon heads for spearing dugong and fish; instead of shaving the wood up and down with the grain as a European workman would do, they turn the piece of wood for a spear-head round and round, and chip it off across the grain, working it as wooden boxes are turned on a lathe. I have often sat and watched them doing this."

According to an official estimate, there are 170,000 wolves in Russia; and the loss caused by the destruction of sheep and swine by wolves is so great that it cannot be even approximately estimated. The reward paid for each wolf killed is 10 roubles. The number killed in 1889 in the single government of Wologda was 49,000, and in the government of Kasan 31,000. The number of human beings killed by wolves during the year was 203.

Mr. John Murray has issued an abridged and popular edition of Mr. Paul du Chaillu's "Adventures in the Great Forest of Equatorial Africa and the Country of the Dwarfs." While recognizing the work that has been done by later travellers in the regions with which his name is associated, Mr. du Chaillu says, in his new preface, that, so far as he is aware, no white man has been able since his time "to penetrate to the haunts of the gorilla and bring home specimens killed by himself."

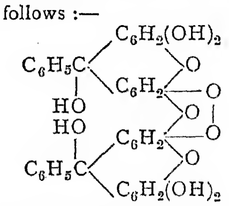
Part 19 of Cassell's "New Popular Educator" has been issued.

We have received "The Medical Register" and "The Dentists' Register" for 1890. Both works are printed and published under the direction of the General Council of Medical Education and Registration of the United Kingdom.

The seventh annual issue of the "Year-book of the Scientific and Learned Societies of Great Britain and Ireland" (C. Griffin and Co.) has been published. It comprises lists of the papers read during 1889 before Societies engaged in fourteen departments of research, with the names of their authors. The work has been compiled from official sources.

The following note on the words "cold-short" and "red-short" appears in *Engineering* of the 25th ult. Some of our readers may perhaps be able to throw light on the subject:—The words "cold-short" and "red-short" are so expressive that their etymology would seem at first sight to be entirely free from difficulty, but such is not the case. The earliest form of "cold-short" occurs in Philemon Holland's translation of Pliny's "Natural History" (1601) where it appears as "colsar." Vernatt and Whitmore, in their patent for the manufacture of iron granted in 1637, speak of "colshire" and "coleshire" iron, whilst Dud Dudley, in his famous tract "Metallum Martis" (1665), calls it "coldshare" iron. A still further variation appears in the Philosophical Transactions for 1693, in the course of a curious paper, written in 1674, giving an account of the hematite ores of Lancashire, where the writer speaks of "coldshire" and "redshire" iron. Andrew Yarranton, in his "England's Improvement by Land and Sea" (1677), uses the word "coldshore," and in Moxon's "Mechanick Exercises," published in the same year, red-short iron is described as "red-sear." The earliest known instance of "cold-short" and "red-short" is in a rare folio tract of 4 pages bearing the title "Beware of Bubbles," which, though undated, must, from internal evidence, have been issued in 1730. It forms one of a number of broadsides circulating about the time referring to a patent for the manufacture of iron taken out by Francis Wood, the well-known manufacturer of "Wood's halfpence," so unmercifully satirized by Swift in the "Drapier Letters." The words "cold-short" and "red-short" are at the present moment occupying the attention of the editor of the "New English Dictionary on Historical Principles," now in course of publication by the Clarendon Press, and if any of our readers are able to throw light upon the etymology of "cold-short" and "red-short" their suggestions will be gladly welcomed by the editor, Dr. Murray, Banbury Road, Oxford.

A new colouring matter from pyrogallol, $C_6H_3(OH)_3$, and benzotrichloride, $C_6H_5 . CCl_3$, is described in the current number of *Liebig's Annalen*, by Drs. Doebner and Foerster, of the University of Halle. When pyrogallol and benzotrichloride are heated to 160° C. in the proportion of two molecules of the former to one of the latter until no more hydrochloric acid is evolved, a fused mass is obtained which dissolves in alkalies with the production of a fine blue colour. The powdered product of the fusion is of a dark brown colour with a greenish metallic lustre. It may be obtained pure from solution in hot glacial acetic acid in the form of dark green crystals, which under the microscope appear as bright red transparent plates by transmitted light. The substance is almost insoluble in water, benzene, or carbon bisulphide, but is more soluble in alcohol and ether, and in hot chloroform. It dissolves in a hot solution of sodium acetate with production of a deep red colour. Caustic alkalies readily dissolve the pure crystals with production of the same blue colour as that yielded by the crude product of fusion. When the solution is just neutralized the colour is a bluish-violet, but the least excess of alkali reproduces the magnificent blue colour. Strong sulphuric acid dissolves the crystals with formation of a soluble sulphonic acid of a fine violet tint. Most metallic salt solutions yield with neutral solutions of the ammonium salt precipitates of the nature of "lakes" of varying composition and of various shades of bluish-violet. The colours produced by salts of aluminium and iron are perhaps the most striking. The yield of the new substance is very good, and generally amounts to about sixty grams of pure crystals for every hundred grams of pyrogallol employed. As regards its composition and constitution, its empirical formula is found to be $C_{38}H_{24}O_{11}$. It evidently contains four phenol hydroxyl groups, for it reacts with acid chlorides and anhydrides with production of compounds containing four acid radicals. The acetyl compound, $C_{38}H_{20}O_{11}(C_2H_3O)_4$, forms bright red crystals, melting at 208° C., which are decomposed by soda with formation once more of a blue colour. The benzoyl compound, $C_{38}H_{20}O_{11}(C_7H_5O)_4$, consists of thin red prisms possessing a brilliant green lustre, and melting to a deep red liquid at 251°. The substance also yields a hydro-reduction product with zinc dust and glacial acetic acid of the composition $C_{19}H_{14}O_5$; this reduction-product forms beautiful long colourless needles of silky lustre, which rapidly reoxidize in air, and especially on heating, to the original compound. Even if the needles are allowed to remain a short time in their mother-liquor they gradually become tipped with red, exhibiting an exceptionally pretty effect. The constitution of this hydro-body is shown to be $C_6H_5CH\left\langle\begin{matrix} C_6H_2(OH)_2 \\ C_6H_2(OH)_2 \end{matrix}\right\rangle O$, from which, taking into account the fact that four phenol hydroxyl groups are shown to be present by the mode of reaction with acid chlorides and anhydrides, the constitution of the new colouring matter is concluded to be as follows:—



The name which the discoverers propose for the compound is pyrogallol-benzeïn.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mrs. Pendry; a Brown Bear (*Ursus arctos* ♂) from Russia, presented by Miss Evelyn Muir; a Bateleur Eagle (*Helotarsus ecaudatus*) from East Africa, presented by Dr. E. J. Baxter; an Elliot's Pheasant (*Phasianus ellioti* ♀) from China, a Cape Weaver Bird (*Hyphantornis capensis* ♂) from South Africa, a Red-eyed Ground Dove (*Pipilo erythrophthalmus*) from North America, presented by Mr. Wilfred G. Marshall; a

Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Mr. J. Catheson Smith; an Egyptian Ichneumon (*Herpestes ichneumon*) from North Africa, two Grey Ichneumons (*Herpestes griseus* ♂ ♂), two Alexandrine Parrakeets (*Palæornis alexandri*) from India, two White Pelicans (*Pelecanus onocrotalus*), South European, deposited; a Musk Deer (*Moschus moschiferus* ♂) from Central Asia, seven Bearded Lizards (*Amphibolurus barbatus*), three —— Lizards (*Amphibolurus muricatus*), a Gould's Monitor (*Varanus gouldi*) from Australia, purchased; a Barnard's Parrakeet (*Platycercus barnardi*) from South Australia, received in exchange; an Indian Muntjac (*Cervulus muntjac*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on May 1 = 12h. 39m. 6s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 2917	—	—	12 18 50	−18 10
(2) δ Virginis	3	Yellowish-red.	12 50 6	+ 4 0
(3) ε Virginis	3	Yellowish-white.	12 56 42	+11 33
(4) ρ Virginis	5	Yellowish-white.	12 36 18	+10 51
(5) Z Virginis	Var.	—	12 28 12	− 3 49
(6) Comet *a* 1890, May 1	—	—	20 59 31	+30 12
,, ,, 2	—	—	58 23	+31 2
,, ,, 3	—	—	57 10	+31 53
,, ,, 4	—	—	55 51	+32 44

Remarks.

(1) During his spectroscopic survey of nebulæ in 1868, Lieut. Herschel noted that this gave a bright line spectrum. The three principal nebular lines and G were observed, but, as I have before remarked, other lines may possibly be found if carefully looked for. Some of the lines observed in other nebulæ, namely D_3 and lines near λ 559, 521, 517, 470, and 447, may be expected. In the General Catalogue the nebula is described as "Very bright; large, round; very suddenly much brighter in the middle to a nucleus; barely resolvable."

(2) According to Secchi, Vogel, and Dunér, this star has a magnificent spectrum of Group II., all of the ten ordinary bands being well visible. The band near D and the one less refrangible (Dunér's 2 and 3) are very wide, but the others are relatively narrow, though strongly marked. Dunér notices the peculiarity that band 5 (λ 546) is double. This should be further examined; the apparent duplicity may be simply due to the superposition of a strong line upon the dark fluting of lead. As the star is an exceptionally bright one for this group, comparisons with the bright flutings of carbon should be made, with the object of further confirming the cometary character of this group of stars.

(3) This is a star which has hitherto been classed with stars like the sun. The usual more detailed observations are required to determine whether the temperature of the star is increasing or decreasing.

(4) A star of Group IV. (Vogel). If the colour given by Vogel is correct, one would expect the metallic lines to be fairly well developed in this star, and it would probably be no longer classed in Group IV. The stars of this group are usually white or bluish-white, the yellowish-white stars generally falling in the later stages of Group III. or the earlier stages of Group V.

(5) The colour and spectrum of this variable have not yet been recorded, as far as I can determine, and the approaching maximum of May 5 may therefore afford a good opportunity of observing it. The range of variability is from 8·0 to 14·0 in a period of about 219 days.

(6) As this comet is travelling northwards and is gradually increasing in brightness, it may be well to note a few of the chief points to which attention should be directed in spectroscopic observations. The positions given are for Berlin midnight, and are reprinted from NATURE, vol. xli. p. 571.

Observations of the spectrum of a comet at one time only are now of little value, as there can be no doubt that the spectrum is subject to changes with the variations of temperature due to varying distances from the sun. The question now is: What is the precise nature of these changes? From a discussion of all the observations made up to 1888, Prof. Lockyer has laid down what he considers to be the most probable sequence; but as yet there has been no opportunity of testing his views by continued observations of one comet. According to his view, the spectrum of a comet near aphelion is like that of a planetary nebula, consisting simply of a bright line near λ 500. This, it will be remembered, was observed by Dr. Huggins in the comets of 1866–67. As the temperature increases, the spectrum of carbon begins to appear; at first the low-temperature spectrum (perhaps better known as the spectrum of carbonic oxide) makes its appearance, and afterwards the spectrum of hot carbon (commonly known as the hydrocarbon spectrum). The principal flutings in the first spectrum are near λ 483, 519, and 561, and those in the second are compound flutings with their brightest maxima near λ 564, 517, and 473. As the temperature goes on increasing, bright flutings of the metals manganese and lead (λ 558 and 546) are added to those of carbon, the chief effect of their presence being a variation in the appearance of the band near λ 564. With a still further increase in temperature, fluting absorptions of manganese and lead replace the corresponding radiations, and apparently shift the position of the citron band from λ 564 to 558 or 546, according to the preponderance of one element or the other. At the highest temperatures, which are only attained by comets which approach very close to the sun, bright lines of sodium, iron, manganese, and other substances, appear, as in Comet Wells and the Great Comet of 1882. (For further details, see Roy. Soc. Proc., vol. xlv. p. 189.)

For comparison spectra, a spirit lamp, and small quantities of magnesium and the chlorides of manganese and lead are all that are likely to be required, unless complete measurements of wave-lengths are attempted. The chief fluting in the spectrum of magnesium will serve for comparison with the line 500.

Variations in the form of a comet have not yet been associated with spectroscopic changes.

A. Fowler.

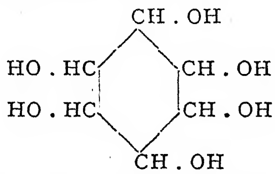
Comets and Meteor Streams.—In the cases of the Leonides and Andromedes, the annexed comet appears to be at the head of the swarms, and Schiaparelli and others have inferred from this fact that a comet is broken up by tidal disturbances. Other influences besides tidal action may cause it however, and M. Bredichin, in his memoir "Sur les étoiles filantes," showed how meteorites became detached from the central condensation by explosions, and describe orbits that differ according to the value of the initial velocity towards the sun, and the angle made by its direction with the radius vector. In a later communication (*Bull. Soc. Impér. des Naturalistes de Moscou*, 1889, No. 4) the form of the orbits generated by explosions in the comet, and their relation to such meteoritic streams as the Leonides and Andromedes, has been investigated. It is noted that in general the less the eccentricity of the generated ellipse, the more clearly marked are periods of maxima in falls of meteors. With the increase of eccentricity the maxima become less marked, and in the case of a parabolic orbit feeble falls occur each year. The regular periodicity of maxima would favour the formation of a meteoritic stream by a single eruption; in some cases, however, a series of eruptions must have taken place. M. Bredichin thinks that in the Leonid stream a single eruption was excessively preponderant, in the Andromedes a series of eruptions would appear to have occurred. Other cases have also been studied in detail. A meteorite is regarded as a portion of a large comet ejected from the parent mass by an eruption, and an investigation of the number of appearances of bright meteors indicates the connection between them and shooting stars, and, as would be expected, both have maxima when the earth is passing through a meteoritic stream. Although the connection between comets and meteorites is not a matter of doubt, the above investigation demonstrates it from a new point of view. It seems most probable, however, that the disintegration of a meteoritic swarm that has entered our system is caused by tidal disturbances as well as the repulsive action which is the cause of a comet's tail.

Stellar Proper Motions.—The number of known stars having proper motions is relatively considerable, but they are much dispersed through astronomical records; M. J. Bossert, however, in the *Bulletin Astronomique* for March 1890 gives an excellent synoptical table of such stars. Many calculations are facilitated by such a table, showing the elements that may vary the position of a star; and in a research on the motion of the solar system it is invaluable. All stars are included whose annual motion is 0″·5 or more. The list has been culled from every known catalogue and astronomical record, but the results

have not been accepted without an examination. Thus it is pointed out that the large proper motion given by Arago in his "Popular Astronomy" for the star in Argus, No. 2151 B.A.C., should be rejected, the comparison of Lacaille's observations with those of Stone and Gould giving, in fact, a motion of about 0″·2 for this star. The magnitude, co-ordinates for 1890·0, proper motion in right ascension and declination, the resultant motion, the direction of this motion, and the authority are given for each star.

OPTICAL ISOMERIDES OF INOSITOL.

DURING the last few months, whilst the brilliant researches of Prof. Emil Fischer on the synthetical production of the glucoses have been attracting so much attention, some very interesting work has been done on a compound which was formerly supposed to belong to the glucose group, viz. inosite. Maquenne, in 1887, showed that this compound, which is fairly widely distributed throughout the animal and vegetable kingdoms, is not a sugar, but a hexahydroxy-derivative of hexamethylene, having the constitutional formula—



It is an alcohol, and in accordance with the usual English nomenclature the name inosite must therefore be altered to inositol.

M. Maquenne has recently examined a compound obtained from the manna-like exudation of one of the Californian pines (*Pinus lambertiana*), and termed β-pinitol. He found that its formula is $C_7H_{14}O_6$, and that on heating with hydriodic acid it is resolved into methyl iodide and a substance which has the same composition as inositol, and resembles it in most of its properties, but melts at a higher temperature and rotates the plane of polarization to the right ($[\alpha]_D = 65$), inositol being inactive. It is therefore called *dextro-inositol.* Almost simultaneously, another French chemist, M. Tanret, obtained from quebracho bark (*Aspidosperma quebracho*) a sugar-like compound to which he has given the name quebrachitol. It has the same formula as β-pinitol, and on treatment with hydriodic acid yields methyl iodide and an inositol which can only be distinguished from the foregoing by its action on the plane of polarized light, which it rotates to the left to the same extent as the first compound does to the right, and must therefore represent the *lævo-inositol.* Both these compounds crystallize with two molecules of water in hemihedral crystals, and are very soluble in water.

MM. Maquenne and Tanret then jointly examined the effect of mixing concentrated solutions of equal weights of the dextro- and lævo-compound, and obtained an inactive inositol, which is much less soluble in water than either of its constituents, and melts at a higher temperature (253°), without previously becoming plastic. From its mode of formation, its constitution must resemble that of racemic acid, and the name *racemo-inositol* has therefore been given to it. It is not identical with the inactive inositol previously known, and the latter must therefore have an analogous constitution to mesotartaric acid.

We have therefore the interesting result that inositol, a derivative of hexamethylene, exists in four different forms, corresponding exactly to those of tartaric acid.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Buchanan, the University Lecturer in Geography, announces a course on "Oceanography," to begin at 2.15 p.m. on Wednesdays. The subject will be "The Distribution of Land and Water on the Globe."

The Council of the Senate have published a report in which they withdraw their original proposal (October 22, 1888) to suspend for 10 years from 1890 the augmentation of the contributions of Colleges to the Common University Fund prescribed by the present statutes, by way of relief to the depressed finances of some of the Colleges. They propose now to discriminate between Colleges that are financially depressed and those that are not. The latter will receive no relief under the new plan, the former will be allowed to make up their University contributions by devoting one or more Fellowships to University purposes. This proposal seems to have been much more widely approved than the former, and is signed by nearly all the members of the Council of the Senate.

The Special Boards for Physics and Chemistry, and for Biology and Geology, propose a new departure in the conduct of the second part of the Natural Sciences Tripos, with regard to which there are likely to be differences of opinion. Hitherto all the work considered by the examiners has been carried on at the time of the examination under their supervision, and under equal conditions for all candidates. The proposal now is to give credit for work in practical chemistry carried on before the examination in the University or College laboratories. The regulations recommended are :—

"In the second part of the examination, every candidate in chemistry may present to the examiners, at the commencement of the examination, a record of the chemical work which he has carried out in the University laboratory, or in some one of the College laboratories, in some one term. Such record shall be the original notes made from day to day in the laboratory, with the necessary calculations in full, and dated so as to show the work of each day.

"To the record shall be appended a certificate, signed by the candidate and by the superintendent of the laboratory, stating that all the manipulations involved in the work have been *boná fide* carried out by the candidate alone, and that the superintendent has watched the progress of the work and believes the record of it to be faithful.

"In estimating the merits of the candidates, the examiners shall give credit for such work.

"This regulation shall be first applicable to the examination for the Natural Sciences Tripos of the year 1892."

The Report is signed by 12 members of the two Boards, the total number of members being 31. The chemists whose names appear are Prof. Liveing, Dr. Ruhemann, and Dr. Tilden.

Mr. J. Pedrozo d'Albuquerque, B.A., Scholar of St. John's College, First Class, Natural Sciences Tripos, 1887–88, has been appointed Government Professor of Chemistry at Barbadoes.

Applications for permission to occupy the University's tables in the Zoological Station at Naples, and in the Marine Biological Laboratory at Plymouth, are to be sent to Prof. Newton, Magdalene College, Cambridge, on or before May 22.

The Newall Telescope Syndicate have issued a further Report, in which it appears that a means has been found for overcoming the threatened financial difficulty. Mr. H. F. Newall, M.A., of Trinity College, University Demonstrator of Experimental Physics, and son of the donor of the telescope, has offered his services as observer, without stipend, for five years, a sum of £500 for initial expenses, and a guarantee of £200 a year for five years for maintenance, provided the University can furnish the balance of the funds required. He also offers to build himself a private house near the new Observatory, if a suitable site can be found. The Sheepshanks Fund is, moreover, able to promise an additional sum of £100 a year after five years from the present date. The outcome of these offers is that the University will only be required to find at present a capital sum of £125, and an annual subsidy of £30. After five years, it may have to build an observer's house at a cost of £800, and provide £150 a year towards his stipend. Mr. Newall has worthily seconded his father's munificence, and it is to be hoped that no further obstacle will arise to the founding of an adequate observatory of stellar physics in Cambridge.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 13.—"The Nitrifying Process and its Specific Ferment." By Percy F. Frankland, Ph.D., B.Sc. (Lond.), A.R.S.M., &c., Professor of Chemistry in University College, Dundee, and Grace C. Frankland. Communicated by Prof. Thorpe, F.R.S.

The authors have been engaged during the last three years in endeavouring to isolate the nitrifying organism.

Nitrification, having been in the first instance induced in a

particular ammoniacal solution by means of a small quantity of garden soil, was carried on through twenty-four generations, a minute quantity on the point of a sterilized needle being introduced from one nitrifying solution to the other. From several of these generations, gelatine plates were poured, and the resulting colonies inoculated into identical ammoniacal solutions, to see if nitrification would ensue; but, although these experiments were repeated many times, on no occasion were they successful.

It appeared, therefore, that the nitrifying organism either refused to grow in gelatine, or that the authors had failed to find it, or that, growing in gelatine, it refused to nitrify after being passed through this medium.

Experiments were, therefore, commenced to endeavour to isolate the organism by the dilution method. For this purpose a number of series of dilutions were made by the addition to sterilized distilled water of a very small quantity of an ammoniacal solution which had nitrified. It was hoped that the attenuation would be so perfect that ultimately the nitrifying organism alone would be introduced.

After a very large number of experiments had been made in this direction, the authors at length succeeded in obtaining an attenuation consisting of about $\frac{1}{1000000}$ of the original nitrifying solution employed, which not only nitrified, but on inoculation into gelatine-peptone refused to grow, and was seen under the microscope to consist of numerous characteristic bacilli hardly longer than broad, which may be described as bacillo-cocci.

Although this bacillo-coccus obstinately refuses to grow in gelatine when inoculated from these dilute media, yet in broth it produces a very characteristic though slow growth.

Nitrification was also induced in ammoniacal solutions by inoculating from such broth cultivations.

March 27.—"On the Progressive Paralysis of the Different Classes of Nerve-cells in the Superior Cervical Ganglion." By J. N. Langley, F.R.S., and W. L. Dickinson.

Summary.—Generally speaking, stimulation of the cervical sympathetic in the dog with minimal effective shocks causes pallor in the lips and gums; with weak to moderately strong shocks, primary pallor followed by flushing; with strong shocks, as shown by Dastre and Morat, primary flushing, but the extent and duration of the primary effect and of the secondary effect, if there is any, vary in different dogs.

In the rabbit and cat, stimulation of the cervical sympathetic always causes, as shown by Bochefontaine and Vulpian, primary pallor in the lips and gums, and the after-flush is not great. The pallor we find is bilateral; the degree of the pallor on the opposite side to that stimulated varies in individual cases, it can be seen in the tongue, as well as in the lips and gums.

On injecting nicotin into a vein, certain of the normally occurring effects of stimulating the cervical sympathetic cease before the others, *i.e.* since all the effects can still be produced by stimulating the fibres running from the superior cervical ganglion, the nerve-cells in the ganglion, which are connected with different classes of nerve-fibres, are paralyzed with different degrees of ease by nicotin.

Arranging the various effects in the order of ease of paralysis, we have:—

Rabbit.

(1) Withdrawal of the nictitating membrane.
(2) Opening of eye.
{ (3) Dilation of pupil.
{ (4) Constriction of blood-vessels of conjunctiva.
{ (5) Constriction of blood-vessels of lips and gums.
{ (6) Constriction of blood-vessels of ear.

In one or two cases, no difference in the ease of paralysis between the bracketed actions has been observed.

Cat.

(1) Secretion from sub-maxillary gland.
(a) { (2) Opening of eye.
(3) Dilation of pupil.
(4) Constriction of blood-vessels of conjunctiva.
(5) Constriction of blood-vessels of mouth. }
(b) { (6) Constriction of blood-vessels of ear.
(7) Withdrawal of nictitating membrane. }

(*a*) Constant differences between these have not been observed.

(*b*) These have not been directly compared, but in separate experiments each has been obtained when (1) to (5) were no longer seen.

Dog.

(1) Dilation of arteries of bucco-facial region.
(2) Movements of eye and opening of eyelids.
(3) Withdrawal of nictitating membrane.
(a) { (4) Constriction of the arteries of gums and lips.
(5) Dilation of pupil.
(6) Secretion from sub-maxillary gland.
(7) Constriction of blood-vessels of the sub-maxillary gland. }

(*a*) Differences between these have not always been observed.

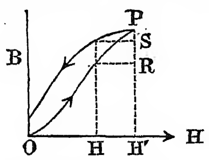
At a certain stage of nicotin poisoning, when stimulation of the sympathetic does not cause withdrawal of the nictitating membrane, but does cause dilation of the pupil, a partial *closing* of the eye is obtained by stimulating the sympathetic.

It will be noticed that in each animal nicotin abolishes most of the effects of stimulating the cervical sympathetic at very nearly the same time. With regard to these, we think that there is only a *primâ facie* case for regarding the differences observed as due to an unequal paralysis of the nerve-cells of the superior cervical ganglion, for it is possible that the differences may be due to an unequal tonic stimulation reaching the parts by nerve-fibres other than the sympathetic. But the greater differences observed, for instance, between the secretion of saliva and the dilation of the pupil in the cat, the flushing of the lips and the constriction of the vessels of the sub-maxillary gland in the dog, we do not think can be due to such a cause, and we attribute them to an unequal paralyzing action of nicotin upon the nerve-cells of the superior cervical ganglion.

Linnean Society, April 17.—Mr. Carruthers, F.R.S., President, in the chair.—Lord Arthur Russell, on behalf of the subscribers to a portrait of Sir Joseph Dalton Hooker, which had been painted at their request by Mr. Hubert Herkomer, R.A., formally presented the portrait to the Society, and in a few words expressed the satisfaction which he was sure would be felt at the acquisition of the likeness of so distinguished a botanist. It was announced that a photogravure of the portrait was in preparation, of which a copy would be presented when ready to every subscriber to the portrait fund.—Prof. P. M. Duncan, F.R.S., exhibited a vertical section through a large coral, *Fungia echinata*, cutting through and across the septa and synapticulæ and the so-called base. The union of the sides of contiguous septa at the base is either incomplete or by means of synapticulæ.—Dr. Edward Fischer, of Zurich, exhibited and made remarks on certain species of *Polyporus* bearing a sclerotium possessing the structure of *Pachyma cocos*, but it was doubtful whether the *Polyporus* represented the fructification of the *Pachyma*, or was merely parasitic on it. Mr. George Murray expressed himself in favour of the latter view.—Mr. J. E. Harting exhibited alive a so-called "singing mouse" which had been captured at Maidenhead a week previously, and which uttered sounds like the subdued warbling of a linnet. He desired to be informed whether the cause usually assigned for the phenomenon was correct—namely, some obstruction or malformation of the trachea. Prof. Stewart stated that he had observed alive, and dissected when dead, a similar specimen, and had found no trace of any organic disease or malformation.—Sir Charles Sawle, Bart., exhibited a specimen of the Little Green Heron, *Butorides virescens*, of North America, which had been shot by his keeper at Penrice, St. Austell, Cornwall, in October last, and which he had sent for preservation to a taxidermist at Bath. Mr. J. E. Harting offered some remarks on the occurrence, and suggested various ways in which the bird might have reached England. He observed that the larger American Bittern, *Botaurus lentiginosus*, had been met with some five-and-twenty or thirty times in the British Islands, and, strange to say, had been described and named by an English naturalist, and a Fellow of this Society, Colonel George Montagu (who obtained a specimen of the bird in Dorsetshire), a year before it was described by Wilson as a native of the United States.—A paper was then read by Mr. Spencer Moore, on some micro-chemical reactions of tannin. In this an account was given of the behaviour of Nessler's test for ammonia upon tannin, which it usually colours almost immediately some shade of brown or reddish brown. The great value of the reagent is held to reside in the rapidity of its action; moreover in none of the many experiments did it fail. Reference was also made to some other new tannin tests, especially to some in which, as in Nessler's fluid, caustic potash furnishes the basis, and which, like that fluid, are very rapid in their action.—A paper by Mr.

E. Saunders, on the tongue of the British Hymenoptera Anthophila, in the absence of the author was read by Mr. W. Percy Sladen, and was illustrated by excellent drawings.

Physical Society, April 18.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Prof. Rücker described the results of some recent magnetic work undertaken by himself and Prof. Thorpe in connection with their magnetic survey of the United Kingdom.—Mr. T. H. Blakesley (Hon. Secretary) read a paper, on a theory of permanent magnetism, by M. Osmond. The author stated that iron exists in two distinct physical states, one soft, or "*α iron,*" and the other hard, or "*β iron.*" The β variety is non-magnetic, and is formed during heating, hardening, or by electrolysis, whilst the soft or α modification is produced by long annealing. In a piece of steel the author considers the β molecules to form a rigid framework in which the α molecules become interlocked under the influence of magnetizing force, and on the degree of interlocking the permanent magnetism depends. By a graphical method it is shown that the permanent magnetism should be a maximum when the two varieties are present in equal quantities. If the proportions of carbon and manganese in the steel are considerable, then nearly all the iron is of the β variety, and the steel is nearly non-magnetic. In hardening a piece of ordinary steel, the surface layers being cooled most rapidly contain more β molecules than the interior; hence for a certain degree of hardness (when the outer layers have more α molecules than β ones) a laminated magnet will be a better permanent magnet than a solid one, but for a much greater degree of hardness the reverse may be the case. Mr. Swinburne asked if the theory would account for the increase of induction which occurs when the circuit of a permanent magnet is closed; most theories founded on the orientation of particles by the magnetizing force seemed defective in this respect. Some time ago he had suggested that the permeability of iron should be tested by first magnetizing it one way, and then at right angles to the first direction; recently he had been informed that no increase of permeability was observed when the experiment was performed. Prof. Perry said he had subjected iron to magnetization in one direction and found the permeability for small forces in a direction at right angles much smaller than he had anticipated; the first magnetizing force was kept constant when the small perpendicular one was applied. Mr. Swinburne thought that for such small perpendicular forces the permeability should be nearly infinite. He also said there seemed to be a sort of angular hysteresis in iron, for if a loose running armature was turned slowly round by hand, it would come back 2° or 3° when left free. The President remarked that, as far as he could see, M. Osmond's theory does not account for the great influence which a small percentage of tungsten has on the magnetic property of steel, and all theories which failed in this particular must necessarily be imperfect. Mr. Blakesley pointed out that the ordinary hysteresis curves showed that a small superimposed magnetizing force in a direction different from the primary one produced only a small change in the induction, and hence would give a small



permeability. For example, the increment HH′ (see diagram) causes an increase RP in the induction, whilst an equal decrement H′H produces only a change PS.

Geological Society, April 16.—J. W. Hulke, F.R.S., Vice-President, in the chair.—The following communications were read:—On the disturbed rocks of North-Western Germany, by Prof. A. von Könen, For.Corr.G.S.—On the origin of the basins of the Great Lakes of America, by Prof. J. W. Spencer, State Geologist of Georgia. From the study of the hydrography of the American lakes, from the discovery of buried channels revealed by borings, from the inspection of the glaciation of the lake region, the consideration of the late high continental elevation, and the investigation of the deformation of old water-levels, as recorded in the high-level beaches, the explanation of the origin of the basins of the Great Lakes becomes possible. The original Erie valley drained into the extreme western end of Lake Ontario—the Niagara river being modern—by a channel now partly buried beneath drift. Lake Huron, by way of Georgian Bay, was a valley continuous with that of Lake Ontario; but between these two bodies of water, for a distance of about 95 miles, it is now buried beneath hundreds of feet of drift. The old channel of this buried valley entered the Ontario basin about twenty miles east of Toronto. The northern part of Lake Michigan basin was drained into the Huron basin, as at present; whilst the southern basin of that lake emptied by a now deeply drift-filled channel into the southwestern part of Huron. The buried fragments of a great ancient valley and river, and its tributaries, are connected with submerged channels in Lake Huron and Lake Ontario, thus forming the course of the ancient St. Lawrence (Laurentian) river, with a great tributary from the Erie basin and another across the southern part of the State of Michigan. This valley is of high antiquity, and was formed during times of high continental elevation, culminating not long before the Pleistocene period. The glaciation of the region is nowhere parallel with the escarpments, forming the sides of, or crossing the lakes or less prominent features. During the Pleistocene period, and especially at the close of the episode of the upper Till, the continent was greatly depressed, and extensive beaches and shorelines were made, which are now preserved at high elevations. With the re-elevation of the continent these old water-levels have been deformed, owing to their unequal elevations. This deformation is sufficient to account for the rocky barriers at the outlets of the lakes. Some of the lakes have been formed, in part, by drift obstructing the old valley. The origin of the basins of the Great Lakes may be stated as the valley (of erosion) of the ancient St. Lawrence river and its tributaries, obstructed during and particularly at the close of the Pleistocene period, by terrestrial movements, warping the earth's crust into barriers, thus producing lake-basins, some of which had just been formed in part by drift deposited in the ancient valley. The reading of this paper was followed by a discussion, in which Dr. Hinde, Prof. Bonney, Dr. Irving, Mr. Clement Reid, Rev. E. Hill, Prof. Seeley, Mr. Whitaker, and the author took part.—On Ornithosaurian remains from the Oxford Clay of Northampton, by R. Lydekker.—Notes on a "wash-out" found in the Pleasley and Teversall Collieries, Derbyshire and Nottinghamshire, by J. C. B. Hendy.

Chemical Society, March 20.—Dr. W. J. Russell, F.R.S., President, in the chair.—Prof. J. W. Judd, F.R.S., delivered a lecture on the evidence afforded by petrographical research of the occurrence of chemical change under great pressure, in which he discussed the question as to how far the phenomena observed by the geologist in the study of rocks under the microscope can be explained by the laws that have been experimentally determined by the physicist and chemist.—The following papers were read:—The formation of triazine-derivatives, by Prof. R. Meldola, F.R.S.—Contributions to the knowledge of mucic acid; Part 1, hydromuconic acid, by Dr. S. Ruhemann and Mr. F. F. Blackman.—The molecular weights of metals when in solution, by Messrs. C. T. Heycock and F. H. Neville. The authors give the results of their observations on the effect of various proportions of silver, gold, copper, nickel, sodium, palladium, magnesium, zinc, lead, cadmium, mercury, bismuth, calcium, indium, aluminium, and antimony on the solidifying point of tin. Of all these metals, antimony alone behaves abnormally, producing a rise instead of a depression in the solidifying point. In the majority of cases the atomic depression is a number not far removed from 3, the theoretical value calculated from Van't Hoff's formula. Assuming the truth of Raoult's generalization, that the depression produced by a molecular proportion of any substance in the solidifying point of the same solvent is the same whatever the substance, it would therefore seem probable that the molecules of most metals are of the same type, M_n, where n is the number of atoms in the molecule; and if it be supposed that the molecules of zinc, for example, when dissolved in tin are monatomic as in the gaseous state, it would follow that n is unity in the case of many other metals. In the case of aluminium, the atomic depression is so nearly half the average value that it seems probable that the molecule is diatomic. Indium resembles aluminium in producing an abnormally low depression, and it is noteworthy that the value for mercury is also distinctly low.

March 27.—Annual General Meeting for the election of Officers and Council.—Dr. W. J. Russell, F.R.S., President, in the chair.—The President, in his address, discussed the teaching of chemistry to medical students, and drew attention to the

importance of the medical man being well trained in elementary chemistry, pointing out that it was too seldom recognized that the fundamental action of medicines—the origin of their power—is a chemical change, and that if an understanding and appreciation of their effects are to be sought for, the first steps must be to learn the laws which govern chemical change, and the chemical nature of the substances employed. He urged, that in place of the present unsatisfactory system, chemistry should be placed on an equal footing with anatomy, medicine, and physiology, in which subjects the Examining Board of the two Colleges insists that the student shall have studied at a recognized medical school, thus recognizing most wisely the importance of study under efficient instructors and at places properly equipped.

PARIS.

Academy of Sciences, April 21.—M. Hermite, President, in the chair.—On the theory of the optical system formed by a double plane mirror in front of the object-glass of an equatorial, and movable about an axis, by MM. Lœwy and Puiseux. In a previous note (April 14) the authors dealt with the formulæ relative to the employment of one plane mirror movable about an axis. They now study the system obtained by replacing the single mirror by two reflecting surfaces cut on the same block of glass in the form of a prism.—On Weber's law of electrodynamics, by M. H. Poincaré.—On the heat of formation and reactions of hydroxylamine, by MM. Berthelot and André. One of the results of the investigation is to confirm the similarity between ammonia and hydroxylamine, their heats of formation showing only a slight difference. Hydroxylamine cannot therefore be regarded as oxidized ammonia.—On the nutrition in hysteria, by M. Bouchard. The author quotes a work by M. Empereur, "Sur la Nutrition dans l'Hystérie," published in 1876, in which demonstrations of the normal pathological state during hysteria, similar to those described by MM. Gilles de la Tourette and Cathelineau, are given.—Observations of Brooks's comet (*a* 1890) made with the *coudé* equatorial (35 cm. free aperture) of Lyons Observatory, by M. G. Le Cadet. On March 28 the comet appeared as an almost perfectly round nebulosity without any noticeable point of condensation. Its magnitude was estimated as 11·5.—On the actual minimum of solar activity, and the spot which appeared in March 1890 at a remarkably high latitude, by M. A. Riccò. A comparison of the number of spots that appeared in 1890 with the number observed in 1878 indicates that the minimum certainly passed towards the end of last year.—On a transformation of differential equations of the first order, by M. Paul Painlevé.—Construction for radius of curvature in certain classes of curves, notably Lamé's curves, parabolas and hyperbolas of various orders, by M. G. Fouret.—On mica condensers, by M. G. Bouty. The author finds that at ordinary temperatures, and for differences of potential from 1 to 20 volts, a thin lamina of mica opposes an absolute obstacle to the continued passage of electricity through it; also, that residual charges do not appear to depend on the penetration of electricity, so to speak, into the dielectric, but rather on a progressive increase of the dielectric constant.—On the mechanical action of alternating currents, by M. J. Borgman. In a note presented on February 3, the author described a method by means of which it was easy to produce the repulsion of conducting masses by a coil traversed by an alternating, or simply an intermittent current, discovered by Elihu Thomson. To determine the influence of various conditions on this phenomenon, the author has undertaken, and describes a series of experiments made with modified apparatus.—Halos and parhelia observed at St. Maur Park, by M. E. Renou. The relative number of halos and parhelia observed in different years and in different months of the year are given.—On one of the causes of the loss of iron ships on account of the perturbations of the magnetic needle; determination of the amount of deviation for each ship, by M. Léon Devaureix. The author has observed the deviation of the compass during six consecutive voyages from Bordeaux to La Plata, returning by Dunkirk.—Note on the preparation of iridium dioxide, by M. G. Geisenheimer. Iridium dioxide is obtained in fine brown-red microscopic needles by heating potassium iridate in a platinum crucible for an hour with 15 times its weight of a mixture of equivalent quantities of chloride and bromide of potassium. The crystals are isolated by washing first with water and then with aqua-regia. Analysis proves them to be pure IrO_2.—Action of hydrogen peroxide on the oxygen compounds of manganese; Part 1, action on the oxides, by M. A. Gorgeu. The author concludes that in the process of decomposition of hydrogen peroxide by the peroxides of manganese, the latter, especially in presence of acids, are themselves reduced to some extent if they contain more oxygen than is indicated by the formula $Mn_3O_4, \frac{1}{2}H_2O$, and that the analysis of hydrogen peroxide should not therefore be carried out by means of the hydrated higher manganese oxides.—Preparation and heat of formation of sodium erythrate, by M. de Forcrand.—Note on the chlorine derivatives of the amylamines, by M. A. Berg. Three chlorine derivatives—namely, monochloramylamine, dichloramylamine, and chlorodiamylamine—have been prepared by the action of hypochlorites on amylamine and diamylamine hydrochlorides. Analyses and descriptions of the properties of the three bodies are given.—On the alcoholic fermentation of inverted sugar, by MM. U. Gayon and E. Dubourg. Following the progress of the fermentation by means of the polarimeter, the authors show that the two components of invert-sugar are attacked with different degrees of rapidity, and that different ferments do not act in the same manner, some attacking the lævulose by preference, others the remaining component.—Note on alcoholic fermentation and the transformation of alcohol into aldehyde caused by *champignon du muguet*, by MM. Georges Linossier and Gabriel Roux.—A geological paper, by M. Stanislas Meunier, gives an account of the results of the lithological and geological examination of the meteorite from Jelica (Servia), 1889.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Studies in Evolution and Biology: A. Bodington (E. Stock).—Glimpses into Nature's Secrets: E. A. Martin (E. Stock).—A Manual of Anatomy for Senior Students: E. Owen (Longmans).—Monograph of the British Cicadæ Part 2: G. B. Buckton (Macmillan).—Fur Seal and other Fisheries of Alaska (Washington).—National Academy of Sciences, vol. 4, Part 2; 3rd Memoir—The Temperature of the Moon: S. P. Langley (Washington).—The Solar Corona: F. H. Bigelow (Washington).—Photographs of the Corona taken during the Total Eclipse of the Sun, January 1, 1889; Structure of the Corona; D. P. Todd (Washington).—National Health: B. W. Richardson (Longmans).—The Function of Labour in the Production of Wealth: A. Philip (Blackwood).—Magnetism and Electricity: W. J. Harrison and C. A. White (Blackie).

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THURSDAY, MAY 8, 1890.

CHEMICAL TECHNOLOGY.

Chemical Technology; or Chemistry in its Applications to Arts and Manufactures. Edited by C. E. Groves, F.R.S., and W. Thorp, B.Sc. Vol. I., "Fuel and its Applications." By E. J. Mills, D.Sc., F.R.S., and F. J. Rowan, C.E. (London: Churchill, 1889.)

THIS work is described as substantially a new edition of the well-known "Chemical Technology" of Richardson and Watts, which in its turn was founded on the German work of Knapp. In its new form, however, it bears about as much resemblance to its prototype as the famous horse of Wallenstein does to the original animal—"The head, neck, legs, and part of the body have been repaired; all the rest is the real horse." How much of the real Knapp is left in the work takes some time and trouble to discover. We recognize here and there a woodcut—not always in the best state of preservation—but the descriptions appended even to these particular cuts are in most cases entirely recast, if not wholly rewritten. As the present work is to all intents and purposes an original production, it would have been better to have so described it. It may be that a sort of good-will has grown up around Richardson and Watts's "Technology" which the publishers desire to retain; but the connection between the two works is so slight that they are practically independent.

The present volume deals exclusively with fuel and its applications. The term fuel is employed in its widest possible sense, and its applications are treated of no less generally. The special employment of fuel in chemical manufactures is reserved for future treatment in the volumes which are concerned more particularly with these subjects. The most superficial comparison of this work with that upon which it is assumed to be founded will serve to show how enormous has been the advance in knowledge of the principles upon which the proper consumption of fuel depends. Take, for example, the question of smoke-prevention. In the preface to the 1856 edition it was stated that a method of smoke-prevention, although much wanted, had not then been discovered. The present work shows that we have changed all that. The idea of "consuming smoke" is obsolete. The conditions of complete combustion are to-day so well understood that it is only the indifference of manufacturers or the apathy of the authorities which prevents the greater part of industrial firing with solid fuel from being practically smokeless. Even if this were not so, gaseous fuel, the use of which is largely extending, is absolutely smokeless. This kind of fuel might be applied to many industries which have not yet adopted it, and without in any way hampering them, and would indeed be so applied if the authorities could be brought to regard it as coming within the definition of "the best practicable means" (to quote the words of the Act) for preventing smoke. We do not, of course, intend by this to insist on the exclusive adoption of gaseous fuel, although the time may come when, partly on economic and partly on sanitary grounds, such adoption may, as the late Sir C. W. Siemens predicted would be the case, become compulsory. How much the community might save, both in health and pocket, by the more systematic adoption of smoke-preventing arrangements, has been demonstrated over and over again, and we may well hope that the various exhibitions of appliances which seek to realize a consummation so devoutly to be wished may bring about this result in a not too distant future. The public may rest assured that efficient smoke-preventing appliances do exist, as the work before us abundantly demonstrates, and it ought to be the duty of the various centres of local government to insist on the more general adoption of these appliances. What can be done by a benevolent despotism in such a matter was well shown by the action of Lord Palmerston in the case of the metropolis, and there is nothing to prevent even such towns as Glasgow, Sheffield, Birmingham, and Newcastle from having atmospheres at least as sootless as that of London.

The out-put of coal in this country up to 1883 practically followed the law of Jevons as modified by Marshall. In that year it attained a maximum of nearly 164 million tons, or about four times the amount raised in 1850. In 1884 the quantity raised was 161 million tons, and in the following year it fell to about 159 millions. This diminution is due to various causes, partly natural and partly economic and social. It is, however, safe to say that a more intelligent appreciation of the principles which determine the conversion of the store of energy existing in coal into actual work has more than compensated for the smaller out-put. The world to-day gets more duty from its coal than it did even six years ago. Authorities differ slightly as to the manner in which the coal raised is distributed. According to Peckar, whose estimate seems to be preferred by the authors, it is somewhat as follows:—[1]

Destination.	Per cent.
Iron manufacture	32·40
Factories	21·87
Dwelling-houses	16·36
Gas- and water-works	6·46
Mining	6·38
Steamers	6·46
Railways	1·76
Copper-works	0·72
Sundries	0 64
Export	10·54

Although it is no necessary part of a treatise which is mainly concerned with the applications of fuel, the authors devote some considerable space to what may be termed the chemistry of coal-getting, *e.g.* the occurrence and nature of fire-damp, the relations of atmospheric temperature and pressure to its escape, fire-damp indicators, the influence of coal-dust on explosions, &c. On the whole, the information given is sound and accurate, and brought fairly well up to date. And the authors steer a very even and judicious course among points on which much difference of opinion still exists. We think, however, that the very careful and remarkable analyses of certain North Country explosions by the Messrs. Atkinson are worthy of more notice than they have apparently received, as they seem to be absolutely conclusive on the point that explosions can be originated and propagated by coal-dust alone.

[1] No explanation is given of the fact that these numbers add up to 103·59.

The question of the spontaneous ignition of coal scarcely meets with the treatment which its importance merits, and no reference is made to the work of the Royal Commission appointed in 1875 at the instigation of the Board of Trade and the Committee of Lloyd's to inquire into this subject. Many hundreds of vessels have without doubt been lost by the spontaneous ignition of coal cargoes, and there is a general belief that, with the considerable increase of temperature in steam-ships due to the introduction of high-pressure boilers and triple-expansion engines, the liability to spontaneous firing in the coal-bunkers is greatly augmented. The old idea of Berzelius, that the tendency to spontaneous ignition was mainly due to the presence of readily-oxidizable pyrites is now exploded. The experiments of Richters, Durand, and, in quite recent times, of Prof. Vivian Lewes, have shown that this substance has quite a subordinate effect. The cause is rather to be ascribed to the effect of absorbed or occluded oxygen upon finely-divided carbonaceous matter, *e.g.* dust or fine slack. The authors are also of this opinion, and state that the only method of preventing fire from such a cause is to keep the temperature of the mass of coal as low as possible by means of thorough ventilation by currents of air. In a paper recently read before the Institution of Naval Architects, in which this question is discussed, Prof. Lewes comes to the conclusion that this so-called ventilation is undoubtedly one of the most prolific causes of spontaneous ignition, and he instances the cases of the four colliers, *Euxine*, *Oliver Cromwell*, *Calcutta*, and *Corah*, which were loaded at Newcastle under the same tips, at the same time, with the same coal, from the same seam. The first three were bound for Aden, and were all ventilated. The *Corah* was bound for Bombay, and was not ventilated. The three thoroughly ventilated ships were totally lost from spontaneous ignition of their cargo, whilst the *Corah* reached Bombay in safety. Prof. Lewes points out that for ventilation to do any good, cool air would have to sweep continuously and freely through every part of the cargo, a condition impossible to attain, whilst anything short of that only increases the danger, the ordinary methods of ventilation supplying about the right amount of air to create the maximum amount of heating. A steam coal absorbs twice its bulk of oxygen, and takes about ten days to do it under favourable conditions, and it is this oxygen which in the next phase of the action enters into chemical combination and causes the serious heating.

One very remarkable change which is slowly making its way in this country is seen in the more extensive adoption of coal-washing machinery. Coal-washing machines have long been in use in Germany, France, and Belgium, and the exigencies of our iron manufacture are gradually necessitating their introduction in Great Britain, although the process has not yet reached the same degree of perfection as on the Continent. The effect of washing is to free the coal from pyrites and other mineral impurities. Of course it is only under special conditions that it can pay to subject the coal of this country to this process, but there is no doubt that as soon as the price of coal touches a certain point many coals which are practically unsaleable at present will be so treated. The authors describe a number of the more important coal-washing machines, and give details of their duty and cost.

The question of coking and coke-ovens naturally comes in for a very considerable share of attention, and practically all the more important methods are described and fairly well illustrated, and the general nature of the tars yielded by the various kinds of ovens is set forth, mainly on the authority of Mr. Watson Smith. One of the most valuable features in the work is the account given of the methods of using liquid fuel, and of the results obtained on various railways (principally Russian) and with various types of marine and stationary engines. The principles of domestic heating by solid and gaseous fuel, and relative efficiency of the various forms of open and closed grates and of gas stoves, are carefully stated, and considerable space is given to an examination of the modes of warming public buildings.

Analyses of boiler performances, and of the results obtained by mechanical stokers and by the application of gas-firing to boilers, methods of evaporation, with special reference to multiple effect apparatus, occupy a large portion of the section devoted to fuel in its applications to vaporization and evaporation; and the concluding portions of the book are occupied by descriptions of special forms of kilns and furnaces.

The work is admirably printed, and on the whole well illustrated, and, what is very important in a book which is mainly a work of reference, it is furnished with an excellent index. T. E. Thorpe.

THE SELKIRK RANGE.

Among the Selkirk Glaciers; being the Account of a Rough Survey in the Rocky Mountain Regions of British Columbia. By William Spotswood Green, M.A., F.R.G.S. (London: Macmillan and Co., 1890.)

THE Canadian Pacific Railway, after crossing the watershed of the Rocky Mountains by the Hector Pass, descends for some four thousand feet into the valley of the Columbia River. This, for a hundred and seventy miles, flows in a northerly direction, parallel with the crest of the range. Then, after a great sweep to the west, it flows southward, parallel to its former course, till it receives the Kootenay, the head waters of which rise only a mile and a half away from its own. The mountain-tract insulated by these rivers is called the Selkirk Mountains. It lies roughly parallel with the Rockies, and yet further west are the Gold Range and the Cascades. Thus the railway traverses a mountain region until the valley of the Frazer River, by which it finally emerges, begins to broaden out towards the sea. It is, to use Mr. Green's words, "a region of vast ravines and wide valleys, whose sides, when not bare rock precipices, are clad in sombre forests, through which wild mountain torrents rush from glacier sources to placid lakes, where, after resting for a while and reflecting the hoary cedars and hemlocks, they issue forth as great rivers, and with swift current hurry on to lose themselves in the Pacific."

Though the peaks of the Selkirks look down upon the railway, their recesses, before Mr. Green's visit, were still almost unknown. The reason is not difficult to discover. The forests which clothe their lower slopes are unusually

dense—often almost impenetrable; the traveller often has to force every step of his way among great trees, upright and prostrate, hampered by a frequent undergrowth, till the forest at last gives place to alder scrub which seems to possess all the offensive properties of the dwarf pine in the Eastern Alps, and to be a yet greater obstacle to the mountaineer. Even hunting-parties of Indians but rarely visit the Selkirks.

Mr. Green left England in the summer of 1888, with the intention of exploring and making a rough survey of the chief peaks and glaciers in the Selkirk Mountains. He was accompanied by a relative, the Rev. H. Swanzy, and took with him a mountain outfit and the requisite scientific instruments, lent by the Royal Geographical Society, who had made a grant in aid of the expense of the expedition. Convenient head-quarters were found at Glacier Hotel (4122 feet), where the trains, as at Göschenen on the St. Gothard, halt for meals; but many nights of the six weeks passed in this region were necessarily spent under canvas.

The portion of the Selkirk Range explored by Mr. Green lies mainly to the south of Rogers Pass (where the railway crosses the watershed of that range at a height of 4275 feet). It is bounded on the east by Beaver Creek, a tributary of the Columbia, and on the north by the Illecellewaet River, by which the railway descends. Many of the peaks rise above 10,000 feet; few, if any, surpass 11,000; Mount Sir Donald, which is possibly the highest, being 10,645. But the average elevation of the range is considerable, and as the peaks rise precipitously some 6000 feet above the valleys, the scenery is very fine. Though not comparable with that of the Pennine Alps, the mountain outlines are not inferior to those in some districts of the Tyrol; and the forests, where spared by the frequent fires, are far more grand. The snow-line is at about 7000 feet, the forests ending at about 6000 feet; the glaciers are numerous, and sometimes large, the most important, named the Geikie Glacier, being about 4 miles long and 1000 yards wide. As usual, old moraines and huge erratics indicate that they formerly extended far below their present limit. The Selkirk Mountains, it may be observed, correspond in latitude with the Mendip Hills.

The dominant rocks are rather fine-grained micaceous schists, the structure of which has evidently been much modified by pressure, so that it is difficult to say whether this has produced crystallization of the constituents or modified a rock once more coarsely crystalline. A snow-white quartzite or quartz-schist is also very conspicuous, and not seldom caps, and no doubt has helped to determine, the higher peaks. In one part the rocks have a yet more ancient aspect, while to the north of the railway, near some lead-mines, a black slate exhibits certain markings which may possibly be the remains of graptolites. So far as can be inferred from the specimens brought back by Mr. Green, the Selkirks are composed of either later Archæan or earlier Palæozoic rocks—probably, in great part, of the former.

That Mr. Green can use the pen as well as the pencil was proved by his former book, on "The High Alps of New Zealand." The present one deals with a more limited district, and does not include any climb equal in difficulty to that of Mount Cook; but the ascent of Mount Bonney, the second, if not the highest, peak in the Selkirks, offered more than one "bad place," and the difficulties of these excursions were enhanced by being made without guides, and in many cases only by the two travellers. Thus they not only had to be their own porters, and often carry a load of forty pounds over rough ground and up steep ascents, but also were only "two on the rope," a phrase which is significant to mountaineers. Their toils and hardships—and these were many—the physical features and natural history of the country, are all graphically described: in short, Mr. Green has written a book which not only is a record of a mountain survey carried out under exceptional difficulties, but also indicates that he possesses in an exceptional degree the qualifications for a scientific traveller, and that he can write as well as he can climb.

T. G. B.

THE ANATOMY OF THE FROG.

The Anatomy of the Frog. By Dr. Alexander Ecker, Professor of Human and Comparative Anatomy in the University of Freiburg. Translated, with numerous Annotations and Additions, by George Haslam, M.D., Scientific Assistant in the Medical Department in the University of Zürich; formerly Assistant-Lecturer in Physiology in the Owens College, Victoria University, Manchester. Illustrated with many Wood Engravings and Two Coloured Plates, executed by Hofmann, Würzburg, Bavaria. (Oxford: Clarendon Press, 1889.)

THE rapid advance of physiology and morphology since the completion of Profs. Ecker's and Wiedersheim's "Anatomie des Frosches" has intensified the desire for a text-book which should deal in the most exhaustive manner with the anatomy of the frog, "the physiological domestic animal." Dr. Haslam remarks in his preface that he has done his best to bring the original of "Ecker's Frog" up to date, and in this task he has thoroughly succeeded. More than one hundred new figures, of which one-third are original, have been added, and copious lists of references to frog literature have been drawn up. He has restricted himself to the most careful and concise description of the various organs, and has abstained from entering into the discussion of such morphological questions as bear upon the comparison of the Anura with other Vertebrata. It would therefore be out of place to criticize the retention of names which—like atlas for the first free vertebra—if applied to the frog alone, are perfectly clear in their meaning, although their true morphological value, and therefore true denomination, may possibly be different. Every anatomist knows the difficulties connected with the frog's first spinal or hypoglossal nerve; its description on p. 182 will enable the reader to form his own opinion as to which of the three or four names he may adopt.

The first section of the book, dealing with the bones and joints, has remained unaltered, but the nomenclature generally used by English anatomists has been adopted throughout. The different nature of the clavicles and the precoracoid elements has been correctly described according to investigations made since the appearance of the German original in 1864. Howes's researches on the composition of the carpus and tarsus came too late to be embodied in the English edition, but the occipito-cervical region might have been more extensively treated.

The second section, on muscles, has not been changed. Here Fuerbringer's and de Man's nomenclature might, with great advantage, have been added to, or rather given instead of, many of the antiquated synonyms of Zenker and Dugès. These, however, are points of minor importance, and are, after all, matters of opinion.

The account of the central nervous system, and that of the sympathetic system, are quite new. The same applies to the heart. Some excellent figures illustrating the anatomy of the heart have been added, or have taken the place of old illustrations. The account of the arterial system remains practically unaltered, but many additions have been made to the venous system, and the description of the lymphatics has been rearranged. Much labour has been devoted to the histology of the alimentary canal and its appendages, to a great extent based upon original research made by the translator himself. A summary tabulation of the researches on the structure of the intestinal epithelium will be found on pp. 288–290. Section VI. is devoted to the respiratory organs and the neighbouring glands. A second carefully-finished and coloured plate contains many histological drawings, mostly original, of lungs, liver, and kidneys. The histological account of the thyreoid and thymus is almost entirely new, and a pair of lymphatic glands in the hyoid region, hitherto mentioned by Tolldt only, have been rediscovered and have been described as tonsils.

"A very large number of preparations have been made to investigate the vessels and uriniferous tubes of the kidneys; and the description of the remaining organs of this section (genital organs, adrenals, and fat-bodies) has received large additions from recent publications." The description of the minute structure of the fat-bodies, with illustrations, is a new and original contribution. The eighth, or last section, treats of the skin and sense-organs. The latter were treated somewhat summarily, and meagrely illustrated, in the original edition. This defect has been amended by so many new illustrations, and by the addition to the text of the results of so much recent research, that the whole section has assumed a completely new aspect. Especially may be mentioned the structure and distribution of the peculiar tegumentary papillæ and other tactile organs. The various other sense-organs, especially the ear, and above all the eye, have received much painstaking attentention, and have been amply illustrated by copies from the works of the most recent writers.

Of the 227 woodcuts we may say that they have been so carefully cut, and come out so clearly on the good paper, that the blue and red colours used for the vessels and muscles in the original edition are not at all missed. It would be going far beyond the scope of a general review to point out all the important additions and emendations which the new book contains (by the way, clearly indicated by square brackets), nor would it be fair to search for mistakes—of which, after all, there seem to be remarkably few. Those who use the work, whether for the sake of the many hundreds of references to the literature, or in order to be guided in the dissection necessary for a delicate experiment, will soon find that Dr. Haslam has greatly improved a book which was already good.

H. G.

OUR BOOK SHELF.

Syllabus of Elementary Dynamics. Part I. Linear Dynamics; with an Appendix on the Meanings of the Symbols in Physical Equations. (London: Macmillan and Co., 1890.)

THIS is a small pamphlet in which the author defines the terms usually found in works on dynamics. When dealing with measurements of quantities, he adopts an appropriate series of capital letters for specified units, and another set where the units are left undefined; thus equations containing the latter class of symbols possess more generality. Other units of higher dimensions are represented by capitals which are over-marked. Thus an acceleration of 10 feet per second per second is written as $10\bar{F}$. These are very useful for the author's purpose, though it requires no little time to become used to them. A few formulæ and results are obtained in connection with falling bodies, energy, and centre of mass.

Where quantities are represented by certain units and multipliers, it becomes necessary in every case to state any existing relations between the units employed. Instead of being under any such necessity, many physicists prefer to regard these multipliers as completely representing the quantities themselves. The advantages of such a system are discussed in the appendix, and a series of examples worked out in parallel columns, showing the advantage of the one or the other of the two methods suggested. G. A. B.

Organic Evolution, as the Result of the Inheritance of Acquired Characters according to the Laws of Organic Growth. By Dr. G. H. Theodor Eimer. Translated by J. T. Cunningham, M.A., F.R.S.E. (London: Macmillan and Co., 1890.)

THE work of which this is a translation we have already reviewed. The only additional matter contributed by the translator seems to be an ill-advised reference to NATURE in the preface.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Bison not Aurochs.

IN his excellent article on the extermination of the American bison, "R. L." remarks (NATURE, p. 11) on the transatlantic practice of miscalling that animal a "buffalo"; but on the next page he writes of "its European congener the Lithuanian aurochs." This is to perpetuate a common error at least as bad. The "aurochs" (= ox of yore), Latinized by Cæsar in the form *urus*, is or was the *Bos primigenius* or *B. urus* of scientific nomenclature. It is wholly by mistake that in its extinction as a wild animal its ancient name was transferred to the bison, or *Zubr*. I would invite "R. L." to turn to the word "Bison" in Dr. Murray's "New English Dictionary," where he will find a reference to an article "*Wisunt*" in Schade's "Altdeutsches Wörterbuch," which ought to settle the question. I only wish one could ascertain to what animal the name "buffalo" strictly belongs. There unfortunately Dr. Murray does not help us.

ALFRED NEWTON.

Magdalene College, Cambridge, May 4.

Unstable Adjustments as affected by Isolation.

IN a brief passage in his very suggestive volume on "Darwinism," Mr. Wallace refers to a principle which seems to me to be worthy of much wider application than he has given. It is a key which requires only a little filing to prepare it for unlocking some difficult problems in divergent evolution. Speaking of the infertility of crosses, he says:—"It appears as if

fertility depended on such delicate adjustment of the male and female elements to each other that, unless constantly kept up by the preservation of the most fertile individuals, sterility is always ready to arise. . . . So long as a species remains undivided, and in occupation of a continuous area, its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions" (p. 184). Here is an application of the principle of segregation, or of like to like in groups that do not cross, in which indiscriminate separation is followed by increasing divergence in the different portions, not because they are exposed to different environments, not because there is any advantage in such divergence, not because there is any need that the function should be performed more perfectly in one portion than in the other, but because intergeneration, which is the principle by which correspondence of function is secured, has been suspended for some generations; and, in the absence of intergeneration, neither natural selection nor any other principle is capable of preserving complete correspondence. In organisms that reproduce sexually, the causes of divergence are many, though they may all be classed as causes of segregation, while the causes of correspondence with variation, whether of functions or of structures, are causes of intergeneration between partial segregations. If the environments surrounding the isolated portions are the same, the *use* of the environment, and therefore the forms of natural selection, may become divergent; if the use continues unchanged, some *useless divergence in the method of securing the use* may appear. Or, if all the relations to the environment, whether useful or useless, remain unchanged, "the adjustment of the male and female elements to each other" are liable to become slightly divergent, producing *mutual infertility*. Or the *preference of the sexes* for certain shades or arrangements of colour in their mates may become slightly different. Through some slight difference in the hereditary elements, distributed in each separated portion at the first, one or all of these causes of accumulated divergence may be introduced. I think it is evident that we have here a general principle, which is as applicable to a wide range of divergences as it is to the divergence that produces mutual infertility and sterility.

The context shows that the prominent idea in Mr. Wallace's mind was divergence in the adjustment of the male and female elements through correlation with "some diversity of form or colour," resulting from divergent forms of natural selection, that had been induced by exposure to "somewhat different conditions of life." But if the reasoning is correct in the sentences I have quoted, it gives an explanation of similar divergences when the separated portions are exposed to the same environment, *and where there is no possible advantage to be gained by divergence*. This is one of the principles I have used in the explanation of the divergences of Sandwich Island land mollusks; and I think that in the earlier stages of the development of infertility between allied forms it is often the only explanation that is applicable. It should, however, be remembered that, for divergence of this kind, it is not always necessary that the isolation should be either complete or very long continued, and that, when the forms that are not fully fertile with each other meet and more or less commingle, there is, through the very laws of propagation, without any aid from natural selection, a constant increase in the ratio of the pure breeds to the mongrels, and an accumulating intensity in the segregative instincts and the physiological incompatibilities. As this point has been fully discussed in my paper on "Divergent Evolution," I do not need to enlarge on it here (see Linn. Soc. Journ., Zoology, vol. xx. pp. 246-72).

There is, however, another phase of the subject which is indicated by Mr. Wallace's suggestion that infertility depends on "such a delicate adjustment" that it is more easily affected by isolation than some other adjustments. This is, I think, a very interesting point, as it suggests how it is that, in some cases at least, physiological divergence of this kind *is one of the first forms of divergence that arises*. But in some species other adjustments seem to be more delicate than this, and therefore more easily disturbed; while in others, several sets of adjustments, as colours and other recognition marks, with the preferences that correspond, and the habits of feeding and defence are in a state of equilibrium, the stability or instability of which is about the same as of that which determines the relations of the male and female elements. In this last class of cases, several forms of divergence may arise during the same stage of development, and that too when the isolated portions are exposed to the same environment. In some species a large number of characters are in a state of unstable adjustment. As Prof. Lankester has suggested near the close of his review of Mr. Wallace's book, this cause of divergence seems to be specially operative in the case of human faculties. But variability with plasticity of type is not the only condition that affects the stability of segregated portions of a species. Other things being equal, a single pair of any species is much less likely to represent the average of all the characters of the species than a million pairs. This consideration throws light on the comparative lack of divergence between the land animals of England and those of Ireland, which lack has been referred to by Mr. Wallace as an objection to my theory. In this case, many millions of some of the species were probably existing in each district at the time of the separation. As Prof. Lankester has pointed out, the representatives of the human species in the two districts have somewhat diverged; and the probability is, that if we were equally acquainted with the other species, we should find other examples of divergence in minor points. If the isolation is made more complete, and is longer continued, I believe the divergence will gradually become more apparent.

Mr. Wallace has mentioned another class of divergences which are best explained by the principle we are now considering —as he seems to have apprehended, though the process is not stated here as clearly as when discussing the divergences that produce infertility. The passage is as follows:—"The enormously lengthened plumes of the bird of paradise and of the peacock must be rather injurious than beneficial in the birds' ordinary life. The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, *a surplus of strength, vitality, and growth-power which is able to expend itself in this way without injury*. That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities of plumage*. . . . Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to that individual variability, which has served as the starting-point for so much that seems to us strange in form, or fantastic in colour, both in the animal and vegetable world" ("Darwinism," p. 293. The italics are mine).

It is no small gratification to me that Mr. Wallace has found this principle of unstable adjustment worthy of application to two important classes of divergences; and that, in the case of one of these classes, he has recognized that correspondence in such adjustments cannot be continuously maintained between the isolated portions of a species. I, moreover, have some hope that, when he understands the relation in which instability and isolation stand to each other in my theory, he will admit that it throws some light on the remarkable divergences of Sandwich Island land mollusks. The subject was only incidentally touched upon in my paper on "Divergent Evolution through Cumulative Segregation," but will be more fully discussed in a supplemental paper on "Intensive Segregation."

26 Concession, Osaka, Japan. JOHN T. GULICK.

Coral Reefs, Fossil and Recent.

MANY Alpine geologists believe the limestone and dolomite mountains which form so peculiarly beautiful and interesting a part of our Eastern Alps to be in great part composed of Triassic coral reefs. If this be so, their geological structure must necessarily contribute much towards elucidating the discussion concerning the origin of atolls and other forms of recent coral reefs. In this discussion, which has chiefly been carried on in England, the structure of our Triassic limestone mountains has been left out of account in a manner very surprising to me, considering that authorities like Richthofen and Mojsisovics have declared them to be remnants of coral reefs.

I have made a number of Alpine ascents in the dolomites of South Tyrol, chiefly in the districts of the Höhlensteinthal, Primiero, and the Langkofel, and have satisfied myself that the theory of the coralligenous origin of great part of these mountains is the only one which will explain the position and nature of the rocks composing them.

Not only do we observe in many places the massy dolomite alternating at its margin with sedimentary deep-sea deposit of

partly non-calcareous nature, but we even find old reef surfaces exposed to view. The volcanic porphyritic lava, or rather tufa, which was spread over the sea-bottom after the termination of the Buchensteiner period (middle Triassic, Mediterranean province) covers the deep-sea deposits of earlier date, but leaves the masses of dolomite free. Here and there, as on the *Plattkofel*, it can be seen overlying the foot of the actual reef precipice and there ending. It is covered by similar dolomite of a later date.

Many observations by Dana, Walther ("Korallriffe des Rothen Meeres"), and others, have shown that old coral reefs, about the nature of which there can be no doubt, are often dolomitic. The structure of Tertiary coral reefs on the Sinai peninsula, about the origin of which no doubt can be entertained, is actually identical with the structure of some Triassic dolomite I have examined here. I believe myself, for these, and many other reasons, justified in agreeing with Richthofen, &c., and in assuming that many Tyrolese Triassic limestones and dolomites are coralligenous.

The zones of the Mediterranean Trias differ altogether from the Trias in Germany. Other limits must here be recognized. In this respect I follow Mojsisovics. In the lowest Triassic no coral reefs are observed; also in the zones corresponding to the German Muschelkalk, we find, although these deposits are usually calcareous, no reefs of any size. It is in the zones distinguished as Buchensteiner, Wengener, and Cassianer-Schichten, that the great massy, unstratified reefs of South Tyrol are met with. The Upper Triassic layers, known as Raiblerschichten and Dachsteinkalk, are in South Tyrol mostly stratified, and in my opinion sedimentary, not coralligenous. Numerous faults traverse South Tyrol and break up the whole, only slightly folded Triassic system, into numerous plates (*Tafeln*) which are elevated on one (usually the northern) and depressed on the other (usually the southern) side. Liassic, Jurassic, and even Cretaceous layers rest on the depressed margins of the plates. Elsewhere these have been entirely removed, and the underlying Triassic reefs, capped with remains of sedimentary Dachsteinkalk and Raiblerschichten, have been laid bare. On the elevated margins along the faults also the Triassic layers have been removed. It is clear that somewhere between the subsided margin of the plates covered with Jurassic deposit, and the elevated margin, laid bare down to the Dyas, there must be places where the erosion has just reached the middle Triassic reefs. Here it is that we find parts of natural reef surfaces exposed.

The Lower Triassic Werfener Schichten are divided from the middle Triassic by deposits of gypsum, which show that the sea receded after the Werfener Schichten had been formed. Afterwards the sea returned, and it is clear that it must have risen at least as high as the later layers are thick, whilst or before they were deposited. The fossils in the sedimentary deposits between the masses of structureless dolomite show that the depth increased during their deposition. These sedimentary deposits alternate, as above stated, at their margins with the dolomite at the foot of the reef precipices. Therefore the dolomite also was formed during the rise of the water, for which I shall henceforth use the more exact expression introduced by Suess—positive shifting of the coast-line.

The dolomite masses are coral reefs. They have been formed during a period of positive shifting of coast-line, therefore we may assume that the high coral reefs now living and growing may have attained their astonishing altitude over the sea-bottom during a period of positive coast-line shifting. The dolomite masses of the Wengener and Cassianer period show no trace of stratification, such as is observed in the lower Muschelkalk and in the higher Dachsteinkalk. Moreover the dolomite has the same perfectly uniform structure from top to bottom; and on the vertical cliffs produced by erosion, which are often several thousand feet high, no trace of a stratified basal part can be detected. Everywhere the massy dolomite rests on discordant older layers, or—as usually at the reef margin—on the simultaneously deposited deep-sea sediment.

I will now proceed to utilize the facts here outlined in criticizing the discussion between the advocates of Darwin's and Dr. Murray's theories concerning the origin of coral reefs. I may say at once that all the phenomena observed in the dolomites of South Tyrol corroborate Darwin's subsidence theory, whereas they do not find explanation if we accept Dr. Murray's. It is the latter, therefore, which requires a closer examination.

Dr. Murray says that on slight elevations of the sea-bottom calcareous sediment accumulates, whereas in the greater surrounding depths this is not the case in consequence of the increase of dissolving power of sea-water with increasing depth. I accept this, and I believe that the caps of stratified Dachsteinkalk on the summits of many of the middle Triassic reefs in South Tyrol have been formed in this manner after the growth of the reefs had terminated. The positive coast-line shifting led to a horizontal extension of the Triassic Mediterranean northward, and a junction with colder northern seas, which caused a cooling of the water in the bay of South Tyrol, and thereby terminated the existence of reef-building corals. The positive coast-line shifting continued, and during its progress the Dachsteinkalk was deposited on the summits of the reefs, whilst the intermediate deeper spaces were left free from calcareous deposit—in accordance with Dr. Murray's view. There is, however, as above stated, nowhere a trace of stratified calcareous sediment forming a *basement* or nucleus to any one of the dolomite masses.

Dr. Murray then goes on to say that, as soon as the accumulating calcareous sediment has reached the region of coral growth, say the 20-fathom line, corals will grow on it, and an isolated atoll rising precipitously, perhaps ten thousand feet from the sea-bottom, will be formed. Against this it must be objected that the soft Globigerina ooze, which forms the whole of the atoll-peak, with the exception of an insignificant coral cap, could never attain such precipitous slopes as the atolls usually have. A slope of 45° or more could never be formed. The fossil deposits of this kind observed in South Tyrol (*Uebergussschichten* of Mojsisovics) nowhere terminate abruptly like the reefs. Neither is a slope of this kind anywhere observed in the region of the Dachsteinkalk.

Then, according to Dr. Murray, an atoll is formed by the solution of the lime in the centre of the reef. Although the Oolithes discovered in reef regions by Walther show clearly enough that there cannot be any solution other than what is compensated by redeposition, in any enclosed lagoon, I would like to draw attention to the logical discrepancy in this part of Dr. Murray's theory. First, a limestone cone is built, because the lime is deposited more rapidly than it can be dissolved. Then a lagoon is formed because the solution exceeds the accumulation, and this on the same spot, in still shallower and less powerfully dissolving water, and in spite of the relative stagnancy of the water in the lagoon and the limestone material, which is continually washed into the lagoon from the parts of the surrounding reef, which lie above the level of the sea. I think that gives the *coup de grâce* to Dr. Murray's atoll theory.

There remains yet something to be said on his ideas concerning the lateral growth of reefs. There can be no doubt that there is such lateral growth, and that the band of living coral on the reef margin can advance towards deeper water on a basis of coral fragments which have fallen from the outward growing face of the reef. As the corals near the surface grow more rapidly than those further down, the advancing reef face must ever tend to become overhanging; parts of the living coral must therefore frequently break off, fall down, and accumulate below. But there is a limit to this lateral growth which restricts it so considerably that it will by no means explain the formation of masses of dolomite 4000 feet thick, like the Cimon della Pala, for instance; and far less will it enable an atoll rising 10,000 feet or more from the bottom of the sea to extend horizontally. The amount of material *available* for the formation of a basis whereon the laterally growing corals may find footing is limited, and grows only in proportion to the circumference of a reef. The amount of material *required* for this purpose grows in a much more rapid proportion because it has to cover the surface of the growing cone.

Take a simple case. An ordinary straight fringing reef advances on a bottom of 10° inclination straight outward. At a distance of 280 metres from the shore a depth of 50 metres is reached. Further lateral growth is only possible on a talus of coral *débris*. 560 metres from the shore the depth is 100 metres. If the talus will lie at an angle of 45°, an amount of 50 cubic metres extended over a surface of 71 square metres will be necessary for the advance of each 1 metre's length of reef a distance of 1 metre. This talus is contributed from 50 square metres of growing coral (vertical reef face). At a distance of 5600 metres from the shore the depth will be 1000 metres. Every 50 square metres of growing reef face will have to furnish 950 cubic metres of material to enable the reef to advance 1 metre. These 950 cubic metres will be distributed over a surface of 1350 square metres. In

other words, the reef will advance nineteen times as slowly as it does 560 metres from the shore, whilst the surface which is exposed to the dissolving effect of the sea-water has also increased nineteen-fold. Where an ocean-current strikes such an incline, no Globigerina ooze can be deposited on it, and here the dissolving action of sea-water will balance the accumulation of coral *débris* long before such a height—of 1000 metres—is attained. It is clear that as soon as such equilibrium is reached there is a limit to the further extension of the reef in that direction. On the opposite side, however, where ooze will accumulate and protect the advancing reef from solution, such advance would be possible, but on that side the growth of coral is notoriously slow. Certainly, when the foot of the reef has advanced to depths below the zone of protecting Globigerina ooze no further lateral growth in *any* direction will be possible, and on the whole I should not think that lateral growth can play any considerable part in the formation of great reefs. Only positive coast-line shifting has such a result. In places where there is no such coast-line shifting (Gulf of Suez) the reefs are exceedingly small and insignificant.

Although therefore lateral growth no doubt does take place, it is not the actual cause of the formation of the great coral reefs.

We must, I think, revert to Darwin's subsidence theory, which is equally proved by the untenability of the hypothesis suggested for the purpose of superseding it, and by the direct evidence of the structure of the Triassic reefs in the Eastern Alps, which have actually attained their immense thickness during a period of positive shifting of the coast-line. R. von Lendenfeld.

Innsbruck.

Slugs and Thorns.

In Nature, vol. xli. p. 393, I pointed out that thorns might not always be a protection from snails and slugs, since they were found in the stomach of a European slug, *Parmacella*. In further confirmation of this view, I have to-day dissected a number of sharp, straight, reddish-brown thorns, over a millimetre long, from the intestinal tract of *Ariolimax niger*, var. nov. *maculatus*, a slug of rather doubtful affinities (possibly referable to *A. andersoni*), received from Dr. J. G. Cooper, who found it under drift-wood at Haywards, California. It is curious that the thorns do not pierce the intestine, but they appear to cause no inconvenience. T. D. A. Cockerell.

West Cliff, Colorado, April 21.

COMETS OF SHORT PERIOD.

It is now generally accepted that the periodic comets of our system did not originate in it, but are bodies captured from outer space by one of the planets, the parabolic orbits in which they approached the system being transformed into elliptical ones. On account of the perturbing action of Jupiter, however, the orbits of short-period comets are liable to considerable modifications, and it is practically impossible to identify two apparitions of the same comet without laborious computations of the perturbations which it must have been subjected to between the two epochs. But even such computations may lead to a negative result, for frequently comets quite distinct have elements very much alike, probably because they are parts of an old comet travelling along the same orbit at greater or less intervals.

In some recent investigations on the capture theory of comets (*Bulletin Astronomique*, June and July 1889), M. Tisserand developed a relation that might be employed to determine the possibility of identity of comets whose elliptical elements are known. This criterion depends upon the fact that the velocity of a body revolving round a central one is the same for equal radius-vectors. In the case of a comet having a parabolic orbit coming under the influence of a planet, the latter plays the part of the central body, and the relative velocity of the comet with reference to it will be the same at the point of entry into the sphere of attraction as at the point of departure from it, the one point being in the old orbit, the other in the new one. If two comets are identical, their velocities with reference to the perturbing planet will be the same at these points.[1]

M. L. Schulhof has discussed the possibility of identity of several comets by means of M. Tisserand's formula (*Bull. Astr.*, November and December 1889, and *Astr. Nach.*, 2964), and the following tables contain the values of n found for those having periods from 3·3 to 8·8 years. In the first table, the comets whose periods are well known are given; in the second, those having uncertain periods. Comets which have undergone strong perturbations since discovery, and those for which perturbations prior to the first known apparition have been calculated, are given more than once, and the year indicated for which the elements are found. The symbols used have their usual signification, and l is the longitude of the comet at the point of nearest approach to Jupiter.

Comets of Known Period.

Name of Comet.	Elements of Orbit. n	π	☊	i	e	a	l
		°	°	°			°
1. Denning, V. 1881 ...	...0·414	19	66	7	0·83	4·23	223
2. Brorsen, 1842 ...	...0·466	112	104	46	0·76	2·99	284
,, 1890 ...	...0·475	116	101	29	0·81	3·10	284
3. Finlay, VII. 1886 ...	...0·483	8	52	3	0·72	3·54	205
4. Lexell, 1767	...0·483	26	352	33	0·33	4·45	163
,, 1770	...0·486	356	132	2	0·79	3·16	184
,, 1779	...0·478	159	178	18	0·91	60·10	184
5. Biela, 1772	...0·486	110	257	17	0·72	3·58	269
,, 1852	.. 0·482	109	246	13	0·76	3·53	269
6. Wolf, 1868	.. 0·492	6	208	29	0·39	4·18	211
,, III. 1884 ...	...0·497	19	206	25	0·56	3·58	210
7. D'Arrest, 1851 ...	...0·503	323	148	14	0·66	3·44	153
,, 1883 ...	...0·504	319	146	16	0·63	3·55	153
8. Faye, 1814	...0·509	55	225	7	0·56	3·83	212
,, 1880	...0·507	51	210	11	0·55	3·85	208
9. Winnecke, 1809 ...	...0·509	274	113	10	0·75	3·21	107
,, 1886 ...	...0·509	276	104	14	0·73	3·23	109
10. Tempel$_1$, 1869 ...	...0 527	43	297	5	0·66	3·11	223
11. Brooks, 1885	...0·531	203	179	8	0·39	8·99	185
,, V. 1889 ...	...0·530	1	18	6	0·47	3·68	185
12. De Vico, 1678 ...	...0·542	323	163	3	0·63	3·07	143
,, I. 1844 ...	...0·537	343	64	3	0·62	3·10	163
13. Barnard, II. 1884 ...	...0·556	306	5	5	0·58	3·08	126
14. Tempel$_2$, 1873 ...	...0·562	306	121	13	0·55	3·00	126
15. Tempel$_1$, 1856 ...	...0·591	236	103	6	0 53	3·13	56
,, 1885	.. 0·589	241	72	11	3·41	3·49	61
16. Encke, 1795	...0·591	157	335	14	0·85	2·21	335

Comets of Uncertain Period.

1. Comet of 1585 ...	...0·484	10	38	5	0·70	3·61	213
2. Grischau, I. 1743 ...	...0·525	93	87	2	0·72	3·09	271
3. Helfenzrieda, II. 1766	...0·493	251	74	8	0·86	2·93	80
4. Pigott, 1783	...0·473	50	56	45	0·55	3·26	233
5. Blainpain, IV. 1819	...0·517	68	78	9	0·71	3·11	248
6. Tuttle, III. 1858 ...	...0·527	201	175	20	0·67	3·52	357
7. Coggia, VII. 1873	...0·484	86	249	26	0·76	3·19	255
8. Brooks, IV. 1886 ...	...0·553	230	53	13	0·61	3·41	54
9. Swift, VI. 1889 ...	.. 0·462	40	330	10	0·68	4·27	189

The value of n therefore found by the formula given is almost constant for the 21 known short-period comets, being contained within the limits 0·41 for Denning's comet, and 0·59 for Encke's and Tempel's comets.

It will also be seen that only five comets have their minimum distance to Jupiter's orbit between $l = 284°$ and $l = 112°$, while twelve have the point of nearest approach between $l = 153°$ and $l = 233°$. This unequal distribution along the ecliptic cannot be accidental, and

[1] M. Tisserand expressed the criterion very approximately by the formula—

$$\frac{1}{a_1} - \frac{1}{a_2} = \frac{2\sqrt{A}}{R^2} (\cos i_2\sqrt{p_2} - \cos i_1\sqrt{p_1}),$$

where a_1, a_2, p_1, p_2, i_1, i_2, are the semi-major axes, parameters, and inclinations of the old and new orbits; A and R the planet's semi-major axis and radius-vector at the point of nearest approach. This relation may be divided up into two parts, having the form—

$$n = \frac{1}{a} + \frac{2\sqrt{A}}{R} \cos i\sqrt{p}.$$

is in favour of the capture of comets by Jupiter. In fact, the accumulation of these points about $l = 192°$, which is the longitude of Jupiter's aphelion, may be partially explained by the circumstance that at this point Jupiter as well as the comets move more slowly than at perihelion, hence the sphere of attraction of the planet is sensibly extended, and its action exercised for a longer time on bodies moving in its neighbourhood.

The similarity of the elements of many comets is very manifest from the above tables, and M. Schulhof discusses in detail the probable identity of such. During last year Mr. Chandler brought forward evidence that Brooks's comet, V. 1889, was identical with the celebrated lost comet of Lexell, and the latter comet has also been asserted to be identical with that of Finlay, VII. 1886, to which it presents many points of resemblance. It is shown in the discussion that, by computing the orbit of Brooks's comet before 1886, the question of its identity with that of Lexell may be settled, while an extended calculation of the perturbations undergone by Finlay's comet indicates that it could not have been near Jupiter in 1779, and hence it is probably not identical with Lexell's comet. The elements of Finlay's comet are also very similar to those of Vico's comet, I. 1844. In order that the two may be identical, it is necessary that Mars should have augmented the period of Vico's comet by almost two years between 1844 and 1886.

The elements of Denning's comet present a certain analogy with Pigott's comet, but the two are shown to be certainly distinct.

Blainpain's comet, 1819, and that of Grischau, 1743, are most probably identical, and the elements of both these present a strong analogy with those of Tempel's comet, so that it is not impossible that this last comet is identical with the other two, or at least with Grischau's comet.

Whether Coggia's comet, VII. 1873, is identical with Pons's comet, I. 1818, is not settled. It is interesting to remark, however, that the value of $n_j = 0·484$, corresponding to a period of 5·67 years for Pons's comet, is exactly equal to that of Biela's comet. This, therefore, appears to confirm the opinion that both Biela's comet and that of Coggia represent the *débris* of an old comet, for, in the case of the division of the materials forming a comet, n_j may be regarded as constant for each of the portions detached.

To decide the question of the identity of Winnecke's with Helfenzrieda's comet, the perturbations undergone by the former towards 1800, when it approached very near to Jupiter, have been found, and it is shown that for the identity to be possible it must have moved faster in its orbit before 1800 than it does now—that is, the period must have been shorter.

This discussion of cometary identities, coupled with M. Tisserand's elaborate investigations, supports strongly Laplace's hypothesis that comets coming from stellar space, and moving in parabolic orbits, only become periodic by the perturbing action of one of the planets. This theory best explains the origin of the families of comets that cluster round the major planets, and the well-established fact of the disintegration of certain periodic comets, as Biela's in 1846, and Brooks's in 1889. Indeed, such disintegration must eventually happen to all periodic comets.

Richard A. Gregory.

THE JOURNAL OF MORPHOLOGY.[1]

THE issues before us constitute the first three parts of the third volume of this excellent journal. They contain 502 pages with numerous plates and a vast number of woodcuts. The chief contribution in the June number is that of Dr. Macmurrich, on "The Actinaria of the Bahama Islands." The author's material was collected during the summer of 1887, in connection with the work of the marine zoological station of the Johns Hopkins University. The monograph forms a very valuable contribution to the literature of the Actiniæ, and it may be regarded as a first step towards a rational comprehension of the tropical members of this group. It is the more welcome to us at the present juncture, in view of the revision of our native Actiniæ now progressing, in the hands of Haddon and his pupils; and we cannot but regard the excellent results obtained by Macmurrich as furnishing an additional argument in favour of the advantages of a peripatetic University marine laboratory, as compared with one of fixed habitat. Fourteen species are described, of which three are new. The illustrations are particularly good, and the following distributional conclusion is arrived at, viz. that

> "so far as the Actinaria are concerned two great areas of distribution can be defined,—the Indo-Pacific, including the Indian and Pacific Oceans and the seas connected with them, such as the Red Sea; and the Atlantic, including in this the Mediterranean. The Caribbean region of the Atlantic is, however, to be separated from the Atlantic region and united with the Indo-Pacific, the relationships of its Actinaria being very certainly with those of that region."

Of the remaining papers in the first part, two are by Dr. R. W. Shufeldt, and they treat respectively of "The Comparative Osteology of the Families of the North American Passeres," and of "The Anatomy of *Speotyto cunicularia hypogæa*." Both communications are written and illustrated in that peculiar style for which their author is notorious. In the first-named paper the author reverts to his recently expressed belief in a near kinship between the swifts and swallows, but he adds nothing of fresh interest in this debated question. His papers bear the mark of honest work, and we wish them a favourable reception.

The last communication is one of 8–9 pages upon the "Variation of the Spinal Nerves in the Caudal Region of the Domestic Pigeon," by J. I. Peck. Although short, it is the outcome of a laborious investigation instituted to ascertain whether the spinal nerves vary in the same ratio as the caudal vertebræ, "or whether they remain constant in number and position of exit from the vertebral canal, without reference to the number of vertebræ themselves." One very interesting result of the author's investigation is the discovery that the coccyx does not diminish in length proportionate to the increase in number of the free caudal vertebræ—on the contrary, it is longest where the said vertebræ are most numerous; therefore, the detachment of the supernumerary vertebræ from the pygostyle would appear to be due to influences at first productive of a lengthening of the entire caudal region. The interest of this topic is vastly increased, on reflection that the assiduity of a Parker has shown us that our swans and ducklings are the bearers of a tail potentially longer than that of the Saururian, Archæopteryx.

In the September issue there are two papers, and each is a valuable monograph of its kind. That which will command most attention is the thesis by Prof. Cope upon "The Mechanical Causes of the Development of the Hard Parts of the Mammalia." To this subject there are devoted 150 pages, 5 plates, and close upon 100 most admirable woodcuts.[1] The paper is, for the most part, an elaborated *résumé* of the author's earlier and scattered contributions upon the subject under discussion; and with

[1] *The Journal of Morphology*, June to December 1889. (Boston: Ginn and Co.)

[1] We wish we could see this author's voluminous treatises invariably illustrated in a manner similarly befitting their contents. We cannot refrain from comparing the one under review with that on the "Batrachia of North America" recently published under the auspices of the United States National Museum. The illustrations in this are as poor as those referred to above are excellent: carelessly drawn, badly planned, miserably lettered, and in places misleading (if not inaccurate), they "illustrate though they hardly adorn" the text to which they are appended, while they render a large portion of the same of little or no avail for working purposes.

these he has incorporated the allied work and generalizations of Ryder and other collaborateurs, the whole being woven into a connected argument. The author declares at the outset that he is the more convinced "that it is the habit that has given rise to the structures of animals, and not the structures which have forced animals to adopt their special habits," while he sets himself to discover, "in the light of the descent traced by palæontology, the mechanical causes for the existence of the salient characters of the skeleton and dentition of the Mammalia." The paper abounds in suggestive and ingenious passages, and the author sums up his conclusions in the words :—

"The general law which we may derive from the preceding evidence is, that in biological growth, as in ordinary mechanics, identical causes produce identical results."

We have, in all, that which savours of rank Lamarckism ; and the assiduous author of the remarks we have cited is (as our readers have lately been made aware), clearly, no believer in the non-transmission of acquired characters. He asserts that

"since the modifications acquired by use during life are necessarily useful, it seems that, according to the post-Darwinians, the only way of acquiring useful variations known to us, is excluded from the process of organic evolution ;"

and further, that

"were this hypothesis true, there would have been no evolution."

Again, he writes,

"in spite of Weismann's theory to the contrary, so long as the germ-plasma is subject to nutrition, it is subject to influences during the adult life of an animal, and it would be an exception to all other tissues were it not so."

The second and last paper in the September issue is by W. M. Wheeler, upon "The Embryology of *Blatta germanica* and *Doryphora decemlineata*." It is illustrated by seven exquisite plates, which, we are glad to note, are of native (American) origin. In testimony to the thoroughness of the author's work, it need only be said that he professes to be able to tell "just what position any oöthecal egg held in the ovary, or just what position any egg in the ovary will hold in the capsule." Evidence of direct cell-division is adduced, and the author's observations under this head have a most important bearing upon the allied researches of Carnoy. The author records the discovery of the very early appearance and paired arrangement of the Malpighian tubes, and he regards the facts to which he alludes as indicating "that at one time they opened on the surface of the body, and that their orifices were subsequently carried in by a deepening of the proctodæal invagination," and that "probably these tubes in insects are homologous with the anal tubes of *Echiurus* larva, which are modified segmental organs." Gegenbaur, as is well known, long ago postulated such an origin for the excretory apparatus of the Insecta, and Beddard has lately substantiated his belief, on argument from analogy to the Chætopod worms, in which he finds (*Acanthodrilus*) evidences of such an inturning of undoubted nephridia. The author has investigated, among other things, the orientation in oökinesis, and he draws the conclusion that "the force of gravitation has no perceptible effect on the development of the eggs of *Blatta*, but that these highly differentiated eggs, utterly unable to revolve in their envelopes, like the eggs of birds and frogs, have their constituents prearranged, and the paths of their nuclei predetermined, with reference to the parts of the embryo."

In the December number of the journal, Dr. Shufeldt communicates a detailed work "On the Position of *Chamæa* in the System" (28 pp.). In this welcome addition to his previous papers on the smaller Passerines, the author gives a short description of the pterylosis, visceral anatomy, and myology of the bird, and deals at greater length with its skeletal anatomy ; he concludes that the Wren Tit is allied to the Bush Tits (*Psaltriparus*) rather than to the true Wrens. Dr. G. Baur rushes into print with two short notes, "On the Morphology of the Vertebrate Skull," and "On the Morphology of Ribs and the Fate of the Actinosts of the Median Fins of Fishes," respectively. In the twelve short pages devoted to the two, the conclusions are arrived at that "the doctrine of the 'otic' bones established by Prof. Huxley twenty-five years ago, and held since that time by nearly all morphologists, is incorrect," and that "the elements of the anal and caudal fins of fishes . . . are represented by the chevron bones of the tail vertebræ, which are the partial homologues of the actinosts." The author's proposal to revive the term "petrosal" for that element now known as the "pro-otic" is especially to be deplored. These notes, although not wholly destitute of merit, are premature. They deal with questions in morphology which have taxed the powers of the greatest anatomists, and which are not to be summarily disposed of in a succession of scrappy communications, any one of which may more or less completely contradict its predecessors. If, in respect to these leading topics, every intelligent inquirer is thus to dogmatically foist upon the public his musings upon facts observed in individual specimens, to say nothing of others pitchforked in second-hand, and which he has therefore not observed at all, what is to become of our already too voluminous literature? We cannot allow to pass unnoticed the contraction of Theromorpha to "Theromora"; life is too short for actions of this kind, even if etymologically justifiable. The remaining contribution is by E. B. Wilson, upon "The Embryology of the Earthworm" (55 pp.). It is an extended account of investigations previously announced in an earlier issue of the same journal ; and it is, moreover, very welcome now that current research is revealing in the earthworms an altogether unexpected and intensely interesting range of modification. The author's results raise momentous questions affecting the most important of current morphological beliefs ; while largely confirmatory of the recent work of Kleinenberg, they run counter to the same in matters of vital importance, and interest in them is thereby enhanced. The most important topics dealt with are the origin of the mesoblast and the development and morphology of the head (prostomium). The author asserts that Kleinenberg was in error in his account of the origin of the first-named, and he criticizes those facts and deductions which lead him to reject the ordinary conception of the mesoblast as an embryonic layer: he attempts to show that the cerebral ganglia do not arise independently of the rest of the nervous system, and that the cavity of the prostomium is from the first unpaired. These and other lines of investigation have led him to a reconsideration of the annelid Trochosphere, and that he regards as "a secondary larval form," which has "arisen from an elongated segmented ancestral form, . . . the head region or prostomium being enormously developed, . . . and the trunk region more or less reduced." The author confirms Kleinenberg's discovery of the remarkable "cleavage-pore," and suggests a probable significance for the same. He regards both muscular and glandular segments of the nephridia as ectoblastic in origin, and he adduces reason for suspecting that the Hirudinea may formerly have possessed setæ. The last-named is by no means the least suggestive point raised in this excellent paper, which fully realizes the expectations raised by its author in his preliminary note referred to.

We observe that the late publication of this journal, so conspicuous at the outset, is being persisted in. With respect to this, as concerning more than one of their scientific serials, our American brethren are establishing a dangerous precedent for which there is absolutely no excuse ; and it is with much dissatisfaction that we note the adoption of a similar course nearer home.

G. B. H.

NOTES.

THE first *conversazione* of the Royal Society will be held at the Society's Rooms, Burlington House, on Wednesday next, May 14.

THE Royal Geographical Society is to be congratulated on the brilliant reception accorded under its auspices to Mr. Stanley at the Albert Hall on Monday. All the arrangements had been made with the greatest care, and the proceedings were in every way most successful. No one who was fortunate enough to be present could fail to see how fully the English people recognize, and how warmly they appreciate, Mr. Stanley's achievements.

THE Chancellor of the Exchequer will receive a deputation on May 15 from the Marine Biological Association of the United Kingdom in reference to the Treasury grant in aid of that Society's investigations of the natural history of marine food-fishes. A large monograph on the common sole, illustrated by many coloured plates, will be among the evidences of work done which the Association will submit to Mr. Goschen. The Fishmongers' Company have recently raised their contribution to the funds of the Association from £200 to £400 a year.

ON Monday evening various questions as to the effects of the dog-muzzling order were addressed to Mr. Chaplin in the House of Commons. He said:—"The return of deaths from hydrophobia since the muzzling order came into force are not at present in the hands of the Board of Agriculture. But I am glad to say, with regard to rabies, that in every county which has been placed under the regulations, and in the country as a whole, there has been a marked diminution in the number of outbreaks since the passing of the order. For instance; in 1889, for the last two quarters of that year there were 133 cases in the third, and 81 cases in the fourth quarter reported to the Board. For the first quarter of the present year they have been reduced to 39, and for the month of April there have only been seven cases throughout England, as compared with 11 for March, 14 for February, and 14 for January of the present year. In the metropolis and the West Riding, although there has been a large diminution, cases are still of constant occurrence, and there have also been comparatively recent outbreaks in Hampshire and West Sussex, in which latter county a muzzling order has been imposed by the local authority. With regard to Lancashire and the home counties of Essex, Hertfordshire, Surrey, and Kent, so far as they are not included in the metropolitan district, no cases have been reported for a considerable period, and if the reports continue to be as favourable in the case of the home counties as they have been of late, I shall hope to be able to modify the order, if it is not suspended, at no distant time. I may be allowed to add, as it will be of interest to the public, that since the order has been enforced, of the rabid dogs seized in public places, nine were properly and securely muzzled, and were thus prevented from doing mischief."

PROF. G. J. ROMANES, F.R.S., has been elected President of the Sunday Society, in succession to Sir James D. Linton, P.R.I., and will deliver his Presidential address at a meeting to be held in London in June.

THE Pharmaceutical Society will hold a *conversazione* at its house on Tuesday evening, May 20.

THE German Ornithological Society will hold its annual meeting at Berlin from May 9 to 12.

M. C. W. ROSSET has arrived at Hamburg after having been absent in Egypt, Cochin China, and China for three years. He has made a most interesting scientific collection, which will be presented to the Ethnographical Museum of Berlin.

THE recent investigations of Dr. Rudolf Kœnig, of Paris, into the composition of musical sounds and the theory of *timbre* will form the subject of an important paper to be read on May 16, at the meeting of the Physical Society, by Prof. Silvanus P. Thompson. Dr. Kœnig is sending over to this country for exhibition on this occasion a number of his wave-sirens and other expensive and elaborate apparatus, by which he has demonstrated the points of his research. Amongst the apparatus are some special appliances for producing audible beat-tones by the interference of two notes, each of which is too shrill to be separately heard. Musicians, as is well known, have never taken cordially to the current theories of Helmholtz respecting overtones and their relation to the consonance or dissonance of intervals and chords. As Dr. Kœnig's investigations have carried matters to a point beyond the speculations of Helmholtz, and not altogether in accordance with them, the occasion promises to be of unusual interest. It is expected that Dr. Kœnig will himself be present at the meeting, which is to be held at 6 o'clock at the Physical Laboratory of the Science Schools, South Kensington.

AT the Royal Academy banquet on Saturday, Sir William Thomson responded for "Science." He spoke chiefly of the mutual obligations of science and art. Aërial perspective, he said, first became known to scientific men through the artist's practical knowledge, and the use made of it in every conceivable representation of light and darkness, of house interiors and exteriors.

THE Select Committee on the sweating system refer in their Report to the evidence submitted to them as to the incompleteness of the education of workmen. "The remedies suggested," says the Committee, "are, on the one hand, a renewal of the apprenticeship system; and, on the other, the promotion of a larger system of technical education. We think that the encouragement of technical education for all classes of artisans is more likely to prove an efficient remedy than a recurrence to the old system of apprenticeship."

IT is reported from the ruby mines of Burmah that a ruby weighing 304 carats has been found.

A PUBLIC library is to be established at Hyderabad, and the Nizam's Government has also decided to undertake an archæological survey of the State.

FATHER FRANCIS DENZA, the Director of the new Vatican Observatory, is sending a circular in English to the Observatories of all English-speaking places, asking them to exchange publications with his institution. The authorities of the Vatican Observatory, which "now revives under the protection of His Holiness, Pope Leo XIII.," are anxious that it may render great service to science. Hence they feel the necessity of entering into communication with every existing scientific establishment of a similar kind. Father Denza expresses a hope that the directors to whom he appeals will let him have all the past publications of their respective Observatories.

MR. W. C. MILLS, Secretary of the Archæological Society of New Comerstown, Ohio, has found a Palæolithic flint implement in the gravel of the glacial terrace which everywhere lines the valley of the Tuscarawas river. Mr. G. F. Wright, to whom the implement was submitted, went to see the spot where it was discovered, and contributes to the *Nation* an interesting paper on his researches and conclusions. At this spot the surface of the terrace is thirty-five feet above the flood-plain of the Tuscarawas. The implement was found by Mr. Mills himself in undisturbed strata, fifteen feet below the surface of the terrace; so that it is "connected, beyond question, with the period when the terrace itself was in process of deposition." Thus it adds "another witness to the fact that man was in the

valley of the Mississippi while the ice of the glacial period still lingered over a large part of its northern area." This is only the fifth locality in which similar discoveries have been made in America—the other places being Trenton, N.J.; Madisonville, Ohio; Medora, Ind.; and Little Falls, Minn.

At a recent meeting of the Washington Chemical Society, Dr. Thomas Taylor, of the United States Department of Agriculture, exhibited a new flash-light intended to take the place of several kinds which have proved highly dangerous. The composition, as described by *Science*, consists largely of charcoal made from the silky down of the milk-weed—a form of carbon which Dr. Taylor prefers to all others, because of its freedom from ash. A few grains of this composition placed on tissue-paper, and lighted by a "punk-match," produced a prompt and blinding flash. The paper on which the powder rested was not even scorched. The flash being instantaneous, the heat is not sufficient to ignite the most inflammable material on which the powder may rest. An inferior flash-light being used, with the same paper for a base, the paper at once caught fire. This was owing to the comparatively slow combustion of the chemicals used in the inferior grade. Dr. Taylor said the powder of his new flash-light would not explode either by concussion or by friction.

At the meeting of the French Meteorological Society on April 1, the President read a circular from the Minister of Public Instruction, with reference to the Congress of Scientific Societies to be held at the Sorbonne from May 27 to 31. The following questions to be discussed are those more particularly of interest to meteorologists:—The study of the mistral; earthquakes; researches on the presence of aqueous vapour in the air by astronomical and spectroscopic observations; comparison of the climates of the different parts of France; the causes which seem to induce a general decrease in the waters of the north of Africa, and a change of climate; to fix for certain localities of the Alps and Pyrenees the present superior limit of vegetation, and to study the variations which it has experienced at different epochs; the study of the periodical phenomena of vegetation, dates of budding, flowering, and maturation; coincidence of these epochs with that of the appearance of the principal species of insects injurious to agriculture.

The Meteorological Council have just published the monthly and annual results of the meteorological observations taken at the stations of the Royal Engineers and the Army Medical Department, for the years 1852–86, comprising thirty-three stations, in different parts of the world. In the year 1852, meteorological instruments were supplied to the principal foreign stations of the Royal Engineers, and the observations were continued till March 1862, when the instruments were transferred to the Army Medical Department, as directions had been given by the War Office for similar observations to be taken by the medical officers in the Army, wherever stationed. The observations were partially published by the Board of Ordnance and the War Office, but as it was pointed out in the *Meteorologische Zeitschrift* for March 1886, that it was "most desirable that this valuable store of observations, especially from stations for which hardly any other information for the period exists, should be worked up according to the modern requirements of the science, and then published," the Meteorological Council decided to undertake the work, and a large mass of original observations was handed over to that body. The result is the present volume of 261 + xiii. large quarto pages of carefully revised results for separate months and years. The combined results, for as many years as possible for each station, accompanied by a brief discussion of this valuable material, would no doubt be welcomed by meteorologists.

Dr. Dixon, Professor of Hygiene at the University of Pennsylvania, has been studying air and dust obtained in street cars. *Science* says he has found in them "the germs of many diseases, contagious and otherwise. Better ventilation and more effective cleansing are surely needed."

In the current number of the *Zoologist*, Mr. E. L. Mitford writes of the survival of the beaver in Western Europe. Some fifteen years ago he saw in the museum at Bayonne a very large white beaver, which had been killed in the Rhone. He was told that it was the last of its race found in Europe. But this year, being at Hyères, where there is a museum with a very fine collection of indigenous birds and quadrupeds, he found another fine specimen, colour light brown, measuring three feet from snout to end of tail. This was obtained about four or five years ago, and is one of several that were sent to M. Fiépi, a naturalist and taxidermist of Marseilles. They were taken in the Rhone at St. Meree, in the neighbourhood of Arles. M. Catal, the naturalist of the Hyères Museum, talking of the subject with Mr. Mitford, said that beavers were more numerous formerly on the Rhone; that the great floods of 1846 had destroyed a large number, and made them more easily captured; and that subsequent inundations had made them much rarer. They are still to be found on the Rhone and its affluents, the Gardon, the Durance, and the Isère below Valence, also lower down the Rhone at Arles, Beaucaire, and Taralcon. They seem to have abandoned their custom of building huts and dams; the race no longer being sufficiently numerous to live in communities, they now live in deep burrows. In 1827 a number of the huts of the beaver were found on the Elbe at its meeting with the Nuthe, near Magdeburg.

The Director of the Norwegian Geological Survey, Dr. Hans Reusch, has lately published a small geological map of the Scandinavian peninsula, Finland, and Denmark. It includes also representations of Greenland, Iceland, Spitzbergen, and the Faroe Islands. The Norwegian terms used in the explanation of the colours are translated into English. The publishers are H. Aschehoug and Co., Christiania.

A work entitled "Dogs, Jackals, Wolves, and Foxes: a Monograph of the Canidæ," is being prepared by Mr. St. George Mivart, F.R.S. It will contain a description, with a plate drawn and coloured from nature, and often from life, of every species which the author thinks can fairly claim to be regarded as distinct, and also of various marked varieties of what he regards as probably one species. In addition to an account of the habits, geographical distribution, and life-history of each species, there will be given in an introduction, enriched with woodcuts, what the author deems a sufficient description of the anatomy of the group, of the structural relations of the Canidæ to other animals, their position in zoological classification, and the general relations they bear to the past and present history of this planet. The execution of the plates has been entrusted to Mr. J. G. Keulemans.

A new and most useful edition of the "Guide" to the exhibition galleries of the Department of Geology and Palæontology in the Natural History Museum, Cromwell Road, has been issued. In the preface, Dr. Henry Woodward explains that the publication of Mr. R. Lydekker's Museum Catalogues of the "Fossil Mammalia," parts i.–v. (1885–87), and the "Reptilia and Amphibia," parts i.–iv. (1888–89), has compelled the rearrangement of a great part of these collections, and changed the plan of the "Guide." Much additional information is given in the present edition, and the illustrations have been increased from 49 to 211. It has therefore been found necessary to subdivide the work into two parts. The first part deals with fossil mammals and birds, the second with fossil reptiles, fishes, and invertebrates.

Messrs. Neill and Co. have issued a volume giving a list of the contents of the first thirty-four volumes of the Transactions of the Royal Society of Edinburgh, with an index of authors and an index of subjects. The Society was founded in 1783, and published the first volume of its Transactions in 1789, adopting demy quarto as the size of the page. A slightly larger size has now been chosen, that the Society's Transactions may be uniform with those of the Royal Society. A new series may thus be said to have been begun. Messrs. Neill and Co., who have been printers to the Society since its foundation, present a copy of the volume to each member, "as a slight acknowledgment of their appreciation of the favour shown their firm for more than a century."

We have received the twentieth annual report of the Wellington College Natural Science Society. A good record of work in various departments is presented, and an account of some very interesting lectures is given in the "minutes of open meetings."

Some time ago the Japanese Minister of Education summoned a committee to discuss the system of building best adapted to withstand earthquakes. For the use of this committee, Prof. Milne, of Tokio, compiled a great quantity of information respecting building in earthquake countries. The various reports collected by him, with some original articles of his own, he has now brought together in a work which is printed as vol. xiv. of the Transactions of the Seismological Society of Japan. The compilation ought to be of great service to builders in countries where shocks of earthquake are frequently felt.

Messrs. Williams and Norgate have published a second edition of Mr. F. Howard Collins's "Epitome of the Synthetic Philosophy," which we recently reviewed. The work has been favourably received in America, and is being translated into French, German, and Russian.

A volume on the Paris Exhibition of 1889, by M. Henri de Parville, has just been published by M. J. Rothschild. It is pleasantly written, and illustrated by 700 vignettes.

The Geneva Society of Physics and Natural History has issued the second part of the thirtieth volume of its *Mémoires*. Besides the President's Report for 1888, a bibliographical bulletin, and a list of members, the volume contains papers on the movements of electrified bodies, by M. Ch. Cellérier; new or little-known locustides in the Museum of Geneva, by M. Alph. Pictet; on the flora of Paraguay, by MM. M. Micheli and R. Chadat; and on certain fossils of Japan, by MM. J. Brun and J. Tempère. The volume includes many illustrations.

Messrs. Macmillan and Bowes, Cambridge, have issued the first part of a Catalogue (No. 230) of books on the mathematics, pure and applied, containing many works of the old mathematicians, mathematical and astronomical journals, observations, &c., including many from the libraries of the late Arthur Buchheim, Fellow of New College, Oxford, and E. Temperley, Fellow of Queens' College, Cambridge.

A Catalogue of zoological and palæontological works has been issued by Messrs. Dulau and Co. It includes works on mammalia, and on anthropology and ethnography.

Prof. Everett sends us the following extract from the *East Anglian Daily Times*. It is by Mr. Herman Bidell, of Playford, who is well known in Suffolk, and is thoroughly competent to describe accurately phenomena he observes:—"I shall be glad to draw the attention of those interested in thunderstorms to a magnificent example of ruin by lightning we have in this village. The parish of Playford was visited on Saturday last (the 26th) by one of those volatile clouds heavily charged with electricity that so often remind us of the approach of summer. The tree struck stands about 300 yards north-east of the church, close by the footpath leading to Great Bealings, one of a row of 'old English' poplars running east and west. At the foot the tree is about $2\frac{1}{2}$ feet in diameter, tapering more or less regularly to a 10-inch diameter at the top. Here the electric fluid came in contact with the trunk some 40 feet from the ground. The two topmost branches are intact, but the bark is completely stripped from top to bottom, the southern half of the body being riven into matchwood. The storm came up from north-west, with a very light breeze, shifting right and left of north. The cloud was a dense dark blue—the effect, in part, of the sun in front—a detached mass of vapour with fringed edges, differing little, except in density and its proximity to the earth, from others which during the forenoon had floated over. At half-past one o'clock a few drops warned four or five men at work close to the tree to take shelter under a stack 200 yards off—a fortunate warning, for no sooner had the cloud drifted overhead than a blinding flash, accompanied by a terrific peal of thunder, left the tree a magnificent ruin, spread over not less than two acres of land, more or less covered with bark, branches, and riven trunk. One solid piece of $5\frac{1}{2}$ pounds was picked up 126 yards away from the tree. Other *débris* lies 70 yards in an opposite direction, and as an evidence of still more inconceivable force, small pieces of riven trunk or bark, some under half an ounce in weight, were found right in the face of the wind, nearly 60 yards from the tree. What force could have been applied to such light particles is beyond comprehension. Nothing that I have ever seen effected by lightning approaches this ruin. Larger trees have been shivered in this parish, but I never saw a tree completely barked all round, with one half literally riven into fibre, leaving the other half of the trunk a whitened stem, still standing as a forty-feet shaft to be seen a mile away. (The remnant is a conspicuous object in a north-westerly direction all the way from a point east of the old Kesgrave Schools for half a mile towards Ipswich on the Martlesham and Rushmere roads). The electric fluid left the tree at the foot, following the direction of the fence for about 15 or 20 feet, threw up a sod about a foot square, and there pierced the soil into the earth. Four hundred yards in a direct line north-east of the tree stands Playford Mount, the residence of Mr. Kemp-West, a commanding object in the landscape. Here some half-dozen of the fine glass plates in the front windows are shattered to atoms, the result, I apprehend, of concussion from the report of the explosion. I have never known this effect from the severest storm in the neighbourhood, but the thunderclap is described as terrific. The tree, as standing, is worth going to see, and, I will add, is of easy access from the road past the church."

The *Engineer* and *Engineering* for May 2 contain much information concerning the proposed great tower in London. The drawings, plans, and designs of the competing schemes are now being exhibited in the Drapers' Hall, Throgmorton Street, E.C. *Engineering* appears to think that, if the tower is to pay, it must be provided with some attractions to bring the people again and again; and if these attractions could be raised 200 feet or 500 feet above the smoke, they would be immensely increased. The country cousin and the conscientious sight-seer would go to the summit, but the first stage would detain the bulk of the visitors. *Engineering* also observes that, in reviewing the various designs, we must frankly admit that none excels the Eiffel Tower in beauty and grace. No fewer than eighty-six competitors have sent in designs.

An interesting paper upon cyanogen iodide, CNI, is communicated to the current number of the *Berichte* by Drs. Seubert and Pollard, of the University of Tübingen. On account, probably, of the extremely poisonous nature of this compound,

rendered even more dangerous by reason of its great volatility, little has hitherto been done towards completing its chemical history beyond a mere description of its more evident properties. Determinations either of its density or its melting-point appear never to have been attempted, and it was with the object of supplying these deficiencies that the work in the Tübingen Laboratory was undertaken. Everyone who has ever prepared this exceptionally beautiful substance for lecture or other purposes will remember the exquisite manner in which it sublimes, forming long, delicate, colourless, but very highly refractive needles, bridging over from side to side of the wide tube or flask in which the operation is performed. Often these elongated prisms attain the length of half a dozen inches or more, and frequently form an interlacing network among which may be seen perched here and there star-shaped or flower-like aggregates of the smaller crystals. Perhaps the most remarkable property of these crystals is to be found in the manner in which they re-sublime from one side of the vessel to the other according as their position is varied as regards the direction of the light which falls upon them. Drs. Seubert and Pollard prepared their specimens by the old method first used by Sir Humphrey Davy, the action of iodine upon mercuric cyanide, $Hg(CN)_2 + 2I_2 = HgI_2 + 2CNI$. About 10 grams of the finely-powdered and well-dried mixture of iodine and cyanide of quicksilver, in the proportion of one molecule of each so as to avoid the presence of much free iodine in the sublimate, was placed in a wide test-tube and interpersed with glass beads in order to render the mixture as porous as possible. The test-tube was then placed at the bottom of a wider glass tube closed at the lower end, and fitted at the upper with a calcium chloride drying tube to prevent access of moisture. The apparatus was then allowed to stand for about three days in a position where it could receive direct sunlight; at the end of this time the reaction was almost complete, the mixture being brilliant red from formation of mercuric iodide. The lower end of the tube was then placed in a hot water bath, when the iodide of cyanogen sublimed in the manner above described into the upper cooler part of the tube. In order to determine the melting point, small quantities were placed in capillary tubes and hermetically sealed, for if the upper end were left open, as is usually the case in taking a melting-point, the cyanide would simply volatilize without fusion. The melting-point was in this way found to be 146°·5 C., and the solidifying point 143°. The vapour-density was determined by Victor Meyer's method, and found to be 5·28, corresponding to the simple formula, CNI. The lowest temperature at which the substance becomes completely and rapidly converted to the gaseous condition appears to be about 250°. The iodide is therefore analogous to the simple bromide and chloride of cyanogen, CNBr and CNCl, and not to the triple polymers tri-cyanogen bromide and chloride, $C_3N_3Br_3$ and $C_3N_3Cl_3$.

The additions to the Zoological Society's Gardens during the past week include the Wild Boars (*Sus scrofa* jv.) bred in Scotland, presented by the Lord Hebrand Russell; a Ring-tailed Coati (*Nasua rufa* ♂) from the Argentine Republic, presented by Mr. R. E. Moore; a Louisianian Meadow Starling (*Sturnella ludoviciana* ♀) from North America, a Black-bellied Sand Grouse (*Pterocles arenarius* ♀) from India, presented by Mr. W. H. St. Quintin; four variegated Sheldrakes (*Tadorna variegata* ♂ ♂ ♂ ♂) from New Zealand, presented by Captain C. A. Findlay, R.N.R.; a Rhomb-marked Snake (*Psammophylax rhombeatus*) from South Africa, presented by Miss Harris; three Common Vipers (*Vipera berus*) from Sussex, presented by Dr. C. W. Cousens; a Green Lizard (*Lacerta viridis*), a Three-toed Skink (*Seps tridactylus*) from France, presented Mr. J. C. Warbury; a Sooty Phalanger (*Phalangista fuliginosa* ♂) from Australia, deposited; a Black-headed Lemur (*Lemur brunneus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on May 8 = 13h. 6m. 42s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 3321 (64 M) ...	—	—	12 51 19	+22 17
(2) G.C. 3572 (51 M) ..	—	—	13 25 13	+47 45
(3) 40 Comæ Ber. ...	6	Yellowish-red.	13 1 0	+23 12
(4) 91 Leonis...	5	Whitish-yellow.	11 30 48	+ 0 11
(5) α Virginis	1	White.	13 9 24	−10 36
(6) V Virginis	Var.	Yellow-red.	13 22 7	− 2 36

Remarks.

(1) The spectrum of this bright nebula does not appear to have been yet recorded. Smyth describes it as "a conspicuous nebula, magnificent in size and brightness." In the General Catalogue it is described as "a very remarkable object; very bright; very large; very much extended in the direction 120°; brightens in the middle to a small bright nucleus, which is possibly a double star." The nebula is in the constellation Coma Berenices, and is now very favourably situated for observations.

(2) This is the famous spiral nebula in Canes Venatici. The details of the nebula are so well-known that a full description is not necessary here. In all but the largest telescopes it simply appears as a double nebula with the two nebulosities running into each other, one of them being surrounded by a ring which is variable in brightness. Smyth likens this to a "ghost" of Saturn. According to Huggins the spectrum is continuous, but some parts were thought to be abnormally bright. Although the observations will not be easy, it is very important that the positions of such bright parts should be measured, if only approximately. In such cases as this it is probable that we shall find spectroscopic connecting links between the bright-line nebulæ and stars which are well advanced in condensation. Our knowledge of the relations between nebulæ and comets is also likely to be advanced in this way.

(3) This is a very fine star of Group II. The bands are very wide and dark, even in the ultra-blue, but especially in the red (Dunér). The star belongs to a late stage of the group, and may be usefully re-examined for further details.

(4) A star of the solar type (Vogel). The usual differential observations as to whether the star is increasing in temperature (Group III.), or decreasing (Group V.) are required. The spectra of this class of stars should be very carefully examined for remnants of the strong bands in the red, which characterize the later stages of Group II., and which are also seen in Aldebaran. If these are found, the star is obviously at an early stage of Group III. It has also become very important to determine whether there are any stars intermediate between stars like the sun and stars of Group VI., and in these more detailed observations this should be borne in mind. The carbon band near *b* will probably be the first to appear, and the presence or absence of traces of this band should be always noted. It is most likely to occur in stars of an orange or reddish tint.

(5) Spica has a spectrum of Group IV. The only observations likely to be of service are those comparing the thicknesses of such lines as *b*, E, and D with their thickness in other bright stars of the same group (*e.g.* α Lyræ). This will determine its relative temperature.

(6) This variable will reach a maximum about May 9. It ranges in magnitude from 8–9 at maximum, to <13 at minimum in a period of about 251 days. The spectrum is still doubtful; Gore writes it III.?

A. Fowler.

Structure of the Corona.—The Smithsonian Institution has had two plates prepared, containing nine photographs of the total eclipse of the sun of January 1, 1889, and distributed them amongst astronomers and others interested in solar physics. All the photographs have been reduced to a uniform diameter, and at Prof. Langley's request, Prof. Todd has contributed a descriptive note to accompany them. In the remarks on the structure of the corona it is noted:—

(1) The axis of symmetry of the corona does not coincide with the axis of revolution of the sun as determined from the solar

spots. The corona appears to be at least a triple phenomenon made up of—

(*a*) The polar rays, seen most prominently about the poles.

(*b*) The inner equatorial corona, the lower regions of which bear some resemblance to an outer solar atmosphere.

(*c*) The outer equatorial corona, consisting of the long streamers for the most part only visible to the naked eye.

(2) The polar corona consists of rays, straight or nearly so, and radial from neither the sun's centre nor the sun's poles. Rather they seem to radiate from areas the centres of which are adjacent to the sun's poles.

(3) The inner equatorial corona emits a large percentage of the total light of the corona; the streamers, however, are not generally so sharply defined as about the poles, and many of them appear to have a real curvature. Four large prominences are visible at about 35° of solar latitude, as if to suggest some connection between the protuberances and the corona.

(4) The equatorial streamers of the corona are very slightly curved, being convergent on the east side of the sun, and divergent on the west.

The fact of chief importance established appears to be the periodicity of the outer corona in a cycle probably of equal duration with that of the solar spots. The epoch of greatest extension of the equatorial corona appears to coincide very nearly with the epoch of minimum sun-spots.

Prof. Todd also directs attention to the most important points requiring elucidation, and throws out a few suggestions for future eclipse observations.

BROOKS'S COMET (*a* 1890).—The following ephemeris has been computed by Dr. Bidschof (*Astr. Nach.*, No. 2966), and is in continuation of that previously given (NATURE, vol. xli. p. 571):—

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
May 6	20 53 21	+34 9·6	0·2874	0·2596	2·60
10	46 41	37 44·1	0·2857	0·2449	2·80
14	38 9	41 28·5	0·2843	0·2314	3·00
18	27 13	45 20·2	0·2832	0·2195	3·18
22	13 16	49 14·9	0·2824	0·2096	3·34
26	19 55 28	53 6·2	0·2818	0·2022	3·47
30	32 54	56 46·1	0·2815	0·1976	3·55

The brightness at discovery (March 21) has been taken as unity.

DISCOVERY OF MINOR PLANETS.—Two more asteroids were discovered by Herr Palisa, at Vienna, on April 25, and observed independently by M. Charlois, at Nice, on the following night. The magnitudes of the planets are 13 and 12 respectively, and their numbers are (291) and (292). Prof. Krueger thinks that the latter is probably Scylla (*Astr. Nach.*, 2966).

THE INSTITUTION OF MECHANICAL ENGINEERS.

AN ordinary general meeting of the Institution of Mechanical Engineers was held on the Thursday and Friday of last week; the President, Mr. J. Tomlinson, in the chair.

The second meeting of the year is not generally looked on as of great importance, but it is a long time since we remember one of such meagre proportions in one respect as that with which we are now dealing, for there was only one paper on the agenda; that and the President's address constituted the whole programme. What the proceedings lacked in variety and amplitude was, however, fully compensated for in solid value. The one paper, Prof. Kennedy's, is full of valuable information, and Mr. Tomlinson's address came as a most welcome surprise to a good many. In the first place it was short, and, secondly, it was practical—two virtues which appeal strongly to engineers when there is talking to be done; but beyond that it was one of the most interesting Presidential addresses we have heard for many a day at any of the Engineering Societies. The reason for this is not far to seek. Mr. Tomlinson simply narrated his own experience in plain language, eschewing those ornamental tags of rhetoric which many people look on as essential when they have to speak in public; and as his experience extends back to a very interesting period of railway engineering, the address proved an exceptionally happy effort.

Mr. Tomlinson has been, as he said, a railway man all his life; and, indeed, he has been connected with the engineering departments of more than half a dozen railways, from the Stockton and Darlington up to the Metropolitan. His father was passenger superintendent to the former line. His recollection therefore carries him back to the very early days of the locomotive. His first knowledge extends to the year 1837, when he was employed at the works of Timothy Hackworth, of Shildon. Perhaps no better instance could be given of the simplicity of those Arcadian days than the fact that Hackworth was at once locomotive superintendent and contractor to the railway. Such a dual position might cause invidious remarks on the part of shareholders in the present day. Mr. Tomlinson remembered the three original locomotives placed on the Stockton and Darlington line. One of them, the *Locomotive*, now stands on a pedestal in front of the North Road Station at Darlington. The load for this engine was about 22 tons of empty waggons to draw up hill; whilst down the hill to Middlesborough the waggons loaded, weighing 64 tons, were drawn. The weight of the engine and two tenders loaded was about 15 tons. Unfortunately there was no record kept of the consumption of fuel, but Mr. Tomlinson used to help put the coal on the tender, and he estimates the quantity to have been 16 to 17 cwts. for 48 miles, or about 40 lbs. per engine mile; but it must be remembered that the gradient was all in favour of the load—in fact, the greater part of the fuel was consumed on the return journey of empty trucks. The cylinders were 10 inches in diameter by 24 inches stroke. The eccentrics had to be changed for back and forward gear by hand, the boiler pressures were 30 to 35 lbs. per square inch, and the pistons were packed with a spun-yarn gasket. As the cylinders were vertical there were necessarily no engine springs. There were no brakes, no water-gauge glass, no head or tail lamps, and no whistle. We have not space to follow Mr. Tomlinson in his interesting engineering reminiscences. Perhaps, since Mr. T. R. Crampton has gone, there is only one other engineer living who could give us such unique personal experience of early locomotive days. If so, that engineer is Mr. E. Woods, Past-President of the Institution of Civil Engineers.

Prof. Alexander Kennedy's paper constituted the second report of the Research Committee appointed by the Council of the Institution to investigate the Marine Engineering question. Within the last few years the Institution has made quite a special feature of these research committees, and we know of no better way in which it could carry out the object of its existence, and, at the same time, keep down the ever-growing financial surplus. The Research Committee on Friction and the Research Committee on Rivetting would have been of great service to engineers if only from the fact that they collected and put in concise form the knowledge already existing on the subjects; but they did more than this, for they made experiments of their own by which doubtful points were cleared up and new possibilities were suggested. The Marine Engine Committee are following the same useful course under the guidance of their Chairman, Prof. Kennedy, who, it may be remarked, gained his first experience as an engineer in the once celebrated Thames-side marine engineering establishment of the Dudgeons.

As we have said, this is the second report of the Committee, the first, which was read last year, being on the trials of the s.s. *Meteor*, a London and Edinburgh steamer of 692 registered tons. The vessels since then under trial, and dealt with in the second report, are the *Fusi Yama*, the *Colchester*, and the *Tartar*. The first is an ordinary trading vessel of 214·3 feet long b.p., 29·3 feet beam, 20·5 feet deep, and of 2175 tons displacement at trial draught. The trial run was from Gravesend to Portland. The engines are by Samuelson, of Hull, and had just been overhauled. They are of the ordinary two-cylinder compound type. The *Colchester* is the latest built vessel of the Great Eastern Railway on the Antwerp service. She is 281 feet long, 31 feet beam, and 15·2 deep. Her trial displacement was 1675 tons. She is a twin-screw ship, the engines being ordinary two-cylinder compounds. The trial run was from the Humber to Harwich, the engines having been overhauled in the former river. The *Tartar* was selected as an excellent example of modern economical engines in a cargo-carrying steamer—what is generally known as an "ocean tramp." She is 332 feet long, 38 feet wide, and 27 feet deep. Her displacement tonnage on trial was 2250 tons. She has triple compound engines of the three-crank type. The trial run was from the Thames to Portland. The vessel was light, so that the engines were working at very low power, and, in addition to this, bad weather was met

with on the voyage, so that the recording of results was much interfered with. It will be noticed that the figures bearing on the efficiency of the *Tartar's* boilers are not given in the table. The reason is that the coefficient based on the recorded data comes out so high that the boilers could hardly have been evaporating all the feed water pumped into them. In ordinary cases we should naturally attribute this to priming; but the power developed was so small that we hesitate to apply this solution in the present case. On the other hand, the phenomenon of excessive cylinder condensation would be induced by working a big engine at low power. We have not, however, sufficient data to enable anything positive to be advanced in this connection. We understand that in ordinary working the boilers show no sign of excessive priming, and the steam space is said to be ample. The *Meteor*, the first vessel experimented upon, is 261 feet long, 32·1 feet wide, and 19·3 feet deep. Her trial displacement was 2090 tons. The engines are of the triple compound type, with three cranks at equal angles.

It will be evident that we have not space to give details of the trials as set forth by Prof. Kennedy, and any fairly intelligible abstract is difficult to make. The paper itself is merely a record of facts—a most admirably arranged record we may say in passing—and each fact is so interdependent on others, that it is difficult to make a selection. We will, however, briefly state in the form of a table a few of the leading features and final results, referring those of our readers most interested in the subject to the report itself. We include the *Meteor*, as her record is necessary to make the matter complete.

	Name of vessel.			
	Meteor.	*Fusi Yama.*	*Colchester.*	*Tartar.*
Boiler pressure above atmosphere in pounds per square inch...	145·2	56·84	80·5	143·6
Vacuum in condenser below atmosphere in pounds per square inch... ...	12·17	12·48	12·49	12·9
Revolutions per minute	71·78	55·59	{86·0 / 87·1}	70·0
Total mean indicated horse-power	199·4	371·3	{1022·5 / 957·2}	1087·4
Coal burnt per square foot grate per hour	19·25	18·98	26·1	11·93
,, ,, ,, total heating surface per hour	0·602	0·437	0·987	0·367
,, ,, 1 horse-power per hour	2·01	2·66	2·90	1·77
Carbon value of coal	0·878	0·878	0·913	1·031
Feed water per square foot total heating surface per hour in pounds ...	4·49	3·48	7·39	4·13
,, ,, pound of coal	7·46	7·96	7·49	11·23
,, from and at 212° F.	8·21	8·87	8·53	13·06
,, per indicated horse-power per hour	14·98	21·17	21 73	19·83
Calorific value of 1 pound of coal as used in thermal units	12,770	12,760	13,280	14,995
Percentage of calorific value of fuel taken up by feed water	62·0	67·2	62·0	—
,, ,, ,, ,, carried away by furnace gases	21·9	23·5	28·0	22·1
,, ,, ,, ,, lost by imperfect combustion	3·6	0·0	1·3	0·0
,, ,, ,, ,, expended in evaporating moisture in coal	1·2	0 9	0·4	·0·0
,, ,, ,, ,, unaccounted for	11·3	8·4	8·3	—
Efficiency of boiler per cent.	62·0	67·2	62	—
,, ,, engine	16·1	11·2	10·7	11·5
,, ,, ,, and boiler combined	10·0	7 6	6·6	9·7

A discussion followed the reading of the paper, the most interesting feature of which was a description, by Mr. Willans, of a device he had used for investigating the effect of condensed steam in an engine cylinder. For this and other points in connection with the trials we must refer our readers to the Transactions of the Institution.

THE SCIENTIFIC INVESTIGATIONS OF THE FISHERY BOARD FOR SCOTLAND.[1]

WHATEVER may be wanting to Scotchmen in the way of Home Rule, they have no cause to complain of a want of Home Rule in their fisheries. The Fishery Board for Scotland is a complete and independent body, exercising complete jurisdiction over all the Scottish coasts, provided with an ample staff, and in receipt of a considerable amount of Government money. We learn from the introduction to the present Report that the scientific staff consists of three trained naturalists and an assistant naturalist, and besides these there is a Committee of eminent scientific men, including representatives from all the Scottish Universities. Finally, the Board has a steamer, the *Garland*, specially devoted to scientific investigations, and is able to make use of the fishery cruisers for the same purpose.

Under these favourable circumstances, and especially in virtue of the powers granted by the Sea Fisheries (Scotland) Amendment Act, 1885, the Scotch Fishery Board has exceptional opportunities for making extensive and continuous scientific investigations. The investigations for 1888 are embodied in the Report which is here dealt with. The Report is divided into three Sections. Section A is largely devoted to the experimental trawling of the *Garland* in the areas closed against beam-trawling, and to a number of statistical tables drawn up for the purpose of comparison with those experiments. This experimental trawling requires some explanation. The Act above-mentioned empowers the Scotch Fishery Board, under stated circumstances, to make by-laws for restricting or prohibiting, either entirely or partially, any method of fishing for sea fish within any specified area in any part of the sea adjoining Scotland, and within the exclusive fishery limits of the British Islands.

In accordance with the Act, by-laws were framed, prohibiting beam-trawling in districts which may roughly be described as the Firth of Forth, St. Andrew's Bay, and the Firth of Tay, and part of the sea off the coast of Aberdeenshire and Kincardineshire. This by-law came into force on April 5, 1886. Since that date the *Garland* has trawled periodically over certain definite stations within the prohibited areas, and the catches have been carefully tabulated, both as regards size and quantity. The object of the experiment is, of course, to study the effect of an enforced period of rest on the piscine fauna of the inclosed and adjacent areas, and to obtain information under the following heads:—(1) Whether the cessation of beam-trawling would cause any marked increase in (*a*) the number, (*b*) the size of trawl-fish within the closed areas. (2) Whether the closure would affect the catches of line-fishermen working in those areas. (3) Whether the closure would affect the catches of trawlers and other fishermen in adjacent areas. No fault can be found with the method of investigation, which is the only possible one under the circumstances; but, as might be expected, the results are influenced by a number of secondary causes which obscure the effect of prohibiting beam-trawling in the places mentioned. This may easily be seen by reference to the published accounts of the experiments. It was found in 1887, a year after the closure, that the average take of fish per "shot" was much greater than in the previous year in the closed

[1] "Seventh Annual Report of the Fishery Board for Scotland, being for the Year 1888." Part III., Scientific Investigations. Presented to both Houses of Parliament in pursuance of Act 45 and 46 Vict, cap. 78. (Edinburgh, 1889.)

areas, and that the increase was chiefly in flat fish, though also in round fish. At the same time, there was an increase in the take of all classes of fish in the free waters outside, but in general the increase in this case was in round fish, rather than in flat fish. So far, then, the experiment promised to show an immediate and most beneficial result. In 1888, however, the take of fish was very much diminished. The average number of fish of all kinds captured in the Firth of Forth per "shot" amounted to 211. In 1887 the corresponding average was 351, and in 1886, 251. There was also a considerable reduction in the average take in offshore waters, but the reduction was less than that in the closed area. Moreover, the proportional decrease of flat fish was greater than that of round fish in the closed waters, and this was more marked in the offshore waters. In St. Andrew's Bay there was, similarly, a great diminution of all kinds of fish, especially of flat fish; but outside, in the free sea, there was an increase in the flat fish and a great decrease of round fish. These negative and partly contradictory results were, without doubt, due to the exceptionally stormy weather in 1888. It shows, however, the great difficulty and complication attending fishery investigations. Nothing could seem to be more obvious than that, if trawling were prohibited in a certain area, less fish would be caught, and that their numbers would increase. The first results of the trawling experiments go to show that this is by no means necessarily the case, but that there are causes more powerful than beam-trawling which affect the numbers of fish in any season.

There are also statistics showing the relative amounts of fish caught by line in restricted and unrestricted areas—that is to say, areas where beam-trawling is prohibited and where it is permitted. These statistics show an increase in the weight of fish caught by line has taken place in 1888, in both areas, but that it is proportionally larger in the unrestricted than in the restricted areas. The increase is not due to a larger number of boats and men engaged in fishing, for these have actually decreased. The statistics of line-fishing are certainly curiously contradictory to those of beam-trawling, for whereas, in 1888, the latter mode of fishing showed a decrease of flat fish in closed areas, the line-fishing showed an increase of flat fish.

It is really impossible to draw any conclusions from statistics extending over so few years. After ten years of work we shall be in a better position to judge the result of the experiment of closing certain inshore waters against trawlers. So far, it must be confessed that no case whatever has been made out against them, and the line fishermen seem to be quite as efficient in depopulating a district. From the way in which the summaries of the statistics are written, the Fishery Board may be suspected of an unconscious leaning towards the interests of the line-fishermen.

No fewer than 129 pages are devoted to the statistical tables referred to.

A very interesting Report is given in Section B (biological investigations) by Prof. Ewart on the spawning of British marine food-fishes. Space forbids a detailed criticism of this Report, but it is definite and satisfactory, and shows that, contrary to the common belief, the majority of British food-fishes do not come inshore to spawn, but at the spawning season they congregate in shoals in deeper waters. This Report is followed by a paper on the food of fishes, by Mr. Ramsay Smith. The greater part of the observations and records necessary for this work were carried out by Mr. Thomas Scott, who is a veritable giant in practical work at sea. The paper on the pelagic fauna of the Bay of St. Andrew's, by Prof. McIntosh, may be considered as complementary to Mr. Ramsay Smith's paper, since the pelagic organisms are considered from the point of view of food for adult and larval fish. Prof. McIntosh's paper, giving a record of all the pelagic organisms observed throughout the year, is a thorough and important contribution to our knowledge of the subject, and has a high practical value, especially that part of it relating to fish ova and larvæ. At the same time, it may be questioned whether the subject of fish food is not dragged in a little too much. Is it perfectly ingenuous to give a series of beautifully-coloured drawings of the metamorphoses of Actinotrocha, and to label them "Pelagic fish food"?

The descriptions of, and suggestions about, the mussel and clam beds are of obvious practical interest, and Dr. Edington's paper on the Saprolegnia of the salmon disease gives promise of a wide extension of our knowledge of a difficult subject.

The Report concludes with a careful record of physical observations made in the North Sea. The value of the physical work of the Board would be much enhanced if arrangements could be made for taking daily observations at definite stations around the Scotch coast. Such observing stations have been established by the United States Fish Commission and by the German Commission for the Scientific Investigation of German Seas, and have been fruitful of results.

The Fishery Board, it may be noticed, is only engaged in one *experiment*—that of closing certain areas against beam-trawling. The remainder of the work is in the preliminary stage of *inquiry*. In the earlier stages of fishery investigation, a large amount of biological and physical inquiry into the natural conditions of the sea is absolutely necessary, as a guide for future experiments upon marine organisms. To those who do not consider the matter attentively, these investigations may seem useless and superfluous, but they are not. It must be observed, however, that these inquiries are not an end in themselves, as in philosophical biology, but must be undertaken solely with the view of applying the experience gained to future attempts to solve the fishery problem. For example, an inquiry into the food of the different species of fishes of a district need only be made once; it is sufficient for practical purposes to know what they do generally eat, without inquiring what they may eat in exceptional circumstances. An inquiry into the relations of pelagic organisms is most useful as a guide to the life-conditions and food of fish larva and certain adult fish, but a great deal of strictly scientific work on this subject is useless; the morphology and phylogeny of each pelagic organism has not the slightest bearing on fishery questions.

The statement of the fishery question is perfectly simple. Given a continuous decrease in a number of valuable fish, due to over-fishing, how may the diminution be checked, and a continuous future supply be insured? The answer to the question is very difficult. Life in the sea is beyond control, and, to a large extent, beyond observation, for the trawl and dredge give a very insufficient idea of the conditions of marine life. There is not so close an analogy between agriculture and fisheries, as is sometimes implied in language. The sea cannot be parcelled out into inclosures; it cannot be cultivated with different kinds of crops at will; its fishes cannot be kept in confinement and protected from their enemies and the weather, nor can they be fed at regular periods as live stock are. It is misleading to talk of "reaping a harvest that is never sown," when the power of sowing *and caring for the crop* is out of reach. The ultimate aim of all scientific investigations in fishery matters must be to find out what circumstances are in human power to control, and to show how that control may best be exercised.

The first and obvious subjects for control are the fishermen themselves. If they are the cause of the depopulation of the seas, such a check may be put upon their proceedings as to obviate the evil. This may be done in one of two ways: by prohibiting fishing altogether in certain specified areas, as has been partially done by the Scotch Fishery Board, so as to afford centres from which fish may spread into the surrounding seas, or by the establishment of close seasons for different species of fish. Both methods are attended with great difficulties, which have been discussed over and over again. They may be summed up as hardship to the fishermen, and the impossibility of preventing the destruction of one species of fish whilst another is being fished for. To establish a close season which would prevent any breeding-fish being caught, would be to prohibit all fishing for three parts of the year. Secondly, the ova of breeding-fish may be artificially fertilized, the fry hatched out and turned out in great numbers to restock the waters that have been depopulated. This method is said to have been attended with success, and demands a further trial; but it must not be supposed that this process in any way resembles the rearing of domestic animals on land, or even the culture of fresh-water fish. The fry, once turned out, are lost sight of, are exposed to the attacks of numerous enemies, and are beyond all further human care. Thirdly, fish might be protected by the wholesale destruction of their natural enemies other than man, just as game is protected by the destruction of stoats, carrion-crows, and other vermin. No doubt a general massacre of cormorants, gannets, and dog-fish would make a great difference to the annual destruction of fish on our coasts, but in the case of the birds, such a course would meet with great opposition; and in the case of dog-fishes, extermination, or even an appreciable reduction in number, would be nearly impossible. Lastly, attempts may be made at culture *sensu stricto*. Young fish may be caught by the ordinary methods, and kept in suitably constructed fattening-ponds until

they are of saleable size; or, to carry the process a step further, the larvæ reared in hatcheries may be turned into similar ponds and brought to maturity. These operations have been conducted with success in more places than one, but the only places where marine fish-culture forms an industry of any importance is in the Adriatic, where there are large inclosures known as *valli*, in which young fry, caught in the open sea, are inclosed and brought to a marketable condition. The possibility of cultivating mussels and oysters in the like manner is too well known to require further mention, and it is quite possible that it may be found practicable to apply the system of culture to lobsters.

These are the practical questions to which fishery officers will have to turn their attention. That a preliminary scientific training is necessary is obvious, for the art of culture requires the most exact knowledge possible of the animals under cultivation, and success will in each case depend on the extent to which the necessities of the organism are studied and supplied. But abstract scientific study must give way to practice; as soon as a man allows the problems of morphology and phylogeny to distract his attention, he will become less careful of his practical experiments, and they will end in disappointment. The Scotch Fishery Board has made an excellent beginning in its trawling experiments; in a short time it may be hoped that its staff will be engaged in numerous other experiments on the protection and production of fishes, crustacea, and mollusks, to which many of the observations published in the Seventh Report are but the preliminary. G. C. B.

THE FIXATION OF FREE NITROGEN.[1]

IN a paper communicated to the Royal Society in 1887-88 (Phil. Trans., 1889), the authors discussed the history and present position of the question of the sources of the nitrogen of vegetation. The earlier results obtained at Rothamsted, as well as those of Boussingault, under conditions in which the action both of electricity and of microbes was excluded, led the authors to conclude that the higher chlorophyllous plants have not the power of taking up elementary nitrogen by means of their leaves, or otherwise. The conclusions arrived at were, that atmospheric nitrogen is not a source of nitrogen in the case of gramineous, cruciferous, chenopodiaceous, or solaneous crops, but with regard to the *Leguminosæ* it was admitted that there was not sufficient evidence to account for the whole of the nitrogen taken up. Of the recent researches bearing on the subject, those of Hellriegel and Wilfarth, first published in 1886, were considered the most striking and conclusive.

In 1883, Hellriegel grew plants of various families in washed sand containing the necessary ash constituents but no nitrogen; in one series nothing further was given, whilst to others varying known amounts of sodium nitrate were added. The gramineous and some other plants of the first series were all limited in growth by the amount of nitrogen contained in the seed, and in the other series the growth was largely proportional to the amount of nitrogen which was applied. On the other hand, whilst most of the peas of the series to which no nitrogen was added failed after a short time, some would develop luxuriantly; and it was found that the roots of the plants of limited growth were free from nodules, and that there was abundant nodule formation on the roots of the well-developed plants. These results led Hellriegel to make further experiments, the results of which showed that leguminous plants will not develop to any extent in sterilized sand free from nitrogen; whilst in the case of peas, vetches, and some other *Papilionaceæ*, the addition of a small quantity of soil extract containing an immaterial amount of nitrogen, causes the plants to grow luxuriantly. A soil extract, prepared from an ordinary soil, which produces such striking results with the plants just mentioned has no effect with lupins. The same result is, however, readily obtained with lupins by the application of a soil extract from a sandy soil in which lupins have been growing. With clover less definite results were obtained for some time, but more recently it has been observed that whilst the extracts from other soils produce little or no effect on clover, an extract from a root-crop soil brought about a considerable nitrogen fixation; but the result was less marked than with the other leguminous plants. In all cases the nitrogen assimilation was accompanied by nodule formation on the roots. Sterilized soil extracts were entirely without effect.

[1] "New Experiments on the Question of the Fixation of Free Nitrogen (Preliminary Notice)," by Sir J. B. Lawes, Bart., LL.D., F.R.S., and Prof. J. H. Gilbert, LL.D., F.R.S. (Proc. Roy. Soc., xlvi. 85).

As stated in a postscript to the paper in the Phil. Trans. already referred to, a preliminary series of pot experiments on similar lines to Hellriegel's was commenced at Rothamsted in 1888. The plants selected were peas, blue lupins, and yellow lupins. They were grown in washed sand containing a small amount (0·0027 per cent.) of nitrogen and the necessary ash constituents; whilst for comparison all the plants were grown in a rich garden soil, and the lupins in a special lupin soil as well. As more normal and satisfactory growth was obtained with peas, only the results relating to these will be discussed here. The lupins, which are admittedly difficult to manage under the artificial conditions which must, more or less, prevail in experiments of this kind, gave no very definite indications in the first year's experiments, although, in 1889, the most striking of the results were those obtained with yellow lupins. Of the peas grown in sand, No. 1 had nothing further added, whilst to Nos. 2 and 3 an extract from the garden soil was added. All the peas germinated and grew well, but about five or six weeks after sowing, the plants of the pots seeded with soil organisms began to acquire a darker colour than those of the pot which was not so seeded, and from this time the plants gained both in leaf surface, and in number of leaflets, and maintained a brighter green colour. At the conclusion of the experiments it was found that the roots of the plants in the unseeded pot had many nodules; the roots of the plants of the seeded pots had many more and much larger nodules than those of the unseeded pot. That these had nodules at all is to be attributed to the impurity and non-sterilization of the sand. The root, too, was much more distributed through the whole of the sand which was seeded than through the sand which was not seeded. The roots of the plants grown in garden soil were very much developed, but showed comparatively few nodules, which were, moreover, smaller than those of the other pots. Owing to the lateness of the season none of the plants flowered.

With regard to the above ground growth at the end of the experiment, there was more vegetable substance produced in the pots seeded with soil organisms than in the unseeded pot; and this increased growth was without doubt connected with the development of the root nodules and their contents. But the greatest gain was in the total nitrogen. In fact, whilst the amount of dry produce in the seeded pots was less than one-fifth more than that of the unseeded pot, there was about twice as much nitrogen in the above ground growth of the seeded, as in that of the unseeded pot. In the case of the garden soil there was more growth, more dry substance, and more nitrogen than in any of the others. In all three pots with sand, the amount of nitrogen in the produce, and in the sand, at the end of the experiment was far greater than that of the seed sown, and the sand, at the commencement. In each case the amount of nitrogen in the sand remained practically unchanged, the gain, therefore, being in the plants. The same may be said of the garden soil, but with some reserve, owing to the great difficulty, to say the least, of detecting slight changes in the amount of nitrogen in a large bulk of rich soil. There is, at least, no evidence to show that either the sand or the garden soil have taken up nitrogen on their own account, independently of the plant.

In order to show clearly that the gain of nitrogen is far beyond the limits of experimental error, it will be well to give some numerical results showing the actual amounts which had to be dealt with. Leaving out of account the difference in the amount of nitrogen of the seeds sown in each pot—the exact amounts are recorded in the paper—and the slight difference in the initial and final amounts of nitrogen in the sand, the results will be as follows:—In the 9 pounds of sand which each pot contained there was nearly 0·1 gram of nitrogen. The three seeds sown in each pot contained nearly 0·03 gram of nitrogen. At the conclusion of the experiment the vegetable produce contained: pot 1, 0·28; pot 2, 0·54; pot 3, 0·44 gram of nitrogen; which, after deducting the nitrogen of the seed sown, corresponds with a gain of 0·25, 0·51, and 0·41 gram of nitrogen.

The experiments in the second season, 1889, included the following leguminous plants: peas, red clover, vetches, blue lupins, yellow lupins, and lucerne. The sand used this time was a coarse, white sand which was well washed and also sufficiently, if not absolutely, sterilized by heating for some days at nearly 100° C. The necessary ash constituents, mixed with an equal weight of calcium carbonate, were added to each pot. There were four pots to each series. No. 1 contained the prepared sand with nothing further added. Nos. 2 and 3 the same sand to which a soil extract was added—prepared from a good garden

soil in the case of all the plants except the lupins, and for these from a special sandy soil from a field in which lupins were growing. No. 4 garden soil, or, for the lupins, the special lupin soil. With regard to the peas and vetches of the pots not seeded with soil extract, the growth was extremely limited, and the colour of the leaves pale green; in the second and third pots of the two series there was luxuriant growth, the plants being taller even than those grown in garden soil. On the other hand, the garden soil plants were more vigorous and produced flower and seed. An examination of the roots of the plants showed an entire absence of nodule formation on those of the pots where no soil extract was given, whilst on the roots of the other plants there was, coincidently with the increased growth, an abundance of nodules. As in the experiments of 1888, the amount of nodules on the roots of the plants grown in garden soil was less than in the sand treated with soil extract.

Still more striking were the results obtained with yellow lupins. Whilst the plants of pot 1 (without soil extract) barely appeared above the rim of the pot, those of pots 2 and 3 (with soil extract) were large branched plants, the largest being 2 feet high—larger even than those grown in the lupin soil. Moreover, unlike the peas and vetches, the yellow lupins grown in sand seeded with soil organisms, flowered and seeded freely. The superiority of these plants over those grown in the lupin sandy soil may be due to the fact that the lupin soil was much less porous than the sand, especially when watered, and perhaps on this account less adapted for favourable growth. The roots of the plants without soil extract seeding were of very limited development and quite free from nodules. In pots 2 and 3, with soil extract seeding, the root development was very great, and the roots showed several large swellings; the ends of the roots were thickly covered with root-hairs, probably indicating an effort to acquire water and mineral nutriment in quantity commensurate with the large amount of nitrogen fixed, and so rendered available to the plant. In the garden soil the root development and nodule formation were much less.

The blue lupins again failed, with the exception of one plant in one pot. The red clover and lucerne are left for further growth. In pot 1 (unseeded) of the lucerne the plants do not appear to have grown at all since a few weeks after the seeds were sown, and for a long time there seemed to be no increased growth in pots 2 and 3, which were seeded with garden soil extract. Pot 2 had, therefore, a fresh quantity of soil extract—this time from a soil where lucerne was growing—added; this also seemed for some time to have no effect, but subsequently there was some increased depth of colour and some increased growth. Pot 3 was watered with a dilute solution of calcium nitrate, which soon produced a very marked and beneficial effect. With regard to the red clover, the results are, as yet, uncertain; both in the pots to which soil extract was added, and in that which had no soil extract, there is much more growth than is believed can be accounted for by nitrogen in the seed sown. The glass house in which the experiments are made stands in the middle of allotment gardens where vegetables of all kinds are grown, and this fact, viewed in the light of Hellriegel and Wilfarth's more recent results, already referred to—according to which the best results with clover are obtained by seeding with organisms from a root-crop soil—points to a possible acquisition of organisms from the air as the most probable explanation.

Attention is drawn to the widely different external appearance of the tubercles of the different plants. In the case of peas, they occurred generally as agglomerations; on the roots of vetches the nodules were generally single. Lupins seem to have two kinds of tubercular development, the most prevalent being "swellings" which entirely encase the thick roots; the "nodules" are generally small, and are distributed on the root-fibres. The lucerne nodules are, again, quite different in form from any of those already mentioned, being long, and generally divided or branched.

Returning to the main object of the investigation, the results confirm those of Hellriegel and Wilfarth, in showing the fixation of free nitrogen under the influence of microbe-seeding of the soil, and the resulting nodule formation on the roots in the case of the leguminous plant.

It appears that, concurrently with the experiments made at Rothamsted, M. Bréal, of the Muséum d'Histoire Naturelle of Paris, has made various experiments instigated by the results of Hellriegel and Wilfarth, and his results also confirm those of Hellriegel.

Hellriegel agrees with the authors that the *Leguminosæ* utilize soil nitrogen. He considers that the soil would be drawn upon first, and that this source is supplemented by the elementary nitrogen of the air, brought into combination by means of the organisms; he also considers that there would be more or less fixation even with a soil rich in nitrogen. On the other hand, Vines found the formation of tubercles, and presumably also the fixation of free nitrogen, is much reduced, or even stopped altogether, by the application of much nitrate to the soil; and the Rothamsted experiments indicate, that with a rich garden soil there are far fewer nodules formed, than with a sand containing but little nitrogen, and seeded with soil organisms. If subsequent experiments should show this to be the case, the amount of nitrogen of a crop, derived from the air, and the amount derived from a soil, would vary very much according to circumstances; fixation would take place most freely in the case of a sandy, or poor and porous soil, and less in a richer soil.

Upon the whole, it is considered that the evidence at command points to the conclusion that, in the case of most, if not all our leguminous crops, more or less of their nitrogen will be due to fixation under the conditions supposed.

Regarding the mode in which the organisms, which, in symbiosis with the higher plants, bring about the fixation, although Marshall Ward, Prażmowski, and Beyerinck have already contributed interesting results as to the mode of life of these bodies, much has yet to be learnt on the subject before an adequate explanation of the phenomena involved can be given. The authors suggest the following alternatives:—"(1) That, somehow or other, the plant itself is enabled, under the conditions of symbiotic life, to fix free nitrogen of the atmosphere by its leaves—a supposition in favour of which there seems to be no evidence whatever. (2) That the parasite utilizes and fixes free nitrogen, and that the nitrogenous compounds formed are taken up by the host. On such a supposition, the actually ascertained large gain of nitrogen by the leguminous plant growing in a nitrogen-free, but properly infected soil, becomes intelligible."

In their former paper (*loc. cit.*) the authors had stated that all the evidence that had been acquired in lines of inquiry previously followed had failed to solve conclusively the question of the sources of the whole of the nitrogen of the *Leguminosæ*, and that hence it should not excite surprise that new light should come from a new line of inquiry.

"The question suggests itself, whether such, or allied agency, comes into play in the nitrogen assimilation of leguminous plants generally, or that of other than the agricultural representatives of the non-leguminous families to which we owe such plants, or of those of the numerous and varied other families of the vegetable kingdom.

"It is true that the families which contribute staple agricultural plants are but few, and that the agricultural representatives of those families are also comparatively few. The families so contributing are, however, among the most important and widely distributed in the vegetable kingdom; as also are some of the plants they contribute. As prominent examples may be mentioned the *Gramineæ*, affording the cereal grains, a large proportion of the mixed herbage of grass-land, and other products; also the *Leguminosæ*, yielding pulse-crops, many useful herbage plants, and numerous other products. As we have said, there does not seem to be an unsolved problem as to the sources of the nitrogen of other of our agricultural plants than those of the leguminous family. Obviously, however, it would be unsafe to generalize in regard to individual families as a whole, from results relating to a limited number of examples supplied by their agricultural representatives alone. Still, there is nothing in the evidence at present at command to point to the supposition that there is any fundamental difference in the source of the nitrogen of different members of the same family, such as is clearly indicated between the representatives of the leguminous, and of the other families, supplying staple agricultural products. On the other hand, existing evidence does not afford any means of judging whether or not similar, or allied agencies to those now under consideration, or even quite different ones, may come into play in the nitrogen assimilation of other families which contribute such a vast variety of vegetation to the earth's surface." N. H. J. M.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—At the meeting of the Philosophical Society, on Monday, May 12, at 4.30 p.m., the following papers are promised:—

Mr. Langley, effect of nicotin on the nervous system of the fresh-water crayfish.

Mr. Shipley, on a new species of Phymosoma, with some account of the geographical distribution of the genus.

Mr. Adami, on the action of the papillary muscles of the heart.

Mr. Harmer, exhibition of specimens of a Land-Planarian (*Rhynchodemus terrestris*, O. F. Müller) found in Cambridge.

SCIENTIFIC SERIALS.

The *American Meteorological Journal* for March contains the conclusion of M. Faye's articles on the theory of storms, based on Redfield's laws. The author maintains that cyclones are descending whirls with a vertical axis, that they originate in the upper currents of the atmosphere, and follow the course of these currents. He considers it very desirable that two different modes of drawing charts should be adopted, to distinguish between cyclones and statical depressions to which, in his opinion, the laws of storms do not in any way apply.—Prof. H. A. Hazen contributes an article on the spectre of the Brocken, and gives a summary of various explanations of the phenomenon. He gives the results of his observations upon a similar shadow seen upon Mount Washington. This number also contains an extract from a paper by Dr. Schenck on the climatic treatment of pulmonary consumption; the advantages of New Mexico, especially, are pointed out.

In the number of the *Journal* for April, M. Faye commences a series of articles on trombes and tornadoes; he deals principally in this number with the theories of various writers and with descriptions of the phenomenon.—Mr. A. H. Dutton analyses the laws laid down by Padre Viñes relative to the normal points of recurvature of West India hurricanes. The result of his inquiry is that less than 14 per cent. of the tropical storms obeyed the laws.—Mr. A. L. Rotch summarizes the proceedings of the International Hydrological Congress held at Paris in October last. The next Congress is to be held in Rome in 1892.

Department of Agriculture, Bulletin No. 4, July 1889 (by authority, Government Printing Office, Melbourne).—This Report embodies the results of State-aided scientific effort which is intended to benefit agriculture, as we sincerely trust it will. The contents are miscellaneous, although all have direct bearing on the theory and practice of agriculture. Reports on horse-breeding, the needs of plants, irrigation, liquorice, yields of milk, vineyards, fruit-culture, Danish dairying, &c., yield a varied diet for the omnivorous reader, and will be of special service to Australian cultivators. We plead guilty to a feeling in connection with the perusal of such *Bulletins* as this, that the work is official, and lacks spontaneity. There is, notwithstanding, much that is valuable. Take, for example, the raisin industry (p. 91). Here we find described the conditions for successful growth, varieties, cultivation, and drying. What can be more useful to a colonist up country than to possess trustworthy information in detail on such a subject? If he is engaged in the wider pursuits of horse or cattle ranching, he will find subject-matter—addresses of breeders, names of sires, and other information of solid value. The *Bulletin* will also be of interest to the increasing class of owners of land in Australia who reside in England, as well as to young men who are thinking of making Australia their home. Anyone writing for this class of information should secure the previous numbers and also the future issues, and these he would probably be able to obtain free of charge by application at the offices of the Agricultural Department at Melbourne, or in London, at the Australian Colonial Offices in Victoria Street.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 27.—"Measurements of the Amount of Oil necessary in order to check the Motions of Camphor upon Water." By Lord Rayleigh, Sec. R.S.

The motion upon the surface of water of small camphor scrapings, a phenomenon which had puzzled several generations of inquirers, was satisfactorily explained by Van der Mensbrugghe (*Mémoires Couronnés* (4to) of the Belgian Academy, vol. xxxiv., 1869), as due to the diminished surface-tension of water impregnated with that body. In order that the rotations may be lively, it is imperative, as was well shown by Mr. Tomlinson, that the utmost cleanliness be observed. It is a good plan to submit the internal surface of the vessel to a preliminary treatment with strong sulphuric acid. A touch of the finger is usually sufficient to arrest the movements by communicating to the surface of the water a film of grease. When the surface-tension is thus lowered, the differences due to varying degrees of dissolved camphor are no longer sufficient to produce the effect.

It is evident at once that the quantity of grease required is excessively small, so small that under the ordinary conditions of experiment it would seem likely to elude our methods of measurement. In view, however, of the great interest which attaches to the determination of molecular magnitudes, the matter seemed well worthy of investigation; and I have found that by sufficiently increasing the water surface the quantities of grease required may be brought easily within the scope of a sensitive balance.

In the present experiments the only grease tried is olive oil. It is desirable that the material which is to be spread out into so thin a film should be insoluble, involatile, and not readily oxidized, requirements which greatly limit the choice.

Passing over some preliminary trials, I will now describe the procedure by which the density of the oil film necessary for the purpose was determined. The water was contained in a sponge-bath of extra size, and was supplied to a small depth by means of an india-rubber pipe in connection with the tap. The diameter of the circular surface thus obtained was 84 cm. (33"). A short length of fine platinum wire, conveniently shaped, held the oil. After each operation it was cleaned by heating to redness, and counterpoised in the balance. A small quantity of oil was then communicated, and determined by the difference of readings. Two releasements of the beam were tried in each condition of the wire, and the deduced weights of oil appeared usually to be accurate to $\frac{1}{20}$ milligram at least. When all is ready, camphor scrapings are deposited upon the water at two or three places widely removed from one another, and enter at once into vigorous movement. At this stage the oiled extremity of the wire is brought cautiously down so as to touch the water. The oil film advances rapidly across the surface, pushing before it any dust or camphor fragments which it may encounter. The surface of the liquid is then brought into contact with all those parts of the wire upon which oil may be present, so as to ensure the thorough removal of the latter. In two or three cases it was verified by trial that the residual oil was incompetent to stop camphor motions upon a surface including only a few square inches.

The manner in which the results are exhibited will be best explained by giving the details of the calculation for a single case, *e.g.* the second of December 17. Here 0·81 milligram of oil was found to be nearly enough to stop the movements. The volume of oil in cubic centimetres is deduced by dividing 0·00081 by the sp. gr., viz. 0·9. The surface over which this volume of oil is spread is

$$\tfrac{1}{4}\pi \times 84^2 \text{ square centimetres};$$

so that the thickness of the oil film, calculated as if its density were the same as in more normal states of aggregation, is

$$\frac{0{\cdot}00081}{0{\cdot}9 \times \frac{1}{4}\pi \times 84^2} = \frac{1{\cdot}63}{10^7} \text{ cm.},$$

or 1·63 micro-millimetres. Other results, obtained as will be seen at considerable intervals of time, are collected in the table.

A Sample of Oil, somewhat decolorized by exposure.

Date.	Weight of oil.	Calculated thickness of film.	Effect upon camphor fragments.
	Mg.		
December 17 ...	0·40	0·81	No distinct effect.
January 11 ...	0·52	1·06	Barely perceptible.
,, 14 ...	0·65	1·32	Not quite enough.
December 20 ..	0·78	1·58	Nearly enough.
January 11 ...	0·78	1·58	Just enough.
December 17 ...	0·81	1·63	Just about enough.
,, 18 ...	0·83	1·68	Nearly enough.
January 22 ...	0·84	1·70	About enough.
December 18 ...	0·95	1·92	Just enough.
,, 17 ...	0·99	2·00	All movements very nearly stopped.
,, 20 ...	1·31	2·65	Fully enough.

A Fresh Sample.

January 28 ...	0·63	1·28	Barely perceptible.
,, 28 ...	1·06	2·14	Just enough.

For convenience of comparison they are arranged, not in order of date, but in order of densities of film.

The sharpest test of the quantity of oil appeared to occur when the motions were nearly, but not quite, stopped. There may be some little uncertainty as to the precise standard indicated by "nearly enough," and it may have varied slightly upon different occasions. But the results are quite distinct, and under the circumstances very accordant. The thickness of oil required to take the life out of the camphor movements lies between one and two millionths of a millimetre, and may be estimated with some precision at 1·6 micro-millimetre. Preliminary results from a water surface of less area are quite in harmony.

For purposes of comparison it will be interesting to note that the thickness of the black parts of soap films was found by Messrs. Reinold and Rücker to be 12 micro-millimetres.

An important question presents itself as to how far these water surfaces may be supposed to have been clean to begin with. I believe that all ordinary water surfaces are sensibly contaminated; but the agreement of the results in the table seems to render it probable that the initial film was not comparable with that purposely contributed. Indeed, the difficulties of the experiments proved to be less than had been expected. Even a twenty-four hours' exposure to the air of the laboratory[1] does not usually render a water surface unfit to exhibit the camphor movements.

The thickness of the oil films here investigated is, of course, much below the range of the forces of cohesion; and thus the tension of the oily surface may be expected to differ from that due to a complete film, and obtained by addition of the tensions of a water-oil surface and of an oil-air surface. The precise determination of the tension of oily surfaces is not an easy matter. A capillary tube is hardly available, as there would be no security that the degree of contamination within the tube was the same as outside. Better results may be obtained from the rise of liquid between two parallel plates. Two such plates of glass, separated at the corners by thin sheet metal, and pressed together near the centre, dipped into the bath. In one experiment of this kind the height of the water when clean was measured by 62. When a small quantity of oil, about sufficient to stop the camphor motions, was communicated to the surface of the water, it spread also over the surface included between the plates, and the height was depressed to 48. Further additions of oil, even in considerable quantity, only depressed the level to 38.

The effect of a small quantity of oleate of soda is much greater. By this agent the height was depressed to 24, which shows that the tension of a surface of soapy water is much less than the combined tensions of a water-oil and of an oil-air surface. According to Quincke, these latter tensions are respectively 2·1 and 3·8, giving by addition, 5·9; that of a water-air surface being 8·3. When soapy water is substituted for clean, the last number certainly falls to less than half its value, and therefore much below 5·9.

April 24.—"On a Pneumatic Analogue of the Wheatstone Bridge." By W. N. Shaw, M.A., Lecturer in Physics in the University of Cambridge. Communicated by Lord Rayleigh, Sec. R.S.

When fluid flows steadily through an orifice in a thin plate, the relation between the rate of flow, V, measured in units of volume of fluid per second, and the head H (the work done on unit mass of the fluid during its passage) may be expressed by the equation:—

$$H = RV^2,$$

where R is a constant depending upon the area of the orifice. If the head be measured in gravitation units, R is equal to $1/2gk^2a^2$, where g is the acceleration of gravity, a the area of the orifice, and k the coefficient of contraction of the vein of fluid, a factor which is independent of the rate of flow.

Measurements made upon the flow of air in order to determine the coefficient of contraction have been hitherto such as may be termed "absolute"; that is to say, the head and the flow have each been separately expressed in absolute measure and the value of R determined by taking the ratio of the head to the square of the flow. This process is exactly analogous to measuring the electrical resistance of a wire by finding the electromotive force between its ends and the current which flows along it.

M. Murgue, in a work on "The Theory and Practice of Centrifugal Ventilating Machines" (translated by A. L. Steavenson), has shown that the internal resistance of a centrifugal fan to the flow of air through it can be calculated from the effects produced on the flow by varying the size of a second orifice through which the air had to pass. This process is evidently parallel to calculating the internal resistance of a battery by finding the effect produced upon the current by varying the external resistance. The further development of the analogy seems to afford a "null" method of comparing resistances to the motion of air, and of verifying the laws of flow, and one which requires only a detector and not an anemometer, and is independent of the constancy of the flow. Whether it could be used practically to test the laws of flow and measure the pneumatic constants for various orifices to a higher degree of accuracy than has hitherto been attained, evidently depends upon the sensitiveness of the arrangement. In order to try this, the author constructed what may be called a pneumatic analogue of the Wheatstone Bridge. It consists of three wooden rectangular boxes, A, B_1, B_2. The ends of B_1 and B_2 abut against the side of A; between B_1 and A is a rectangular opening, a_2, 1 in. × ½ in., in a cardboard diaphragm; between B_2 and A a rectangular opening, a_4, 1 in. × 1 in., in a similar diaphragm. In the side of B_1 at a_1 is an adjustable slit, made by cardboard shutters sliding in cardboard grooves, and at a_3 in the side of B_2, opposite to a_1, is a similar adjustable slit. The tube connecting B_1 and B_2, or "galvanometer" tube, is a straight tube of glass, G, of about 1·1 inch internal diameter. It can be closed at one end by a small trap-door in the interior of the box B_1, which can be opened and shut by a steel wire passing through a cork in the top of B_1. The sensitiveness of the apparatus depends upon the indicator employed. There are many indicators that might be suggested; the one tried and found to work well consists of two very small parallel magnetized sewing needles, stuck through a cap of elder-pith, supported on a small agate compass centre; the needles carry very light mica vanes on one side of the centre, counterpoised by a small quantity of platinum wire. The whole is balanced on the point of the finest needle obtainable, and forms a very delicate wind vane. The needles take up a definite position of equilibrium with the planes of the vanes approximately north and south. The apparatus being so placed that the tube, G, is east and west, the vanes always set across the tube when there is no current. The needle-points enable the position of equilibrium to be clearly identified by the aid of a fiducial mark on the glass tube. The sensitiveness can be altered as desired by an external control magnet, just as that of a galvanometer needle can be. The little compass needle or wind vane, M, is very sensitive to the motion of air in the tube.

The head is produced by a gas-burner in a metal chimney fitted to the lid of the box A.

Of the four apertures of the bridge, two, viz. a_2 and a_4, are inaccessible without pulling the arrangement to pieces; they represent areas of ½ sq. in. and 1 sq. in. respectively, as accurately as a knife can cut them in cardboard.

The other two areas, viz. a_1 and a_3, are made by sliding shutters, as already mentioned. The edges were cut with a knife, and they probably are only rough approximations to areas in a truly thin plate.

If the coefficient of contraction may be assumed to be independent of the shape of the orifice, we get the condition for no flow through the "galvanometer" tube:—

$$\frac{a_1}{a_2} = \frac{a_3}{a_4},$$

where the a's represent the actual measured areas of the four orifices.

Observations have been taken with the apparatus—

(1) To verify the law of proportionality of areas, viz.

$$\frac{a_1}{a_2} = \frac{a_3}{a_4}.$$

(2) To verify the inference that the condition of no flow is independent of the total head.

[1] In the country.

(3) To compare a circular with a rectangular aperture.

The observations are sufficient to show that the width of the adjustable slit when there is no flow is a perfectly definite magnitude, and that a properly constructed apparatus is capable of making measurements of the effective areas of orifices with a very considerable degree of precision.

"On the Effect of Tension upon Magnetic Changes of Length in Wires of Iron, Nickel, and Cobalt." By Shelford Bidwell, M.A., F.R.S.

The *iron* used in these experiments was a piece of soft annealed wire, 0·7 mm. in diameter and 10 cm. in length between the clamps. The weights successively attached to it were equivalent to 1950, 1600, 1170, 819, 585, and 351 kilos. per square cm. of section.

The *nickel* wire was 100 mm. long, and 0·65 mm. in diameter. The loads under which it was examined were 2310, 1890, 1400, 980, 700, and 420 kilos. per sq. cm.

The *cobalt* used was a narrow strip measuring 100 mm. by 2·6 mm. by 0·7 mm., its cross section being, therefore, 1·82 sq. mm. It was not possible to obtain this metal in the form of a wire. The loads employed for the strip were equivalent to 772, 344, and 75 kilos. per sq. cm.

In all the experiments the loads were successively applied in decreasing order of magnitude, and before every single observation the wire or strip was demagnetized by reversals, without, of course, being removed from the coil. The magnetizing force was carried up to about 375 C.G.S. units for iron and nickel, and 500 units for cobalt.

The results are given in several tables and curves, and point to the following conclusions:—

Iron.—Tension diminishes the magnetic elongation of iron, and causes contraction to take place with a smaller magnetizing force.

Nickel.—In weak fields the magnetic contraction of nickel is diminished by tension. In fields of more than 140 or 150 units, the magnetic contraction is increased by tensional stress up to a certain critical value, depending upon the strength of the field, and diminished by greater tension.

Cobalt.—The magnetic contraction of cobalt is (for magnetic fields up to 500 G.C.S. units and loads up to 772 kilos. per sq. cm.) practically unaffected by tension.

Chemical Society, April 3.—Dr. Hugo Müller, F.R.S., Vice-President, in the chair.—The following papers were read:—Note on the hydrosulphides, by Messrs. S. E. Linder and H. Picton. The authors find that freshly-precipitated metallic sulphides almost always contain hydrogen sulphide, that they are, in fact, hydrosulphides or remnants of hydrosulphides, and that if, instead of adopting the usual plan of passing gas through the solution, the metallic salt be allowed to run slowly into a solution of hydrogen sulphide in water in the absence of too large an excess of acid, a solution of the hydrosulphide is obtained which can be freed from dissolved hydrogen sulphide by the current of hydrogen. The copper hydrosulphide, $7CuS,H_2S$, and mercury hydrosulphide, $31HgS, H_2S$, are described in the paper.—Researches on the germination of some of the *Gramineæ*, Part I., by H. T. Brown, F.R.S., and Dr. G. H. Morris. This investigation was undertaken with the view of throwing some light on the complex metabolic processes which take place in the germination of seeds. The authors, during the progress of the inquiry, have examined and experimented with the seeds of a great number of the grasses, but this, the first part of their paper, is confined almost entirely to a consideration of the changes which take place in barley during the earlier periods of its growth. In recording the visible changes which occur in the seed during germination, it is shown that a disintegration and dissolution of the cell-walls of the endosperm always precede any attack upon the cell-contents. This breaking down of the cell-wall is shown in a subsequent portion of the paper to depend on the production during germination of a special cellulose-dissolving or "cyto-hydrolytic" enzyme, which, like diastase, is soluble. The action of this enzyme on the cell-walls of some kinds of vegetable parenchyma is very energetic. The physiological importance of this cyto-hydrolyst is very great, for, owing to the non-diffusible nature of the amylo-hydrolytic enzyme—diastase—the previous breaking down of the cell-wall is a necessary prelude to the dissolution of the contained starch-granules. The authors show that the appearance of the cyto- and amylo-hydrolysts is due to a specialized secretory function of the layer of columnar epithelium which covers the outer surface of the scutellum. It has hitherto been considered that the function of this epithelium was exclusively that of an absorptive tissue: its absorptive as compared with its secretory functions are, however, of quite secondary importance. The natural food material—starch—does not appear to have any special power of stimulating the cells of the epithelium to increased secretion of a diastase, but the flow both of diastase and of the cyto-hydrolytic enzyme from these cells is affected in a very remarkable degree by the presence of certain carbohydrates. Providing the carbohydrate is one which is readily assimilable by the embryo, such as cane-sugar or maltose, secretion of ferment is checked or even entirely inhibited. No such inhibitory action is, however, produced by such substances as mannitol and milk-sugar, which are entirely without nutritive value. The authors' experiments in this direction point to the secretion of the amylo-hydrolytic and cyto-hydrolytic enzymes as being to some extent *starvation phenomena.* The power of secretion possessed by the epithelium is in some way or other so adapted to the requirements of the young plant as to be only exercised when the supply of tissue-forming carbon compounds begins to fail. The histological changes which take place in the cells of the epithelium during secretion are very similar to those which have been observed in certain secretory cells of the alimentary tract of animals, and in the secretory cells of some of the insectivorous plants. The authors confirm the important generalization of Sachs, that the relation of the embryo to the endosperm is that of parasite to host, and they have availed themselves of this relation by cultivating the embryo on suitable media after separating it from its endosperm. In this way they have obtained information with regard to the secretory powers of the embryo and the chemical modifications of its absorbed nutriment which it would have been impossible to obtain by any other means. The results of cultivating excised embryos on various nutrient solutions, more especially of the carbohydrates, are recorded, and it is shown that, whilst cane-sugar, invert-sugar, dextrose, lævulose, maltose, raffinose, galactose, and glycerol have all more or less nutrient value, milk-sugar and mannitol do not in any way contribute to the growth of tissue in the young plant. Of all the substances tried, cane-sugar has by far the greatest nutritive power. Maltose, although the natural food of the embryo when attached to its endosperm, is decidedly inferior in this respect to cane-sugar. This, at a later point in the paper, is shown to be due to the fact that maltose, directly it is absorbed by the growing embryo, becomes transformed into cane-sugar by the living cells, and in this form is passed from cell to cell. When cane-sugar is supplied ready formed to the young plantlet, there is manifestly a saving of energy to the living cell, which receives its nutriment in a form in which it is readily available for its requirements. An examination of the sugars produced during germination, and their mode of distribution in the grain, have convinced the authors that the transformed starch of the endosperm is absorbed by the embryo in the form of maltose, and that the seat of production of the cane-sugar which all germinated grain contains is the tissues of the embryo itself. The authors are continuing their work upon the germination of the grasses, and are applying the methods described in this first part of their paper to an elucidation of the chemical changes which the other reserve materials, especially the proteids, undergo in their passage from the endosperm, and of the agencies which are at work in bringing about these transformations. In the discussion which followed the reading of this paper, Prof. Marshall Ward, F.R.S., pointed out that in the seeds of the *Gramineæ*, *Cyperaceæ*, and other families of plants, there is a peculiar layer of cells, from one to three or more deep, surrounding the starchy endosperm, and distinguished from the latter by containing no starch, but relatively large quantities of proteids: this layer belongs to the endosperm, but as the seed ripens, the cells store special proteids instead of the starch-grains which predominate in other endosperm cells. In the oat there is such a layer, one cell deep, and it has been shown that, during germination, the dissolution of the starch and the cell-walls of the starch-containing cells begins near the surface of this layer, which itself persists, and the cells of which take up food and undergo changes so like those of excreting cells that it was concluded that they excrete the diastatic enzyme. Prof. Ward further remarked that the authors' suggestion that more than one enzyme may be excreted according to the nutrition of the cells, and their proof that a cellulose-dissolving enzyme exists in barley, are borne out by various recent researches, and by Wortmann's observations on the behaviour of bacteria in a mixture of starch and proteids. Wortmann proved

that so long as the bacteria were fed with proteids they refused to excrete the diastatic enzyme which they produce in abundance when only carbohydrates are at their disposal. Prof. Green said that in the case of the date-stone his observations led him to believe that the enzyme was independent of the endosperm, and that probably it was located in the epithelial layer. But in castor oil seeds not only the embryo but also the endosperm cells appeared to be possessed of vitality, the fatty matter of the latter undergoing change even when not subject to the action of the embryo; probably the enzyme was present in the form of an enzymogen, as extracts of the seeds were rendered active by acids. Prof. Armstrong remarked that the authors had shown that in the plant maltose was converted into cane-sugar; dextrose, according to their observations, did not undergo conversion into cane-sugar, but gave invert-sugar—that is to say, it became partially converted into lævulose, these constituents of cane-sugar being apparently incapable of interacting. It was known from Emil Fischer's work that dextrose could be converted into lævulose, and that maltose was an etheric compound of the acetal type, formed from two molecules of dextrose, one of which acted as aldehyde, the other as alcohol; it was conceivable that if the "dextrose residue" in maltose underwent a change comparable with that which is involved in the conversion of dextrose into lævulose, a compound would be obtained which if not identical with cane-sugar would be convertible into it by hydration and subsequent dehydration. The authors had spoken of the maltose becoming incorporated with the protoplasm from which the cane-sugar was then elaborated; perhaps the effect was comparable with that exercised by phenylhydrazine in effecting the conversion of dextrose into lævulose through the agency of the osazone. Dr. Lauder Brunton and Mr. Thiselton Dyer also took part in the discussion.—The formation of indene-derivatives from dibrom-α-naphthol, by Prof. R. Meldola, F.R.S., and Mr. F. Hughes.—The action of hydrochloric acid on manganese dioxide; manganese tetrachloride, by Mr. H. M. Vernon. Contrary to the statements of Pickering (Chem. Soc. Trans., 1879, 654), the author finds that the original product of the action of hydrochloric acid on manganese dioxide is manganese tetrachloride, and that at first no free chlorine is formed.

April 17.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Phosphorous oxide, Part 1, by Prof. T. E. Thorpe, F.R.S., and Mr. A. E. Tutton. The authors describe a method of making phosphorous oxide by burning phosphorus in air. Pure phosphorous oxide crystallizes in thin monoclinic prisms, melts at 22°·5, solidifies at 21°, and boils unchanged in an atmosphere of nitrogen or carbon dioxide at 173°. When heated at 300°, it decomposes, and at 440° is wholly converted into phosphorus and phosphorus tetroxide: $4P_4O_6 = 6P_2O_4 + P_4$. Phosphorous oxide is readily acted on by light, and in bright sunshine its colour rapidly becomes yellow, and eventually dark red, the violet rays being most active in effecting the change. Its molecular weight, as determined by Hofmann's and Raoult's methods, corresponds with the formula P_4O_6; phosphorous oxide, therefore, in this respect is analogous to arsenious and antimonious oxides. The thermal expansion of liquid phosphorous oxide is expressed by the formula—

$$V = 1 + 0{\cdot}0_3 91377\, t - 0{\cdot}0_6 11175\, t^2 + 0{\cdot}0_8 38607\, t^3;$$

its relative density at the boiling-point is 1·6859, whence its molecular volume = 130·5; and its molecular refraction for A ($\lambda = 7604$) at 27°·4 is 60·5. Contrary to the usual statement of the text-books, cold water has very little action on phosphorous oxide: many days elapse before even a small quantity is dissolved; it then forms phosphorous acid. Hot water acts upon it with explosive violence, forming the red sub-oxide, phosphoric acid, and spontaneously inflammable phosphoretted hydrogen. Phosphorous oxide spontaneously oxidizes to phosphorus pentoxide on exposure to air or to oxygen, and the process of oxidation is attended under diminished pressure by a faint luminous glow; ozone is not formed as the oxidation proceeds. On gently warming the oxide in oxygen, the glow gradually increases in intensity until it passes into flame. In contact with ozone, phosphorous oxide glows at the ordinary temperature and pressure. Phosphorous oxide has a well-marked physiological effect, and it is not improbable that the action hitherto attributed to phosphorus, especially as regards its influence on the glycogenic functions of the liver and on tissue change, may be really due to this substance. The fumes from phosphorus consist largely of phosphorous oxide, and the odour of the product obtained by drawing air over phosphorus without allowing it to ignite is identical with that of the pure oxide; it is, indeed, highly probable, as Schönbein long ago surmised, that phosphorus vapour, as such, is odourless, and that the smell which phosphorus ordinarily possesses is a mixture of that of ozone and of phosphorous oxide.—The action of chlorine on water in the light, and the action of light on certain chlorine acids, by Prof. A. Pedler. As a general result of a number of experiments, it is found that, even in very strong tropical sunlight, water and chlorine interact to but a very slight extent when the proportion is about $100\ H_2O : Cl_2$; when the ratio is about $150\ H_2O : Cl_2$ action takes place to the extent of perhaps 50 per cent., and when more than $400\ H_2O : Cl_2$, to about 80 per cent. of the theoretical. Chlorine water containing about $708\ H_2O : Cl_2$ when exposed to direct tropical sunlight decomposes practically entirely in the sense of the equation $2H_2O + 2Cl_2 = O_2 + 4HCl$, an exceedingly small amount of chloric acid being formed; but when exposed in a south aspect to strong diffused daylight, gives much less oxygen and a variable amount of hypochlorous or chloric acids, very little oxygen but an increased amount of hypochlorous or chloric acids being formed when it is exposed in a north aspect to moderate diffused daylight. Hypochlorous acid, on exposure to light in dilute solutions, yields both oxygen and chloric acid, the proportion of oxygen being larger, the greater the intensity of the light. Solutions of chloric acid undergo little or no change. The author concludes that the action of chlorine on water is in its first stage similar to that which it exercises on cold, dilute aqueous potash or soda, and in its second stage to that on more concentrated hot solutions of these alkalis.—Note on the explosion of hydrogen sulphide and of carbon bisulphide with air and oxygen, by the same. The author finds that when a mixture of hydrogen sulphide, air, and oxygen is exploded, a normal result is obtained, sulphur dioxide and water being formed. But when carbon bisulphide vapour is similarly treated, a not inconsiderable proportion of the nitrogen of the air becomes oxidized, and sulphuric compounds are formed under the combined influence of the oxides of nitrogen and sulphur and of the moisture present.—The action of light on phosphorus, and on some of the properties of amorphous phosphorus, by the same. The author brings forward evidence to show that the term "amorphous phosphorus" is a distinct misnomer, and that, so far from the commercial amorphous phosphorus constituting a separate allotropic modification of the element, it is in reality the same substance as the form called rhombohedral or metallic phosphorus; the very slight differences in character noticed between the substances in question being explained by the difference in the state of division and the slight variations conditioned by their mode of formation. The change of red into ordinary phosphorus does not take place below 358°; above this the change takes place *in vacuo*, but exceedingly slowly, even at 445°.—The action of phosphoric anhydride on fatty acids, by Dr. F. S. Kipping. Heptylic acid yields 25–33 per cent. of dihexyl ketone when heated with phosphoric anhydride.

Royal Microscopical Society, April 16.—Dr. C. T. Hudson, F.R.S., President, in the chair.—Mr. J. Mayall, Jun., called attention to a spiral ruling on glass, sent by Mr. P. Braham, of Bath, which had been produced in an ordinary lathe, the diamond point being adjusted on the slide rest; also to a series of photomicrographs of diatoms, sent by Mr. T. Comber. These were of special interest from the fact that they were produced with sunlight, by which the maximum resolving power of the objective was obtained.—Mr. Mayall referred to an improved form of fine-adjustment, constructed and exhibited by Messrs. Powell and Lealand, in which the chief aim had been to construct a fine-adjustment which should combine extreme sensitiveness of action with accuracy and probable durability beyond what had previously been obtained. The essential feature was the application of what watchmakers would term a "jewelled movement." The whole of the contact surfaces by which the fine-adjustment was actuated consisted of polished steel and agate, the intention being to reduce the friction as much as was consistent with steadiness of motion. The result attained was undoubtedly an improvement on the old system, though the cost would probably limit the application to the few instruments required for very special and difficult investigations in microscopy. For high-class photomicrographic work, or where preparations had to be retained under observation for long periods of time, the new mechanism should be particularly useful, for the greater solidity of the general construction clearly pointed to greater

precision and increased stability.—Mr. Goodwin exhibited a form of eye-piece for the microscope which gave a large field with considerable magnifying power.—Mr. A. W. Bennett gave a *résumé* of a paper, by Mr. W. West, on the fresh-water Algæ of North Wales. The paper described a collection of fresh-water Algæ, chiefly diatoms and desmids, made in various localities in North Wales and Anglesey, and it furnished what was beyond comparison the richest list of desmids which had ever been prepared in this country.—Prof. M. M. Hartog's paper, on the state in which water exists in live protoplasm, was read.—A paper descriptive of the method adopted by Mr. Halford in mounting the spermatozoa of the Salmonidæ was read, and specimens in illustration were exhibited by the lantern.—Mr. E. M. Nelson exhibited on the screen several slides showing under high powers (× 1350) the bordered pits of *Pinus* and *Tilia.* He also exhibited a small series of slides to show the qualities of a new apochromatic $\frac{1}{4}$-in. objective with fluorite lenses and of 95 N.A., which had recently been made by Messrs. Powell and Lealand.—Mr. Mayall mentioned that the gathering which was to have taken place at Antwerp, in celebration of the 300th anniversary of the invention of the microscope, was unavoidably postponed until next year.

Paris.

Academy of Sciences, April 28.—M. Hermite, President, in the chair.—On a class of differential equations of which the general integral is uniform, by M. Emile Picard.—On the characteristic equation of nitrogen, by M. Sarrau. In previous communications the author pointed out that certain experiments with carbonic acid verified an equation analogous to those proposed by Van der Waals and Clausius to represent the relation between the pressure, p, the volume, v, and absolute temperature, T. The following is the equation—

$$p = \frac{RT}{v - \alpha} - \frac{K\epsilon^{-T}}{(v - \beta)^2};$$

where R, α, β, K, and ϵ are constants. A discussion of the experiments made by Regnault and by Amagat on nitrogen shows that its critical point may also be represented by this formula.—On the heats of formation and combustion of several nitrogenous bodies derived from albumenoid matters, by MM. Berthelot and André. The bodies experimented upon are glycollamine or glycocoll, alanine, leucine, tyrosine, asparagine, aspartic acid, and hippuric acid.—Researches on the condensation of benzine and acetylene vapour under the action of the silent discharge, by M. P. Schutzenberger. The benzine condenses into a clear, yellow, resinous solid. Analyses of the liquid employed and of the condensed product are given, and it is shown that the amount of oxygen contained in the latter could not have been taken up from the air, but must have passed through the glass tube.—On *Gomphostrobus heterophylla*, a coniferous prototype from the Permian of Lodève, by M. A. F. Marion.—Observation of Brooks's comet (α 1890) made with the Brunner equatorial at Toulouse Observatory, by M. E. Cosserat.—General theory of the visibility of interference fringes, by MM. J. Macé de Lépinay and Ch. Fabry. The consequences which follow from the theorem demonstrated are pointed out, and it is proposed to describe the experiments which verify them in a future communication.—On the phosphites and the pyrophosphite of lead, note by M. L. Amat.—The action of erythrite upon the alkaline alcoholates, by M. de Forcrand. The author gives a continuation of a previous paper, here discussing formulæ for the bodies discovered and giving thermal data which explain the behaviour of the new substances when heated.—The action of lead oxide upon toluene and the production of benzene, by M. C. Vincent. The paper treats of this reaction at temperatures below the melting-point of lead. The conclusions are drawn: (1) that oxide of lead attacks toluene below 335°, giving water, carbonic anhydride, and benzene; (2) that, at higher temperatures, less benzene and more stilbene and higher hydrocarbons are obtained; (3) that at a red heat, in addition to the above, hydrocarbons produced by the simple heat decomposition of benzene and toluene are obtained; (4) that diphenyl formed during this experiment in small quantity comes rather from the benzene formed by the action of oxide of lead upon the toluene than from benzene contained in the toluene employed.—Thermochemical researches on textile fibres (wool and cotton), by M. Léo Vignon.—Experiments relative to the loss and gain of nitrogen by fallow or cultivated land, by M. A. Pagnoul. The writer finds in the cases examined that the gain of nitrogen in two years is—(1) with bare soil 29 kg. per hectare; (2) with grass land 394 kg. per hectare; (3) with land laid down in clover 904 kg. per hectare.—Note by M. Ant. Magnin, on the parasitic castration of *Anemone ranunculoides* by *Æcidium leucospermum.*—On the discovery of a giant land tortoise at Mont Léberon, by M. Ch. Depéret.—On the action of the positive pole of a constant galvanic current upon microbes, and particularly upon the anthrax bacillus, by MM. Apostoli and Laquerrière. Among the conclusions drawn are the following: the heating effects of the current may be experimentally neutralized, but the destruction or weakening of vitality of the microbe still takes place; it is the positive pole only which acts upon the microbes, the negative pole and the intermediate space do not give any evidence of adverse action upon the organisms; the current *sui generis* has no effect upon the microbes; the action at the positive pole is due to the disengagement of acids and of oxygen, as will be shown in a further note.—On the existence of tuberculous endocarditis, note by M. Raymond Tripier.

Berlin.

Meteorological Society, April 1.—Prof. Schwalbe, President, in the chair.—Dr. Perlewitz spoke on the influence of the city of Berlin on local climatic conditions. To investigate this he had compared, for the year 1889, the meteorological records of two stations outside the city with those of three inside. As regards temperature, some allowance must be made for the fact that the exposure of the thermometers was not identical at all the stations. The differences in temperature between the city and the surrounding country were greater than for Vienna, the maximal difference showing itself in spring and summer, the minimal in winter. The differences were least at 2 p.m., greater at 7 a.m., and greatest at 9 p.m. The absolute humidity was much less inside the city than in the neighbouring country, and the difference was, as regards maxima and minima, the exact reverse of that which held good for the temperature; whereas, on the other hand, the relative humidity followed the same lines as for differences of temperature. The direction of the wind was generally different in the city from that in the surrounding country, but no definite relationship of the two could be deduced from the observations, and the same held good for the frequency and extent of clouds in the two localities. Thunderstorms were observed less frequently in the city than in the country, but here again it must be borne in mind that the conditions under which observations can be made in the former are much less favourable than for the latter.—Prof. Spörer spoke on the rotation of the sun, and came to the conclusion that the continued endeavours which have been made to determine the rotation of the sun from observations of sun-spots, cannot lead to any definite conclusions.

Physical Society, April 18.—Prof. du Bois-Reymond, President, in the chair.—Prof. Planck spoke on the difference of potential of two binary electrolytes. According to recent views there exists, in any uniform dilute solution of an electrolyte, a complete dissociation of the ions, the latter being in equilibrium, since the sum of the two electricities of the anions and kathions is equal and the osmotic pressure is everywhere the same, quite independently of the nature of the ions. The electrical charge of the ions and the osmotic pressure are the sole forces which are at work in the solution, and suffice to account for all the phenomena which take place inside it. But in order to calculate the above it is necessary to know the mobility of the ions; this has been determined experimentally by Kohlrausch for a large number of different ions, and he has also measured the electrical charge of the ions, this charge being independent of their nature. If the solution is not of uniform composition, the osmotic pressure leads to a movement of the ions from the more to the less concentrated parts of the solution. Now, since the mobility of the ions varies, being five times as great for hydrogen as for chlorine, it follows that a larger number of hydrogen atoms will pass from the more concentrated parts of the solution, than of chlorine. This, however, leads to an upsetting of the electrical equilibrium, and the electrical affinities work in a direction opposite to that of the flow of atoms. The speaker had developed a general mathematical formula to express what takes place in the case of two solutions of different concentrations which are in contact with each other through an intervening porous partition. By means of this formula he has

calculated the magnitude of the differences of potential which establishes itself between the two electrolytes. Applying the formula to a special case, and calculating the difference of potential from the observed rate of flow of the ions and their known electrical charge, he showed that the values thus obtained correspond very closely with those obtained by direct measurement of the difference of potential.

Physiological Society, April 25.—Prof. du Bois-Reymond, President, in the chair.—Dr. Heymans spoke on medullated and non-medullated nerves. The medullary sheath of the former is characterized by the myelin formations which it yields under the action of water and the dark coloration with osmic acid. This last reaction is common to lecithin, protagon, and cholesterin, all of which are found in the medulla. When, however, lecithin has been treated with osmic acid it can no longer be extracted from the nerve, whereas protagon and cholesterin may be extracted by alcohol at 70° C. By taking advantage of this difference in their behaviour it becomes possible to test the statement that lecithin occurs in the neurokeratinous network of the medullary sheath, while protagon and cholesterin are present in the meshes of the network. Experiment does not support the above statement. The speaker had further used the reaction with osmic acid (2 per cent. solution) to investigate the occurrence of non-medullated nerves in certain places in which their presence is a subject of dispute. He found them in the sympathetic and olfactory nerves of the pike, but in much smaller numbers in the former than is usually stated to be the case. In many of the sympathetic fibres he observed a sheath composed of protoplasm which stained brown with osmic acid. He finally discussed fully the transition of medullated cerebrospinal nerves into the non-medullated processes of the ganglia.—Dr. Cowl spoke on methods of recording the variations of blood-pressure in an artery. He criticized the various forms of apparatus in use, and pointed out the errors arising from the use of elastic connections so frequently employed, as a result of which it is impossible to register the exact moment at which the pressure is zero. He had constructed an apparatus in which this source of error is avoided, and which admits of extremely delicate adjustment. He finally exhibited curves to demonstrate the advantages of the newer instrument.

Brussels.

Academy of Sciences, March 1, 1890.—The following were the papers communicated:—Experiments made by Count Espiennes at Scy (Ciney), on the circulation of air during calm nights from the surface of broken ground, by M. F. Folie.—On certain inversions of temperature, and on the frost of September 16, 1887, at Spa, by M. G. Dewalque. In this and the preceding paper it is shown that cold strata of air lie in valleys, and many cases are given of places situated on elevated plateaus where the minimum temperature is habitually higher than that at places of less altitude lying in valleys.—Gustavus Adolphus Hirn, Associate of the Academy, born at Logelbach (Colmar), August 21, 1815, died at the same place, January 14, 1890. An account of his life and work is given by M. F. Folie.—Another obituary notice by M. F. Folie, on C. H. Buys-Ballot, born at Kloetingen, October 10, 1817, died at Utrecht, February 3, 1890.—On phillipsite crystals from sediments found in the centre of the Pacific Ocean, by M. A. F. Renard. In a previous note (February 1890) the physical characteristics and the composition of zeolite crystals from deposits in the Pacific were indicated; the author now shows the conditions under which the phillipsite and mineral matters which accompany it are found.—Determination of the variations in the coefficient and diffusion with temperature for liquids other than water, by M. P. De Heen. The liquids investigated are xylene, benzine, ethyl alcohol, amyl alcohol, amyl benzoate, and carbon bisulphide, at temperatures of 10°, 30°, 50°, 70°, and 90°.—On the nature of the polarizing matter of the beetroot in alcohol; rotatory power of pectous matters, by MM. L. Chevron and A. Droische. It is found pectine and its derivatives exercise an energetic action on polarized light; the rotatory power of these matters is three or four times greater than that of saccharose sugar.—Some properties of conics, by M. C. Servais.—On the centre of curvature of lines described during the displacement of a plane figure in its own plane, by the same author.—Solanidine from potato sprouts; preparation and properties, by M. A. Jorissen.—On semi-invariant functions, by Jacques Deruyts.

Amsterdam.

Royal Academy of Sciences, April 25.—Prof. van de Sande Bakhuysen in the chair.—M. T. Forster read a paper on the influence of smoking on tuberculous matter. He had formerly shown that tuberculous matter does not cease to be infectious after salting. Experiments subsequently made in his laboratory prove that salting *and* smoking do not kill the bacteria of tuberculosis. Not only tuberculous matter, but meat from tuberculous cattle is very infectious.—Prof. van de Sande Bakhuysen communicated an abstract of a paper published by him on an instrument for the determination of the absolute personal error in astronomical transit-observations.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Light, Heat, and Sound: C. H. Draper (Blackie).—Leçons sur l'Electricité, tome i.: E. Gerard (Paris, Gauthier-Villars).—A Hand-book of Descriptive and Practical Astronomy, vol. 2, 4th edition: G. F. Chambers (Oxford, Clarendon Press).—Official Year-Book of the Scientific and Learned Societies of Great Britain and Ireland (Griffin).—Electrical Influence Machines: J. Gray (Whittaker).—Electric Transmission of Energy, 2nd edition: G. Kapp (Whittaker).—Zoologische Ergebnisse einer reise in Niederländisch Ost-Indien, Erstes Heft: Dr. Max Weber (Leiden, Brill).—Memoirs of the Geological Survey of India—Palæontologia Indica; ser. xiii., Salt Range Fossils; vol. iv. Part 1, Geological Results: W. Waagen (Trübner).—The Criminal: H. Ellis (W. Scott).—Notes on the Pearl and Chank Fisheries and Marine Fauna of the Gulf of Manaar: E. Thurston (Madras).—Food Adulterations: A. J. Wedderburn (Washington).—The Beginnings of American Nationality: A. W. Small (Baltimore).—Journal of the Royal Statistical Society, March (Stanford).—Traité Encyc. de Photographie, 15 April (Paris, Gauthier-Villars).—Journal of the Anthropological Institute, vol. xix. No. 3 (Trübner).—Brain, Part 49 (Macmillan).—Mass. Institute of Technology, Boston, Annual Cat., 1889-90 (Camb., Mass.).—Journal of the Royal Microscopical Society, April (Williams and Norgate).—Bulletin of the American Geological Society, vol. 21, Supplement 89, vol. 22, No. 1 (New York).—Missouri Agricultural College Experiment Station Bulletin, Nos. 6 and 9 (Columbia, Miss.).

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THURSDAY, MAY 15, 1890.

THE ALTERNATE CURRENT TRANSFORMER.

The Alternate Current Transformer in Theory and Practice. By J. A. Fleming, M.A., D.Sc. Vol. I. "The Induction of Electric Currents." Pp. 479. (London: The *Electrician* Printing and Publishing Company, Limited, 1889.)

THE alternating current has of late years sprung into great commercial importance, and accordingly the laws regulating its flow, long known to a few, are becoming recognized and assimilated by the many. The behaviour of alternating currents is so vastly more complex than anything which had to be dealt with in the time-honoured chapter of the text-book concerning "divided circuits" for the case of steady currents, that a new literature has arisen, and a number of half-accepted new terms have been coined.

It is evidently with the aim of making accessible to average readers the greater portion of this subject that Dr. Fleming has put together the above-named book.

The volume is distinctly a compilation, a *réchauffée* of recent work, though it consists partly of a reprint of the author's own articles in the *Electrician*, and it is a compilation of a very useful kind. It brings together a quantity of floating information collected with a keen scent for practically useful items, as well as for topics of contemporary interest.

It is hardly a book to be recommended to the student as a logical treatise. The proof of the laws is a secondary consideration, and though proofs are indicated, it is more to link them on to other things than really to justify and deduce them. In fact the book has the air of being hastily written; but the facts are there, whether deduced rigorously or not, and the practical man, for whom it is written, will not be likely to find fault. In several instances, however, it can be claimed that the presentation of doctrines is as clear as could be wished and as the ability of the author would lead us to expect. Students will undoubtedly be glad of a book which contains so much useful information only accessible otherwise with difficulty.

The early part of the book, dealing with the modern treatment of magnetism, hysteresis, and the like, is fairly good, but is probably now superseded by some still more recent articles by Prof. Ewing himself. The next portion, on simply periodic currents, is very instructive. In it Lord Rayleigh's investigation of the laws of branching for alternating currents is incorporated, and the result applied to determine the correcting factor for a watt-meter; a clear explanation being given of why it is so difficult to measure the average power of an alternating current. Some of Mr. Blakesley's geometrical representations are also utilized; and the whole circumstances of a simply alternating current are very clearly explained.

Then comes an abstract of some of the too-long-buried researches of Prof. Henry, which the recent publication of his memoirs by the Smithsonian Institution has brought into prominence.

A brief account of the experimental investigations of Masson and Breguet, Blaserna, Helmholtz, on transient currents, of Hughes on the induction balance and on the throttling effect of iron wire, follows; together with the Maxwell-Rayleigh-Heaviside theory of the same.

We then come to the main subject of the book—the laws of mutual induction and the theory of induction coils or transformers. Here the author enters into very instructive detail, showing how to deal with transformers containing iron, and incorporating the researches of Hopkinson, Forbes, Sumpner, Ferraris, and Kapp, as well as the general theories of Maxwell and Lord Rayleigh.

A chapter headed "The Dynamical Theory of Current Induction" follows, wherein Prof. Garnett's summary of Maxwell's electro-magnetic ether models is reproduced; some instructive explanations of many Maxwellian ideas is given in a much clearer and more elementary form than is frequent; some experiments and articles of the present writer are incorporated; and lastly, Mr. Tunzelmann's abstract of Hertz's papers is reprinted once more. It is to be regretted that we have not the advantage of a fresh abstract and discussion of these papers by Dr. Fleming himself. So much in Hertz's papers was confessedly crude and tentative that it would have been much more satisfactory to have had a real digest from a contemporary point of view, instead of the useful but now out-of-date summary with which most persons were already familiar.

Perhaps also it may without offence be suggested that a free use of quotation marks in this last chapter, possibly in other chapters also, would not have been out of place. One is a little startled to find whole paragraphs and diagrams incorporated into a book without rather more explicit statement concerning their origin. I may instance pp. 380, 408, 409, among others, as having struck me personally with some surprise, though very likely the feeling was unjustifiable.

It may be useful if I record such trifling slips as I have noticed. Quite possibly some are not erroneous at all. On p. 7 the assertion is made that Faraday "came to see that that which he had denominated the *electrotonic state* is really the amount of electro-magnetic momentum which the circuit possesses." As a matter of history this is surely incorrect? And is there any evidence for the statement on p. 2, that Faraday's failure to obtain current induction in 1825 and 1828 was because his galvanometer circuit was not closed? It seems very improbable. At the bottom of p. 67 the argument seems to me incorrect and confusing. In the diagram on p. 140 *current* should appear as a factor in the lengths OB, BA, &c. On the top of p. 145, there is no need for L and N to be *both* zero in order that the wattmeter factor, F, may equal unity; it is sufficient if the time constant of the fine wire shunt alone vanishes. On p. 209 a p is twice dropped out of an equation. On p. 253 the statement is made "that we may regard the inductance of a conductor as an effect which is due to the fact that the current takes time to penetrate into the conductor"—a statement which is by no means true. And two pages on we read, "as the frequency of alternation is increased, the resultant self-induction of the circuit is lessened, but [? so that] although the true resistance is increased, the impedance may be diminished on the whole." It may, however, be more truly asserted that no

increase in frequency can diminish impedance: it always tends to increase it; and in no case can the impedance of a conductor to alternating currents fall below that felt by steady ones. Both resistance and impedance increase with frequency. It is true that inductance diminishes, but the diminution is very slight except for iron rods. The punctuation of p. 353 has gone somewhat astray.

All these are trifles: the average level of the book is high, and it contains few dull pages. The practical importance and interest of the subject treated is so great that there should be little need to urge students and electrical engineers to make themselves acquainted with it, but I do urge them nevertheless; and they may think it fortunate that Dr. Fleming has managed to find time to issue so instructive and readable and well-timed a volume.

OLIVER J. LODGE.

McKENDRICK'S "SPECIAL PHYSIOLOGY."

Special Physiology, including Nutrition, Innervation, and Reproduction. Vol. II. By J. G. McKendrick, M.D., LL.D., F.R.S. (Glasgow: Maclehose and Sons. London: Macmillan and Co. 1889.)

IN the first volume the only purely physiological part was that on muscle, leaving all the rest of the science to be treated of in this volume, which thus includes the physiology of digestion, nutrition, blood and circulation, respiration and the nervous system, as well as reproduction. It is evident that the book must either be of an entirely elementary character, or that the treatment must in parts be extremely inadequate, in order to include all these subjects within the dimensions of a moderate-sized volume. As a matter of fact, it lies open to both these objections. In some places the author hampers himself in the treatment of the purely physiological part of the subject by expounding the first elements of chemistry and physics (for the benefit, I suppose, of the average Glasgow student); while other parts, though good, are much too condensed to be understood by the reader who is ignorant of the first principles of science. This disproportion in the treatment of the various subjects meets us at the very beginning of the volume, where twenty pages are devoted to dietetics before any description has been given of the processes of digestion.

In the section on digestion a very good condensed account is given of the theory of secretion. One is surprised, however, to meet with the statement that the submaxillary ganglion can act as a reflex centre. The importance of this view for the physiology of sporadic ganglia is enormous; yet Prof. McKendrick is content with describing Claude Bernard's old experiments, which seemed to support it, and makes no mention of the researches of Bidder and Schiff, made so long ago as 1867, which showed that the secretion obtained in Bernard's experiments was due to recurrent fibres of the chorda tympani, and not to any action of the ganglion as a reflex centre.

The account of the action of the bile on the chyme is not quite accurate. He says: "At the same time the taurocholate of soda throws down the non-peptonized albuminous matters, such as coagulable albumin and syntonin, while the hemialbumose and peptones remain in solution." As a matter of fact, the precipitate consists of parapeptone (syntonin) with a small quantity of peptones.

In describing the formed elements of the blood, no mention is made of the plasma or granule cells. Yet these are daily assuming a larger importance in pathological processes, and every student who is to study medicine should at any rate know of their existence.

In the section on coagulation as fair an account is given of Wooldridge's work on the subject as is possible in the limits of a page and a half; but in his summing up of the differences between this observer and Halliburton, he does not seem to have grasped Wooldridge's theory. He rejects this on the grounds that all Wooldridge's work was done with peptone plasma (which was not the case), and that fibrinogen (Hammarstens) contains no lecithin and can yet clot on addition of lecithin free ferment. That fibrinogen contains no lecithin is, to say the least, extremely doubtful. Bunge states that he has never succeeded in preparing any proteid free from phosphorus. It is practically impossible, however, to form a good judgment on the merits of Dr. Wooldridge's work without reading all his papers on the subject. In none of them has he discussed the question in all its details, and it is probably on this account that his work has been so misunderstood and misrepresented.

It is surprising how few books on physiology mention the *rôle* of the spleen (made so much of by Metschnikoff) as the great *blood-filter*, where all effete blood corpuscles and other deleterious materials are devoured and destroyed by the cells of the splenic pulp. In this volume the rhythmic movements of this organ are fully described, and a long list is given of the extractives that it contains, but its function is left entirely in doubt.

The next two sections, on the circulation and respiration, present the subject fairly completely, and are brief without being obscure. Yet these are not free from some misleading statements. Thus the depressor nerve is included among the afferent nerves that act on the inhibitory or accelerating cardiac centres, so that a student would imagine that stimulation of this nerve lowered the blood-pressure by reflex inhibition of the heart—a mistake one meets with only too often in teaching. Again, in describing the forces concerned in carrying on the circulation through the capillaries, he makes the following statement:—

"Some have supposed that it is supplemented by an attractive influence exerted by the tissues (*vis a fronte*), and the statement is supported by the observation that, when there is an increased demand for blood owing to active nutritional changes, there is an increase in the amount of blood flowing to the part, such as occurs, for example, in the mammary gland during lactation, and in the growth of the stag's horn. Such an attractive influence on the part of the tissues is quite conceivable as a force assisting in the onward flow of blood, acting along with capillarity."

It is rather hard to see how an attractive influence on the part of the tissues can assist in the onward flow of blood, even when it *is* assisted by capillarity. At any rate this statement is sure to be devoured greedily by the studious fool, who will thereafter reproduce it on all possible occasions, probably as the chief factor in the circulation of the blood.

In discussing the nervous mechanism of respiration,

the author confines himself almost entirely to an exposition of Marckwald's views. Apnœa is referred to a hyperoxygenation of the blood, no mention being made of the fact that it may be produced by positive ventilation with any inert gas, and so must be mainly a reflex effect.

Surely, too, in the treatment of the changes in the blood, the researches of Bohr, Blix, and others, on the combination of hæmoglobin with CO_2, were worthy of note.

In the section on the kidney, about 30 pages are devoted to an elaborate description of the normal and abnormal constituents of urine, with their tests and quantitative determination, while the subject of the process of secretion itself is dismissed in the ridiculously small space of three pages.

The final section, on the nervous system, is one of the best parts of the book. Especially good are the pages treating of the special senses. The chapters on the spinal cord and brain are less complete, and present several omissions and errors. Thus no mention is made of the perfectly definite antero-lateral ascending tract, and the knee-jerk is referred to as a true reflex, which is, to say the least, highly dubious. Again, the statement is made that clots in, or lesions of, the corpus striatum cause hemiplegia, whereas this is rarely or never the case unless the internal capsule is also implicated.

Throughout the work the author lies under the disadvantage of having tried to cater for two distinct classes of students, beginners and those who have already a fair general knowledge of the subject. For the former the work is too large and not sufficiently accurate; for the latter, in most parts, too meagre. Still it will be found useful by many of the latter class who have enough critical power to eschew the evil and choose the good, and will serve them as an excellent introduction to the reading of original memoirs.

E. H. S.

OUR BOOK SHELF.

I. *Historia Naturalis Itinerum N. M. Przewalskii per Asiam Centralem.* Augustissimus auspiciis sumptibusque ab Societate Imperiali Geographica Rossica edita. Pars Botanica elaboravit C. J. Maximowicz. Volumen I. "Flora Tangutica." Fasciculus 1, Thalamifloræ et Discifloræ. 4to, pp. 110, cum tabulis 31. II. Volumen II. "Enumeratio Plantarum hucusque in Mongolia nec non in adjacente parte Turkestaniæ Sinensis lectarum." Fasciculus 1, Thalamifloræ et Discifloræ. Pp. 138, cum tabulis 14.

III. *Plantæ Chinenses Potaninianæ nec non Piasezkianæ* (*Acta Horti Petropolitani*, Vol. XI., pp. 1–112). Elaboravit C. J. Maximowicz. (St. Petersburg Botanic Garden, 1889.)

Here are three separate contributions to the flora of Eastern Central Asia, by the well-known authority on the botany of Central and Eastern Asia. It is now nearly forty years since Mr. Maximowicz, through the force of circumstances, had an opportunity of exploring Mandshuria, the results of which he published under the title of "Primitiæ Floræ Amurensis." He was attached as botanist to the Russian frigate *Diana* on a scientific voyage round the world, but in consequence of war breaking out with England and France he was landed in Mandshuria, where he spent three or four years, returning through Siberia and European Russia to St. Petersburg. Subsequently he has visited Japan two or three times, and made large collections of dried plants, and his life, apart from official duties, has been devoted to working out the botany of temperate Asia.

It was known to botanists that he was engaged on the collections made by the renowned Russian explorer Przewalski and others, and we now have the first instalments in a connected form. Many of the novelties he had previously published in the *Mélanges Biologiques* and elsewhere. From the titles cited above, it will be seen that the plan of publication is, if not exactly an ambitious one, at least very laborious, involving much repetition. Possibly such conditions were imposed upon the author. Anyhow, it seems a great pity that the materials were not all worked up in one enumeration. This course would have been far preferable from a practical standpoint, and, what is of greater importance, there would have been a reasonable prospect of its being finished within a few years. With all Mr. Maximowicz's capacity for work, it seems unlikely that he can hope to reach the end on the present elaborate scale. The aggregate of the two quarto publications is 250 pages, and contains the Thalamifloræ and Discifloræ of the collections. At the outside, this is only a sixth of the flowering plants. Then there is the octavo enumeration of Chinese plants brought down to the same point, and this is not the whole of Mr. Maximowicz's botanical work in hand. Recently he issued a monograph of the genus Pedicularis, comprising about 250 species, nearly 100 of which were new, and these mostly Chinese. When it is added that Mr. Maximowicz is a very critical worker, some idea may be formed of the magnitude of the task he has undertaken.

Glancing over the pages we find that the novelties consist almost entirely of new species of old genera, chiefly of those of a wide range, in the northern hemisphere, at least. In fact, only two new genera are described: *Clematoclethra*, near *Actinidia* (which Maximowicz places in the Dilleniaceæ in preference to the Ternstrœmiaceæ), and *Tetraena*, an obscure plant provisionally referred to the Zygophyllaceæ. New genera are more numerous in Dr. Henry's collections from the warm temperate regions of Central and Western China.

W. Botting Hemsley.

Le Glacier de l'Aletsch et le Lac de Märjelen. By Prince Roland Bonaparte. (Paris: Printed for the Author, 1889.)

In this ample pamphlet the author gives an account of the well-known glacier of the Aletsch and the neighbouring mountain region, in the course of which it is incidentally mentioned that the glaciers are again showing signs of increase after a period of general retreat which began in 1854. This statement, I think, requires some qualification, for the Gorner glacier certainly was advancing about the year 1859. The most marked diminution occurred in the next decade, and it did not commence till, at any rate, after 1861. The author describes the curious Märjelen See, which has already been noticed in these pages (vol. xxxvi. p. 612), giving some statistics as to its area, depth, &c. He quotes also a list of the occasions, so far as known, since 1813, on which its waters have escaped beneath the Aletsch glacier. In this, however, there is either an omission or a misprint. It states that in 1859 *le lac se vide*. This may be true, though it seems improbable, for the lake was also drained in 1858. In the latter part of August in that year I saw it for the first time. It was then full. The next evening I again visited the lake. The water had almost all vanished, and the great blocks of ice were stranded on the muddy floor. In reference to this floor the author makes a statement which I fail to understand: "Le bassin du lac est une ancienne moraine de fond d'une des branches du glacier de l'Aletsch. Unless this mud be claimed as *moraine profonde*—and this I should dispute—the assertion seems to me without any valid foundation. The lake lies in the upper part of a small valley, worn by the passage of ice

into a shape something like the pointed half of the bowl of a spoon. Another statement appears to me of questionable accuracy. The author notices the earth pillars on the southern slopes of the Eggishorn, describing them correctly, but saying of them, "Les pyramides des fées, aussi appelées 'blocs perchés.'" Surely this is an unwonted extension of the latter term.

The pamphlet, in short, is rather disappointing. It is beautifully printed on quarto pages with large margins, and is illustrated with three photogravures of glacier scenery, which would be improved by the omission of the human figures, for these by contrast look like negroes in mourning; but it tells us little that is new, and is a "popular" article rather than a scientific memoir.

T. G. Bonney.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Panmixia.

Private communications which I have received from naturalists interested in this controversy, and from Mr. Romanes himself, have thrown light on the apparently irreconcilable difference of the views which have been expressed.

I think it desirable that an explanation should be afforded to the readers of Nature.

When Mr. Romanes contends that cessation of selection leads to a dwindling in the size of a useless organ, he now tells me that he assumes that the mean size of the part in all born (what we may call the birth-mean) was smaller than the mean size of that part in those individuals surviving under selection. Hence the withdrawal of selection substitutes in the adult survivors the lower birth-mean for the former higher selection-mean.

Mr. Romanes had not specifically stated that he made this assumption.

On the other hand, I had—for the purpose of estimating purely and solely the result of panmixia and cessation of selection—assumed that birth-mean and selection-mean were identical, in which case the withdrawal of selection would, of course, not alter the mean.

To assume that birth-mean is smaller than selection-mean in a given case seems to me to be introducing causes other than panmixia or cessation of selection.

It is evident that cases are *possible* in which the mean given by selection is identical with the birth-mean—others in which it is *smaller* than the birth-mean, and others in which it is *larger*. Special causes of a complex character determine whether the ratio is one or the other. If we are to consider the effects of cessation of selection alone, apart from other causes, it seems to me that we must not introduce causes which affect the ratio of birth-mean and selection-mean; we must eliminate them altogether by assuming the ratio to be one of equality. Hence my conclusion that panmixia or cessation of selection alone cannot produce the dwindling of an organ.

If, however, we admit the assumption that the selection-mean is larger than the birth-mean, Mr. Romanes has my full concurrence in stating that cessation of selection leads to dwindling, and I am of course aware that, given that assumption, Weismann and Galton are of the same mind.

The point of interest therefore shifts. The question is, whether we are justified in assuming that in organisms generally in a state of nature the mean size of an organ or part in the selected survivors is larger than in all born, or, to put it fully, larger than would have been the mean size of the part in all born supposing that they had all reached maturity.

I do not think that we have data which warrant this assumption. It is, I think, certain that some cases must sometimes occur in which this is the case, and others in which the selection-mean-size is *smaller* than the birth-mean-size. It is not improbable that in well-established species there is identity of the two means. This is, however, a question which ought to be settled by observation—*not* of domesticated races, but, if possible, of wild forms.

It seems to me that this assumption is precisely what Mr. Darwin considered, and refused to make, so that he avoided attributing dwindling of parts to the cessation of selection. He says ("Origin," 6th ed., p. 401): "If it could be proved that every part of the organization tends to vary in a greater degree towards diminution than augmentation of size, then we should be able to understand how an organ which has become useless would be rendered, independently of the effects of disuse, rudimentary, and would at last be wholly suppressed." Mr. Darwin says, "If it could be proved." This is really the whole point. If the greater size of selection-mean than of birth-mean could have been proved, Mr. Darwin was ready to formulate the doctrine of dwindling by cessation of selection. But, apparently, it could not be proved then. It has not been proved yet. I do not think it at all impossible that it may be proved. The facts are as yet not recorded.

E. Ray Lankester.

May 10.

Bertrand's Idiocyclophanous Spar-prism.

It is a good thing that Prof. Silvanus Thompson has brought the above prism to the notice of the Physical Society (see Nature, vol. xli. p. 574); it is certainly remarkable that M. E. Bertrand himself has never thought fit to publish any description of his interesting invention. Perhaps it may be worth while to mention a fairly simple method of constructing the prism (which may easily have occurred to others besides myself, and) which has the advantage of requiring only two artificially-worked surfaces, and hence of interfering as little as possible with the natural rhombohedral crystal of Iceland spar.

Four plane, polished faces are required for the prism, which is, in fact, a four-sided parallelopipedon, having two opposite sides parallel to the optic axis, while the two others make an angle of 45° with it.

Now, since in Iceland spar the faces of the natural rhombohedron make angles of very approximately 45° (strictly, 45° 24′) with the optic axis, two of these faces can be utilized for the last-mentioned pair of prism-sides.

Take, then, a cleavage-rhomb of spar, about 1 cm. in thickness, and having edges about 4 cm. in length (Fig. 1); observing

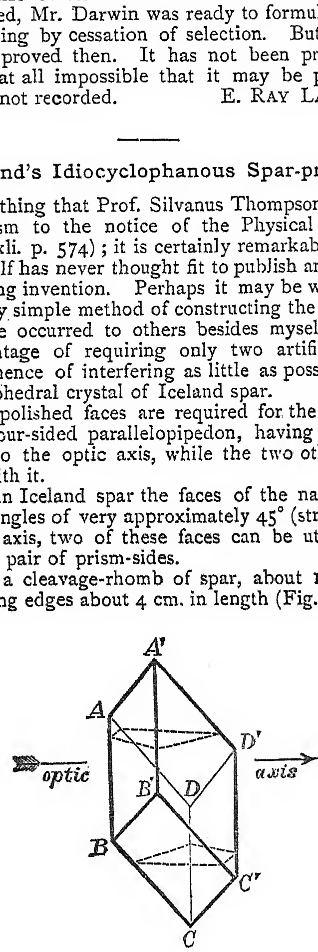

Fig. 1.

that both the face A B C D and the opposite one, A′ B′ C′ D′, are flat and free from blemishes (such a crystal is easily found, even in these spar-famine days). Grind away the solid angle A′ down to about the level shown by the dotted lines, working the face thus obtained so that it makes an angle of 45° with the natural face A B C D. Cut away the opposite solid angle C in a similar way, so as to make another plane, parallel to the first. Polish the two cut surfaces, and the prism is complete in all essential particulars.

Thus, if a beam of common white light is allowed to fall normally on one of the worked surfaces, A, Fig. 2 (which is a section of the prism), it will be (1) totally reflected at the natural face B (corresponding to A B C D in Fig. 1); (2) pass on through the crystal parallel to the optic axis; (3) undergo another total reflexion at the opposite natural face C; and (4) finally emerge through the second worked plane D. An eye placed close to D will then observe the well-known pair of ring-systems side by side, one set complementary to the other.

A very convenient source of illumination seems to be a lamp-

flame within a globular opal shade, placed at such a distance that the three images of it produced by the action of the prism (the centre image formed, of course, by the superposition of two, similarly polarized) just touch each other. Two of these images are then filled (like a lantern-disk) with the complementary ring-systems; and by a very slight motion of the crystal the rings pass from a given disk to the adjacent one, becoming complementary in so doing. (It is hardly necessary to explain, for no doubt Prof. Thompson did so fully, that the whole prism is precisely

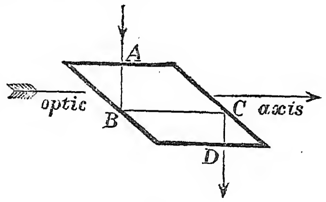

Fig. 2.

equivalent to a train of two double-image prisms with refracting angles of 45°, having between them a plate of spar with surfaces at right angles to the optic axis; a "Huyghens apparatus," in fact, with an interposed spar-plate instead of the usual selenite film.)

I may add that I have found it convenient, in order to demonstrate the principle of the prism, to divide it into halves; or, more strictly, to cut a piece of spar so as to form one half of the prism only, as shown in Fig. 3. Then, if common light from

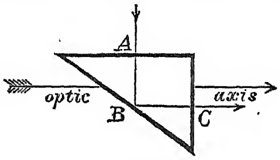

Fig. 3.

the lamp-shade (as described above) is allowed to enter the face A, and a tourmaline is held in the path of the rays emergent from C, ring-systems are seen just as when a double-image prism is used as a polarizer and a plate of spar held in front of it. Also, if plane-polarized light is allowed to enter C, and the eye is held close to A, ring-systems are seen side by side; that portion of the spar through which the rays pass after total reflection at B acting, of course, exactly as a double-image analyzer. In fact, the prism may, in this position, be used alone as a "Savart's polariscope" to detect traces of polarization in sky-light, &c. But for this application, the prism would possess, in the eyes of the true votary of science, the inestimable value of being of no practical utility whatever.

Queen's College, Oxford. H. G. Madan.

Coral Reefs, Fossil and Recent.

Dr. von Lendenfeld's account of the dolomites of the Italian Tyrol, in his letter on "Coral Reefs, Fossil and Recent," is a very valuable contribution to this interesting question; but I think he can hardly have fallen in with the new edition of Darwin's "Coral Reefs," or he would not have asserted that in the discussion "the structure of our Triassic limestone mountains has been left out of account." In the appendix (p. 332) I wrote:—"If those geologists are right who consider the Schlern dolomites as being to a great extent due to reef-building corals, we have, in the Triassic deposits of the Italian Tyrol, reefs thick enough to satisfy the most exacting requirements." I could not venture upon a more positive statement, because I knew controversy on this question was not ended, and I had not myself, though fairly familiar with the "Dolomites," discovered evidence which appeared to me conclusive (though I incline to the above opinion myself), and because I considered that the view advanced several years since by Richthofen required some modification—indeed, as to one detail, if I understand him rightly, I should differ from Dr. von Lendenfeld.

I am confirmed in my idea that he has not read this book, because I find that one of his chief arguments—that against the indefinite lateral extension of a coral reef on a talus of its own building—appears to correspond with one advanced by myself on p. 327, differing only in the addition of an arithmetical example; one of which, indeed, I did work out, but afterwards suppressed as needless, the truth of the statement being obvious when it was once pointed out. T. G. Bonney.

Bison and Aurochs.

In regard to Prof. Newton's letter in your issue of the 8th, I beg to state that in restricting the name aurochs to the European bison, I have merely followed the general custom of English zoologists.

Citing a few authorities, I may first make the following extract from a paper by Prof. W. B. Dawkins, published in the Quart. Journ. Geol. Soc., vol. xxii. p. 394 (1866). There, after alluding to the Indian gaur, this author writes, "the term *Aurochs* has been restricted to the European bison by the authority of Buffon, Cuvier, and Prof. Owen; the term *Urox* or *Bos urus*, to the species under consideration [the extinct wild ox of Europe] by Julius Cæsar, Pliny, . . . also by Cuvier, Nilsson, and our great naturalist, Prof. Owen."

Again, in the article on Ruminants by the late Prof. Garrod in "Cassell's Natural History" (2nd ed., p. 35), the term aurochs is applied to the European bison. Finally, we find in Prof. Flower's "Catalogue of Mammalia in the Museum of the College of Surgeons," p. 232 (1884), the animal in question mentioned as the *European Bison* or *Aurochs*.

I find, however, that modern German zoologists (see Brehm's "Thierleben," vol. iii. p. 386) consider it proved that the name *Aurochs* belongs properly to the extinct *Bos primigenius*; and they term the bison, as Prof. Newton states, the *Wisent*. If this be really correct, English zoologists must accept the emendation. R. Lydekker.

The Lodge, Harpenden, Herts.

The Haunts of the Gorilla.

Concerning Mr. Du Chaillu's saying (see Nature, May 1, p. 19) "that, so far as he is aware, no white man has been able since his time to penetrate to the haunts of the gorilla and bring home specimens killed by himself," I beg to remark that Herr von Koppenfels, in the years 1873–80, killed personally a number of gorillas in the environs of the Ogowé, and sent 3 large specimens, with their skeletons, to the Dresden Museum, some of which I described in the *Mittheilungen aus dem königl. zoologischen Museum zu Dresden*, vol. ii. 1877, p. 230 *seq*. The Museum in Stuttgart also contains several specimens killed by that intrepid traveller; and other museums, I believe—American museums, for instance—possess some. (See also his remarks in the *American Naturalist*, vol. xv. p. 447; and *Die Gartenlaube*, 1877, p. 416 *seq*., with plate; as well as mine in *Der zoologische Garten*, 1881, vol. xxii. p. 231.) Herr von Koppenfels, who died in the year 1884 in Erfurt, in consequence of diseases acquired in the tropical climate, says (*l.c.*) that the haunts of the gorilla in West Africa are in the forests between the mouths of the Mimi and the Congo Rivers, *i.e.* between 1° N. lat. and 6° S. lat. How far the region extends into the interior is even yet unknown. A. B. Meyer.

Royal Zoological Museum, Dresden, May 7.

Flat-fishes.

Mr. Gulick, in Nature, vol. xli. p. 537, has raised a puzzling point about the flat-fish. In the case of his two Japanese species, it might appear that the ancestor of them both varied in the two directions as to the position of its eyes, &c., and that by the segregation of each form, *neither of which had any advantage over the other*, two species eventually were evolved. But this is not so clear in other cases, apparently. On the American coast of the Pacific, there is a flat-fish, *Paralichthys californicus*, Ayres, which is said by Messrs. Jordan and Goss to be almost as frequently dextral as sinistral. Here, then, is the same sort of variation exactly, yet we see no evidence of segregation and the formation of new species. In the whole sub-family *Soleinæ*, the eyes and colour are on the right side: now, if the "dextral" soles segregated themselves, having no advantage in being dextral rather than sinistral, what has become of all the sinistral ones? If there was no natural selection at play, ought we not to get some sinistral species of *Solea*? Perhaps it may be said that *Solea*, as such, never varied in this way, and was always dextral. But this cannot be so, since we have it on Day's authority that the common sole has a reversed aberration. But, after all, the allied *Cynoglossinæ* are sinistral soles.

Taking the *Pleuronectidæ* as a whole, we certainly get a division into dextral and sinistral groups, which might be supposed to be the result of a "like to like" segregation at an early period. The following table of the sub-families, compiled from Messrs. Jordan and Goss's excellent work on these fishes, illustrates the point :—

(1) *Hippoglossinæ:* normally dextral, except the tropical species, which are sinistral.

(2) *Pleuronectinæ:* sinistral flounders.

(3) *Oncopterinæ:* dextral.

(4) *Platessinæ:* dextral, but *Platichthys stellatus* is frequently sinistral.

(5) *Soleinæ:* dextral soles.

(6) *Cynoglossinæ:* sinistral soles.

But how comes it that the tropical flounders are nearly all sinistral, while the Arctic and Antarctic ones are chiefly dextral?

It would be interesting to know more of the reversed aberrations which occur. *Platessa flesus* var. *passer* (Linn.) is a reversed form, and Day records reversed aberrations of *Solea solea*, *Pleuronectes rhombus*, and *P. maximus*, while others have been noted by various writers. T. D. A. Cockerell.

West Cliff, Custer Co., Colorado, April 25.

Variation in the Nesting-Habits of Birds.

In connection with T. D. A. Cockerell's letter, *re* the nesting-habits of the blackbird in Colorado, it may be of interest to note that in the grounds around our residence about a fortnight ago, I discovered the nest of a blackbird (*Merula vulgaris*) built upon the ground close to a boundary wall about five feet high. The bottom of the nest is resting on the ground, but there is some trailing ivy growing around—but not on the wall—which supports it at sides and partially obscures it. There is a public road on the other side of the wall and the noise of considerable traffic. There are many suitable trees, bushes, and shrubs, all around, some of which have been utilized by other blackbirds—indeed, there is a tree within a few feet of the nest, which would have been suitable but for the chances of observation from the road. Thos. Swan.

Bankplace House, Leslie, Fifeshire, Scotland, May 7.

Doppler's Principle.

In answer to a correspondent who has met with a difficulty in the consideration of Doppler's principle, I may say that I think I fairly solved the difficulty in a paper delivered last year before the University College Chemical and Physical Society. In cases (1) and (2) of your correspondent, viz. approach or recession of observer, source and medium being at rest, the correct formula is $n' = n\frac{a \pm v}{a}$; and in cases (3) and (4), viz. approach and recession of source, observer and medium at rest, it is $n' = n\frac{a}{a \mp v}$, n' = the new, and n the old frequency of vibration of the note heard. It should be remarked, however, that in all practical cases, the two formulæ give very nearly the same result; but if the velocity v is very great, the case is entirely different. Suppose, for instance, in cases (1) and (2) that v = velocity of sound a, then $n' = 2n$ for approach, and 0 for recession of the observer. The correctness of these results is obvious without the aid of any formula. Again, in cases (3) and (4) suppose $v = a$, then $n' = \infty$ for approach, and $\frac{n}{2}$ for recession of the source of sound. The effect of an infinite number of waves striking the ear at the same moment would be simply that nothing would be heard. It would be interesting to notice the change in pitch of the whistle of a rifle-bullet passing near an observer. Ganot's formula is correct for cases (1) and (2), and Prof. Everett's for cases (3) and (4); the proofs are very simple, and may be easily thought out. When the observer and the source of sound both move, the two formulæ should be applied separately when a very accurate result is desired. These conclusions have been confirmed by Dr. Fison, of University College. I had not considered the effect of the motion of the medium, but it appears to me, after a little reflection, that this would increase or diminish the velocity of the sound, and the wave-length, in the same proportion, leaving the pitch unaltered; the velocity of the medium should therefore be added to or subtracted from a in the formula.

University College, May 5. E. P. Perman.

"Index Generum et Specierum Animalium."

Naturalists have long needed a reference book to the names of genera and species. Such a want has already been partially supplied by Agassiz, Bronn, Morris, Marschall, Scudder, Waterhouse, and others—only Bronn and Morris having attempted palæontological species—but no one book including references to all names given to living and fossil animals has yet been attempted. Botanists, more fortunate, will soon possess Daydon Jackson's index to flowering plants. The idea has therefore suggested itself to me to begin at the end of June next, such an "Index Generum et Specierum Animalium," taking the following rules for guidance :—

(1) The earliest reference is to date from the twelfth edition of Linnæus, 1766.

(2) The last reference to close with December 31, 1899.

(3) The names of genera and species to be given in a single alphabetical sequence, and accompanied by a reference to the original source.

(4) The names of species of each genus to be also quoted in alphabetical order under that genus.

(5) No attempt at synonymy to be given; but, to assist reference, the various genera in which a species has from time to time been placed, to be indicated under that species.

(6) Pre-Linnæan names to be quoted as founded by the author first using them after 1766:—*e.g.* *Echinocorys*, Leske, 1778 (*ex* Klein, 1734). Should a pre-Linnæan species or genus have been re-named after 1766, before the post-Linnæan use of that pre-Linnæan name, the new name is to stand. [References will be given to Artedi, Brisson, and Scopoli, in accordance with British Association rules.]

Among the many offers of assistance, that of Prof. Flower, F.R.S., Dr. Günther, F.R.S., and Dr. Henry Woodward, F.R.S., who have promised the necessary space for the storage of the MS. in the Natural History Museum, is most valuable, as it practically ensures safety from fire, and renders the MS. easily accessible to those wishing to consult it while still imperfect.

The contribution of inaugural addresses, theses, or other publications difficult to obtain, would be of great assistance; and, after use, such pamphlets would be handed over to the library at the Museum.

Any suggestions for the improvement of this plan, before the commencement of the undertaking, would be gladly received and carefully considered.

Appended is a rough outline of the scheme :—

[cordatus -a, -um]
——— Amphidetus (Penn.) Düb. and Koren, Zool. Bid. 285 1844
  [*v.* Echinus]
——— Amphidotus (Penn.) E. Forbes, Brit. Starf. 190, fig. 1841
  [*v.* Echinus]
——— Echinocardium (Penn.) J. E. Gray, Cat. R. Ech. 43 1855
  [*v.* Echinus]
——— Echinus, Pennant, Brit. Zool. iv. 58, xxxiv. 2, xxxvi. 2 1777
  [*v.* also Amphidetus, Amphidotus, Echinocardium, Spatangus]
——— Spatangus (Penn.) Flem. Brit. Anim. 480 ... 1828
  [*v.* Echinus]
Cordia, Stål, Hem. Afric. iv. 78 ... ... Hem. 1866
  [albilaterata, peragrans.]
Cordienia, A. Rouault, B. S. géol. France, v. 207.. Gast. 1848
  [biaritziana, iberica, palensis, pyrenaica, all *nom. nud.*]

Charles Davies Sherborn.

540 King's Road, London, S.W.

"The Anatomy of the Frog."

In your notice of the above work, in Nature of the 8th inst., you are pleased to express a favourable opinion of the wood engravings. As the heading of the article might lead your readers to imagine that these, in addition to the coloured plates, were all executed by Hofmann, of Bavaria, I think they, as well as yourself, will be pleased to know that all the *new* blocks, numbering upwards of one hundred, were engraved by

172 Strand, London. T. P. Collings.

COLOUR-VISION AND COLOUR-BLINDNESS.[1]

It is a matter of familiar knowledge that the sense of vision is called into activity by the formation, on the retina or internal nervous expansion of the eye, of an inverted optical image of external objects—an image precisely analogous to that of the photographic camera. The retina lines the interior of the eyeball over somewhat more than its posterior hemisphere. It is a very delicate transparent membrane, about one-fifth of a millimetre in thickness at its thickest part, near the entrance of the optic nerve, and it gradually diminishes to less than half that thickness at its periphery. It is resolvable by the microscope into ten layers, which are united together by a web of connective tissue, which also carries blood-vessels to minister to the maintenance of the structure. I need only refer to two of these layers: the anterior or fibre-layer, mainly composed of the fibres of the optic nerve, which spread out radially from their point of entrance in every direction, except where they curve around the central portion of the membrane; and the perceptive layer, which, as viewed from the interior of the eyeball, may be likened to an extremely fine mosaic, each individual piece of which is in communication with a nerve fibre, by which the impressions made upon it are conducted to the brain. The terminals of the perceptive layer are of two kinds, called respectively rods and cones; the former, as the name implies, being cylindrical in shape, and the latter conical. The bases of the cones are directed towards the interior of the eye, so as to receive the light; and it is probable that each cone may be regarded as a collecting apparatus, calculated to gather together the light which it receives, and to concentrate this light upon its deeper and more slender portion, or posterior limb, which is believed to be the portion of the whole structure which is really sensitive to luminous impressions. The distribution of the two elements differs greatly in different animals; and the differences point to corresponding differences in function. The cones are more sensitive than the rods, and minister to a higher acuteness of vision. In the human eye, there is a small central region in which the perceptive layer consists of cones only, a region which the fibres avoid by curving round it, and in which the other layers of the retina are much thinner than elsewhere, so as to leave a depression, and are stained of a lemon-yellow colour. In a zone immediately around this yellow spot each cone is surrounded by a single circle of rods; and, as we proceed outwards towards the periphery of the retina, the circle of rods around each cone becomes successively double, triple, quadruple, or even more numerous. The yellow spot receives the image of the object to which the eye is actually directed, while the images of surrounding objects fall upon zones which surround the yellow spot; and the result of this arrangement is that, generally speaking, the distinctness of vision diminishes in proportion to the distance of the image of the object from the retinal centre. The consequent effect has been well described by saying that what we see resembles a picture, the central part of which is exquisitely finished, while the parts around the centre are only roughly sketched in. We are conscious that these outer parts are there; but, if we desire to see them accurately, they must be made the objects of direct vision in their turn.

The indistinctness with which we see lateral objects is so completely neutralized by the quick mobility of the eyes, and by the manner in which they range almost unconsciously over the whole field of vision, that it seldom or never forces itself upon the attention. It may be conveniently displayed by means of an instrument called a perimeter, which enables the observer to look steadily at a central spot, while a second spot, or other object, is moved along an arc, in any meridian, from the circumference of the field of view towards the centre, or *vice versâ*. Slight differences will be found between individuals; but, speaking generally, a capital letter one-third of an inch high, which is legible by direct vision at a distance of sixteen feet, and is recognizable as a dark object at 40° or 50° from the fixing point, will not become legible, at a distance of one foot, until it arrives within about 10°.

The image formed upon the retina is rendered visible by two different conditions—that is to say, by differences in the amount of light which enters into the formation of its different parts, and by differences in the quality of this light, that is, in its colour. The former conditions are fulfilled by an engraving, the latter by a painting. It is with the latter conditions only, and with the power of perceiving them, that we are concerned this evening.

Before such an audience as that which I have the honour to address, it is unnecessary to say more about colour than that it depends upon the power, possessed by the objects which we describe as coloured, to absorb and retain certain portions of white or other mixed light, and to reflect or transmit other portions. The resulting effect of colour is the impression produced upon the eye or upon the brain by the waves of light which are left, after the process of selective absorption has been accomplished. Some substances absorb two of the three fundamental colours of the solar spectrum, others absorb one only, others absorb portions of one or more. Whatever remains is transmitted through the media of the eye; and, in the great majority of the human race, suffices to excite the retina to a characteristic kind of activity. Few things are more curious than the multitude of different colour sensations which may be produced by the varying combinations of the three simple elements, red, green, and violet; but this is a part of the subject into which it would be impossible for me now to enter, and with which most of those who hear me must already be perfectly familiar.

Apart from the effect of colour as one of the chief sources of beauty in the world, it is manifest that the power of distinguishing it adds greatly to the acuteness of vision. Objects which differ from their surroundings by differences of colour are far more conspicuous than those which differ only by differences of light and shade. Flowers are much indebted to their brilliant colouring for the visits of the insects by which they are fertilized; and creatures which are the prey of others find their best protection in a resemblance to the colours of their environment. It is probably a universal truth that the organs of colour-perception are more highly specialized, and that the sense of colour is more developed, in all animals, in precise proportion to the general acuteness of vision of each.

From a variety of considerations, into which time will not allow me to enter, it has been concluded that the sense of colour is an endowment of the retinal cones, and that the rods are sensitive only to differences in the quantity of the incident light, without regard to its quality. Nocturnal mammals, such as mice, bats, and hedgehogs, have no cones; and cones are less developed in nocturnal birds than in diurnal ones. Certain limitations of the human colour sense may almost be inferred from the anatomy of the retina. It is found, as that anatomy would lead us to suppose, that complete colour sense exists only in the retinal centre, or in and immediately around the yellow spot region, and that it diminishes as we pass away from this centre towards the periphery. The precise facts are more difficult to ascertain than might be supposed; for, although it is easy to bring coloured objects from the circumference to the centre of the field of vision on the perimeter, it is by no means easy to be quite sure of the point at which the true colour of the advancing

[1] Lecture delivered at the Royal Institution, on Friday, May 9, 1890, by Mr. R. Brudenell Carter.

object can first be said to be distinctly seen. Much depends, moreover, on the size of this advancing object; because, the larger it is, the sooner will its image fall upon some of the more sparsely distributed cones of the peripheral portion of the retina. Testing the matter upon myself with coloured cards of the size of a man's visiting card, I find that I am conscious of red or blue at about 40° from the fixing point, but not of green until it comes within about 30°; while, if I take three spots, respectively of bright red, bright green, and bright blue, each half a centimetre in diameter, and separated from its neighbour on either side by an interval of half a centimetre, spots which would be visible as distinct and separate objects at eight metres, I cannot fairly and distinctly see all three colours until they come within 10° of the centre. Beyond 40°, albeit with slight differences between individuals, and on different meridians for the same individual, colours are only seen by the degree of their luminosity—that is, they appear as light spots if upon a dark ground, and as dark spots if upon a light ground. Speaking generally, therefore, it may be said that human vision is only trichromatic, or complete for the three fundamental colours of the solar spectrum, over a small central area, which certainly does not cover more than 30° of the field; that it is bi-chromatic, or limited to red and violet, over an annulus outside this central area; and that it is limited to light and shade from thence to the outermost limits of the field.

The nature and limitations of the colour-sense in man long ago suggested to Thomas Young that the retina might contain three sets of fibres, each set capable of responding to only one of the fundamental colours; or, in other words, that there are special nerve fibres for red, special nerve fibres for green, and special nerve fibres for violet. It has also been assumed that the differences between these fibres might essentially consist in the ability of each set to respond only to light-vibrations of a certain wave-length, much as a tuned string will only respond to a note with which it is in unison. In the human subject, so far as has yet been ascertained, no optical differences between the cones are discoverable; but the analogy of the ear, and the facts which have been supplied by comparative anatomy, combine to render Young's hypothesis exceedingly probable, and it is generally accepted, at least provisionally, as the only one which furnishes an explanation of the facts. It implies that elements of all three varieties are present in the central portion of the retina; that elements sensitive to green are absent from an annulus around the centre; and that the peripheral portions are destitute of any elements by which colour-sense can be called into activity.

According to the observation already made, that the highest degree of acuteness of vision is necessarily attended by a corresponding acuteness of colour-sense, we should naturally expect to find such a highly-developed colour-sense in birds, many of which appear, as regards visual power, to surpass all other creatures. I need not dwell upon the often-described acuteness of vision of vultures, or upon the vision of fishing birds; but may pass on to remark that the acuteness of their vision appears not only to be unquestionable, but also to be much more widely diffused over the retina than is the case with man. If we watch domestic poultry, or pigeons, feeding, we shall frequently see a bird, when busily picking up food immediately in front of its beak, suddenly make a lateral dart to some grain lying sideways to its line of sight, which would have been practically invisible to a human eye looking in the same direction as that of the fowl. When we examine the retina the explanation both of the acuteness of vision and of its distribution becomes at once apparent. In birds, in some reptiles, and in fishes, not only are cones distributed over the retina much more abundantly and more evenly than in man, but the cones are provided with coloured globules, droplets of coloured oil, at their apices, through which the light entering them must pass before it can excite sensation, and which are practically impervious to any colour but their own. Each globule is so placed as to intervene between what is regarded as the collecting portion of the cone and what is regarded as its perceptive portion in such a way that the latter can only receive colour which is capable of passing through the globule. The retinæ of many birds, especially of the finch, the pigeon, and the domestic fowl, have been carefully examined by Dr. Waelchli, who finds that near the centre green is the predominant colour of the cones, while among the green cones red and orange ones are somewhat sparingly interspersed, and are nearly always arranged alternately, a red cone between two orange ones, and *vice versâ*. In a surrounding portion, called by Dr. Waelchli the red zone, the red and orange cones are arranged in chains, and are larger and more numerous than near the yellow spot; the green ones are of smaller size, and fill up the interspaces. Near the periphery the cones are scattered, the three colours about equally numerous and of equal size, while a few colourless cones are also seen. Dr. Waelchli examined the optical properties of the coloured cones by means of the micro-spectroscope, and found, as the colours would lead us to suppose, that they transmitted only the corresponding portions of the spectrum; and it would almost seem, excepting for the few colourless cones at the peripheral part of the retina, that the birds examined must have been unable to see blue, the whole of which would be absorbed by their colour globules. It would be necessary to be thoroughly acquainted with their food in order to understand any advantage which the birds in question may derive from the predominance of green, red, and orange globules over others; but it is impossible to consider the structure thus described without coming to the conclusion that the birds in which it exists must have a very acute sense of the colours corresponding to the globules with which they are so abundantly provided, and that this colour-sense, instead of being localized in the centre, as in the human eye, must be diffused over a very large portion of the retina. Dr. Waelchli points out that the coloration of the yellow spot in man must, to a certain extent, exclude blue from the central and most sensitive portion of his retina.

It is hardly necessary to mention how completely the high differentiation of the cones in the creatures referred to tends to support the hypothesis of Young, that a similar differentiation, although not equally manifest, exists also in man. If this be so, we must conclude that the region of the yellow spot contains cones, some of which are capable of being called into activity by red, others by green, and others by violet; that a surrounding annulus contains no cones sensitive to green, but such as are sensitive to red or to violet only; and that, beyond and around this latter region, such cones as may exist are not sensitive to any colour, but, like the rods, only to differences in the amount of light. When cones of only one kind are called into activity, the sensation produced is named red, green, or violet; and, when all three varieties are stimulated in about an equal degree, the sensation produced is called white. In the same way, the innumerable intermediate colour-sensations of which the normal eye is susceptible, must be ascribed to stimulation of the three varieties of cones in unequal degrees.

The conditions of colour-sense which, in the human race, or at least in civilized man, exist normally in outer zones of the retina, are found, in a few individuals, to exist also in the centre. There are persons in whom the region of the yellow spot is absolutely insensitive to colour, and recognizes only differences in the amount or quantity of light. To such persons, the term "colour-blind" ought perhaps in strictness to be limited; but the individuals in question are so rare that they are hardly

entitled to a monopoly of an appellation which is conveniently applied also to others. The totally colour-blind would see a coloured picture as if it were an engraving, or a drawing in black and white, and would perceive differences between its parts only in the degree in which they differed in brightness.

A more common condition is the existence, in the centre of the retina, of a kind of vision like that which normally exists in the zone next surrounding it—that is, a blindness to green. Persons who are blind to green appear to see violet and yellow much as these are seen by the normal-sighted; and they can see red, but they cannot distinguish it from green. Others, and this form is more common than the preceding, are blind to red; and a very small number of persons are blind to violet. Such blindness to one of the fundamental colours may be either complete or incomplete—that is to say, the power of the colour in question to excite its proper sensation may be either absent or feeble. In some cases, the defect is so moderate in degree as to be adequately described by the phrase "defective colour-sense."

The experiments of Helmholtz upon colour led him to supplement the original hypothesis of Young by the supposition that the special nerve elements excited by any one colour are also excited in some degree by each of the other two, but that they respond by the sensation appropriate to themselves, and not by that appropriate to the colour by which they are thus feebly excited. This, which is often called the Young-Helmholtz hypothesis, assumes that the pure red of the spectrum, while it mainly stimulates the fibres sensitive to red, stimulates in a less degree those which are sensitive to green, and in a still less degree those which are sensitive to violet, the resulting sensation being red. Pure green stimulates strongly the green-perceptive fibres, and stimulates slightly both the red-perceptive and the violet-perceptive—resulting sensation, green. Pure violet stimulates strongly the violet-perceptive fibres, less strongly the green-perceptive, least strongly the red-perceptive—resulting sensation, violet. When all three sets of fibres are stimulated at once, the resulting sensation is white; and when a normal eye is directed to the spectrum, the region of greatest luminosity is in the middle of the yellow; because, while here both the green-perceptive and the red-perceptive fibres are stimulated in a high degree, the violet-perceptive are also stimulated in some degree.

According to this view of the case, the person who is red-blind, or in whom the red-perceptive fibres are wanting or paralyzed, has only two fundamental colours in the spectrum instead of three. Spectral red, nevertheless, is not invisible to him, because it feebly excites his green-perceptive fibres, and hence appears as a saturated green of feeble luminosity; saturated, because it scarcely at all excites the violet-perceptive fibres. The brightest part of the spectrum, instead of being in the yellow, is in the blue-green, because here both sets of sensitive fibres are stimulated. In the case of the green-blind, in whom the fibres perceptive of green are supposed to be wanting or paralyzed, the only stimulation produced by spectral green is that of the red-perceptive and of the violet-perceptive fibres; and where these are equally stimulated, we obtain the white of the green-blind, which, to ordinary eyes, is a sort of rose-colour, a mixture of red and violet. In like manner, the white of the red-blind is a mixture of green and violet; and, if we consider the facts, we shall see that spectral red, which somewhat feebly stimulates the green-perceptive fibres of the normal eye, and spectral green, which somewhat feebly stimulates the red-perceptive fibres of the normal, and also of the green-blind eye, must appear to the green-blind to be one and the same colour, differing only in luminosity, and that in an opposite sense to the perception of the red-blind. In other words, red and green are undistinguishable from each other, as colours, alike to the red-blind and to the green-blind; but to the former the red, and to the latter the green, appears, as compared with the other, to be of feeble luminosity. In either case, the two are only lighter and darker shades of the same colour. The conditions of violet-blindness are analogous, but the defect itself is very rare; and, as it is of small industrial importance, it has attracted but a small degree of attention.

Very extensive investigations, conducted during the last few years both in Europe and in America, have shown that those which may be called the common forms of colour-blindness, the blindness to red and to green, exist in about four per cent. of the male population, and in perhaps one per thousand of females. Among the rest, there are slight differences of colour-sense, partly due to differences of habit and training, but of little or no practical importance. One such difference, to which Lord Rayleigh was the first to direct attention, has reference to yellow. The pure yellow of the spectrum may, as is generally known, be precisely matched by a mixture of spectral red with spectral green; but the proportions in which the mixture should be made differ within certain limits for different people. The difference must, I think, depend upon differences in the pigmentation of the yellow spot, rather than upon any defect in the nervous apparatus of the colour-sense. There is a very ingenious instrument, invented by Mr. Lovibond, and called by him the "tintometer," which allows the colour of any object to be accurately matched by combinations of coloured glass, and to be expressed in terms of the combination. In using this instrument, we not only find slight differences in the combinations required by different people, but also in the combinations required by the two eyes of the same person. Here, again, I think the differences must be due either to differences in the pigmentation of the yellow spot, or possibly also to differences in the colour of the internal lenses of the several eyes, the lens, as is well known, being usually somewhat yellow after middle age. The differences are plainly manifest in comparing persons all of whom possess tri-chromatic vision, and are not sufficient in degree to be of any practical importance.

Taking the ordinary case of a red-blind or of a green-blind person, it is interesting to speculate upon the appearance which the world must present to them. Being insensible to one of the fundamental colours of the spectrum, they must lose, roughly speaking, one-third of the luminosity of Nature; unless, as is possible, the deficiency is made good to them by increased acuteness of perception to the colours which they see. Whether they see white as we see it, or as we see the mixtures of red and violet, or of green and violet, which they make to match with it, we can only conjecture, on account of the inadequacy of language to convey any accurate idea of sensation. We have all heard of the blind man who concluded, from the attempts made to describe scarlet to him, that it was like the sound of a trumpet. If we take a heap of coloured wools, and look at them first through a glass of peacock-blue, by which the red rays are filtered out, and next through a purple glass, by which a large proportion of the green will be filtered out, we may presume that, under the first condition, the wools will appear much as they would do to the red-blind; and, under the second, much as they would do to the green-blind. It will be observed that the appearances differ in the two conditions, but that, in both, red and green are practically undistinguishable from each other, and appear as the same colour, but of different luminosity.

Prior to reflection, and still more, prior to experience, we should be apt to conjecture that the existence of colour-blindness in any individual could not remain concealed, either from himself or from those around him; but such a conjecture would be directly at variance with the truth. Just as it was reserved for Mariotte, in the reign of Charles II., to discover that there is, in the field of vision of every eye, a lacuna or blind spot, correspond-

ing with the entrance of the optic nerve, so it was reserved for a still later generation to discover the existence of so common a defect as colour-blindness. The first recorded case was described to Dr. Priestley by Mr. Huddart, in 1777, and was that of a man named Harris, a shoemaker at Maryport in Cumberland, who had also a colour-blind brother, a mariner. Soon afterwards, the case of Dalton, the chemist, was fully described, and led to the discovery of other examples of a similar kind. The condition was still, however, looked upon as a very exceptional one; insomuch that the name of "Daltonism" was proposed for it, and is still generally used in France as a synonym for colour-blindness. Such use is objectionable, not only because it is undesirable thus to perpetuate the memory of the physical infirmity of an eminent philosopher, but also because Dalton was a red-blind, so that the name could only be correctly applied to his particular form of defect.

Colour-blindness often escapes detection on account of the use of colour-names by the colour-blind in the same manner as that in which they hear them used by other people. Children learn from the talk of those around them, that it is proper to describe grass as green, and bricks or cherries as red; and they follow this usage, although the difference may appear to them so slight that their interpretation of either colour-name may be simply as a lighter or darker shade of the other. When they make mistakes, they are laughed at, and thought careless, or to be merely using colour-names incorrectly; and a common result is that they rather avoid such names, and shrink from committing themselves to statements about colour. Dr. Joy Jefferies gives an interesting description of the almost unconscious devices practised by the colour-blind in this way. He says:—

"The colour-blind, who are quick-witted enough to discover early that something is wrong with their vision by the smiles of their listeners when they mention this or that object by colour, are equally quick-witted in avoiding so doing. They have found that there are names of certain attributes they cannot comprehend, and hence must let alone. They learn, also, what we forget, that so many objects of every-day life always have the same colour, as red tiles or bricks, and the colour names of these they use with freedom; whilst they often, even unconsciously, are cautious not to name the colour of a new object till they have heard it applied, after which it is a mere matter of memory stimulated by a consciousness of defect. I have often recalled to the colour-blind their own acts and words, and surprised them by an exposure of the mental jugglery they employed to escape detection, and of which they were almost unaware, so much had it become matter of habit. Another important point is, that as violet-blindness is very rare, the vast majority of defective eyes are red or green blind. These persons see violet and yellow as the normal-eyed, and they naturally apply these colour-names correctly. When, therefore, they fail in red or green, a casual observer attributes it to simple carelessness—hence a very ready avoidance of detection. It does not seem possible that anyone who sees so much correctly, and whose ideas of colour so correspond with our own, cannot be equally correct throughout, if they will but take the pains to notice and learn."

When the colour-blind are placed in positions which compel them to select colours for themselves and others, or when, as sometimes happens, they are not sensitive with regard to their defect, but rather find amusement in the astonishment which it produces among the colour-seeing, the results which occasionally follow are apt to be curious. They have often been rendered still more curious, by having been the unconscious work of members of the Society of Friends. Colour-blindness is a structural peculiarity, constituting what may be called a variety of the human race; and, like other varieties, it is liable to be handed down to posterity. Hence, if the variety occurs in a person belonging to a community which is small by comparison with the nation, and among whose members there is frequent inter-marriage, it has an increased probability of being reproduced; and thus, while many of the best known of the early examples of colour-blindness, including that of Dalton himself, were furnished by the Society of Friends, the examinations of large numbers of scholars and others, conducted during the last few years, have shown that, in this country, colour-blindness is more common among Jews than among the general population. The Jews have no peculiarities of costume; but the spectacle, which has more than once been witnessed, of a venerable Quaker who had clothed himself in bright green or in vivid scarlet, could scarcely fail to excite the derision of the unreflecting. Time does not allow me to relate the many errors of the colour-blind which have been recorded; but there is an instance of a clerk in a Government office, whose duty it was to tick certain entries, in relation to their subject-matter, with ink of one or of another colour, and whose accuracy was dependent upon the order in which his ink-bottles were ranged in front of him. This order having been accidentally disturbed, great confusion was produced by his mistakes, and it was a long time before these were satisfactorily accounted for. An official of the Prussian Post-Office, again, who was accustomed to sell stamps of different values and colours, was frequently wrong in his cash, his errors being as often against himself as in his favour, so as to exclude any suspicion of dishonesty. His seeming carelessness was at last explained by the discovery of his colour-blindness, and he was relieved of a duty which it was impossible for him to discharge without falling into error.

The colour-mistakes of former years were, however, of little moment when compared with those now liable to be committed by engine-drivers and mariners. The avoidance of collisions at sea and on railways depends largely on the power promptly to recognize the colours of signals; and the colours most available for signalling purposes are red and green, or precisely those between which the sufferers from the two most common forms of colour-blindness are unable with any certainty to discriminate. About thirteen years ago there was a serious railway accident in Sweden, and, in the investigation subsequent to this accident, there were some remarkable discrepancies in the evidence given with regard to the colour of the signals which had been displayed. Prof. Holmgren, of the University of Upsala, had his attention called to this discrepancy, and he found, on further examination, that the witness whose assertions about the signals differed from those of other people was actually colour-blind. From this incident arose Prof. Holmgren's great interest in the subject, and he did not rest until he had obtained the enactment of a law under which no one can be taken into the employment of a Swedish railway until his colour-vision has been tested, and has been found to be sufficient for the duties he will be called upon to perform. The example thus set by Sweden has been followed, more or less, by other countries, and especially, thanks to the untiring labours of Dr. Joy Jeffries, of Boston, by several of the United States; while at the same time much evidence has been collected to show the connection between railway and marine accidents and the defect.

It has been found, by very extensive and carefully conducted examinations of large bodies of men, soldiers, policemen, the workers in great industrial establishments, and so forth, as well as of children in many schools, that colour-blindness exists in a noticeable degree, as I have already said, in about four per cent. of the male industrial population in civilized countries, and in about one per thousand of females. Among the males of the more highly educated classes, taking Eton boys as an example, the colour-blind are only between two and three per cent., and perhaps nearer to two than to three.

Whether a similar difference exists between females of different classes, we have no statistics to establish. The condition of colour-blindness is absolutely incurable, absolutely incapable of modification by training or exercise, in the case of the individual; although the comparative immunity of the female sex justifies the suggestion that it may possibly be due to training throughout successive generations, on account of the more habitual occupation of the female eyes about colour in relation to costume. However this may be, in the individual, as I have said, the defect is unalterable; and if the difference between red and green is uncertain at eight years of age, it will be equally uncertain at eighty. Hence the existence of colour-blindness, among those who have to control the movements of ships or of railway trains, constitutes a real danger to the public; and it is highly important that the colour-blind, in their own interests as well as in those of others, should be excluded from employments the duties of which they are unfit to discharge.

The attempts hitherto made in this country to exclude the colour-blind from railway and marine employment have not been by any means successful. As far as the merchant navy is concerned, so-called examinations have been conducted by the Board of Trade, with results which can only be described as ludicrous. Candidates have been "plucked" in colour at one examination, and permitted to pass at a subsequent one; as if correct colour-vision were something which could be acquired. Such candidates were either improperly rejected on the first occasion, or improperly accepted on the second. On English railways there has been no uniformity in the methods of testing; except, in so far as I am acquainted with them, that they have been almost uniformly misleading, calculated to lead to the imputation of colour-blindness where it did not exist, and to leave it undiscovered where it did. In these circumstances it is not surprising that great discontent should have arisen among railway men in relation to the subject; and this discontent has led, indirectly, to the appointment of a Committee by the Royal Society, with the sanction of the Board of Trade, for the purpose of investigating the whole question as completely as may be possible.

It is perhaps worth while, before proceeding to describe the manner in which the colour-sense of large bodies of men should be tested for industrial purposes, to say something as to the amount of danger which colour-blindness produces. A locomotive, as we all know, is under the charge of two men—the driver and the fireman. In a staff of one thousand of each, allotted to one thousand locomotives, we should expect, in the absence of any efficient method of examination, to find forty colour-blind drivers, and forty colour-blind firemen. The chances would be one in twenty-five that either the driver or the fireman on any particular engine would be colour-blind; they would be one in 625 that both would be colour-blind. These figures appear to show a greater risk of accident than we find realized in actual working, and it is manifest that there are compensations to be taken into account. In the first place, the term "colour-blind" is itself in some degree misleading; for it must be remembered that the signals to which the colour-blind person is said to be "blind" are not invisible to him. To the red-blind, the red light is a less luminous green; to the green-blind, the green light is a less luminous red. The danger arises because the apparent differences are not sufficiently characteristic to lead to certain and prompt identification in all states of illumination and of atmosphere. It must be admitted, therefore, that a colour-blind driver may be at work for a long time without mistakes; and it is probable, knowing as he must that the differences between different signal lights appear to him to be only trivial, that he will exercise extreme caution. Then it must be remembered that lights never appear to an engine-driver in unexpected places. Before being intrusted with a train, he is taken over the line, and is shown the precise position of every light. If a light did not appear where it was due, he would naturally ask his fireman to aid in the look-out. It must be also remembered that to overrun a danger signal does not of necessity imply a collision. A driver may overrun the signal, and after doing so may see a train or other obstruction on the line, and may stop in time to avoid an accident. In such a case, he would probably be reported and fined for overrunning the signal; and, if the same thing occurred again, he would be dismissed for his assumed carelessness, probably with no suspicion of his defect. Colour-blind firemen are unquestionably thus driven out of the service by the complaints of their drivers; and none but railway officials know how many cases of overrunning signals, followed by disputes as to what the signals actually were, occur in the course of a year's work. I have never heard of an instance in this country, in which, after a railway accident, the colour-vision of the driver concerned, or of his fireman, has been tested by an expert, on the part either of the Board of Trade or of the Company; but a fireman in the United States has recently recovered heavy damages from the Company for the loss of one of his legs in a collision which was proved to have been occasioned by the colour-blindness of the driver. Looking at the whole question, I feel that the danger on railways is a real one, but that it is minimized by the several considerations to which I have referred, and that it is much smaller than the frequency of the defect might lead us to think likely.

At sea, the danger is much more formidable. The lights appear at all sorts of times and places, and there may be only one responsible person on the look-out. Mr. Bickerton, of Liverpool, has lately published accounts of three cases in which the colour-blindness of officers of the mercantile marine, all of whom had passed the Board of Trade examination, was accidentally discovered by the captains being on deck when the officers in question gave wrong orders consequent upon mistaking the light shown by an approaching vessel. The loss of the *Ville du Havre* was almost certainly due to colour-blindness; and a very fatal collision in American waters, some years ago, between the *Isaac Bell* and the *Lumberman*, was traced, long after the event, to the colour-blindness of a pilot, who had been unjustly accused of being drunk at the time of the occurrence. In how many instances colour-blindness has been the unsuspected cause of wrecks and other calamities at sea, it is impossible to do more than conjecture.

It is necessary, then, alike in the public interest and in the interest of the colour-blind, who have doubtless often suffered in the misfortunes which their defects have produced, to detect them in time to prevent them from entering into the marine and railway services; and the next question is, how this detection should be accomplished. We have to distinguish the colour-blind from the colour-sighted; but we must be careful not to confound colour-blindness with the much more common condition of colour-ignorance.

It would surprise many people, more especially many ladies, to discover the extent to which sheer ignorance of colour prevails among boys and men of the labouring classes. Many, who can see colours perfectly, and who would never be in the least danger of mistaking a railway signal, are quite unable to name colours or to describe them; and they are sometimes unable to perceive, for want of education of a faculty which they notwithstanding possess, anything like fine shades of difference. Mr. Gladstone once published a paper on the scanty and uncertain colour-nomenclature of the Homeric poems; and he might have found very similar examples among his own contemporaries and in his own country. I have lately seen a pattern card of coloured silks, issued by a Lyons manufacturer, which contains samples of two

thousand different colours, each with its more or less appropriate name. There is here a larger colour-vocabulary than the entire vocabulary, for the expression of all his knowledge and of all his ideas, which is possessed by an average engine-driver or fireman; and, just as most of us would be ignorant of the names of the immense majority of the colours displayed on that card, so hundreds of men and boys among the labouring classes, especially in large towns, where the opportunities of education by the colours of flowers and insects are very limited, are ignorant of the names of colours which persons of ordinary cultivation mention constantly in their daily talk, and expect their children to pick up and to understand unconsciously. It is among people thus ignorant that the officials of the Board of Trade, and of railways, have been most successful in finding their supposed colour-blind persons; and these persons, who would never have been pronounced colour-blind by an expert, have been able, as soon as they have paid a little attention to the observation and naming of colour, to pass an official examination triumphantly. The sense of colour presents many analogies to that of hearing. Some people can hear a higher or a lower note than others, the difference depending upon structure, and being incapable of alteration. No one who cannot hear a note of a certain pitch can ever be trained to do so; but, within the original auditory limits of each individual, the sense of hearing may be greatly improved by cultivation. In like manner, a person who is blind to red or green must remain so; but one whose colour-sense is merely undeveloped by want of cultivation may have its acuteness for fine differences very considerably increased.

In order to test colour-vision for railway and marine purposes, the first suggestion which would occur to many people would be to employ as objects the flags and signal lanterns which are used in actual working. I have heard apparently sensible people use, with reference to such a procedure, the phrase upon which Faraday was wont to pour ridicule, and to say that the fitness of the suggested method "stands to reason." To be effectual, such a test must be applied in different states of atmosphere, with coloured glasses of various tints, with various degrees of illumination, and with the objects at various distances; so that much time would be required in order to exhaust all the conditions under which railway signals may present themselves. This being done, the examinee must be either right or wrong each time. He has always an even chance of being right; and it would be an insoluble problem to discover how many correct answers were due to accident, or how many incorrect ones might be attributed to nervousness or to confusion of names.

We must remember that what is required is to detect a colour-blind person against his will; and to ascertain, not whether he describes a given signal rightly or wrongly on a particular occasion, but whether he can safely be trusted to distinguish correctly between signals on all occasions. We want, in short, to ascertain the state of his colour-vision generally; and hence to infer his fitness or unfitness to discharge the duties of a particular occupation.

For the accomplishment of this object, we do not in the least want to know what the examinee calls colours, but only how he sees them, what colours appear to him to be alike and what appear to be unlike; and the only way of attaining this knowledge with certainty is to cause him to make matches between coloured objects, to put those together which appear to him to be essentially the same, and to separate those which appear to him to be essentially different. This principle of testing was first laid down by Seebeck, who required from examinees a complete arrangement of a large number of coloured objects; but it has been greatly simplified and improved by Prof. Holmgren, who pointed out that such a complete arrangement was superfluous, and that the only thing required was to cause the examinee to make matches to certain test colours, and, for this purpose, to select from a heap which contained not only such matches but also the colours which the colour-blind were liable to confuse with them.

After many trials, Holmgren finally selected skeins of Berlin wool as the material best suited for this purpose; and his set of wools comprises about 150 skeins. The advantages of his method over every other are that the wool is very cheap, very portable, and always to be obtained in every conceivable colour and shade. The skeins are not lustrous, so that light reflected from the surfaces does not interfere with the accuracy of the observation; and they are very easily picked up and manipulated, much more easily than coloured paper or coloured glass. The person to be tested is placed before a table in good daylight, the table is covered by a white cloth, and the skeins are thrown upon it in a loosely arranged heap. The examiner then selects a skein of pale green, much diluted with white, and throws it down by itself to the left of the heap. The examinee is directed to look at this pattern skein and at the heap, and to pick out from the latter, and to place beside the pattern, as many skeins as he can find which are of the same colour. He is not to be particular about lighter or darker shades, and is not to compare narrowly, or to rummage much amongst the heap, but to select by his eyes, and to use his hands chiefly to change the position of the selected material.

In such circumstances, a person with normal colour-sight will select the greens rapidly and without hesitation, will select nothing else, and will select with a certain readiness and confidence easily recognized by an experienced examiner, and which may even be carried to the extent of neglecting the minute accuracy which a person who distrusts his own colour-sight will frequently endeavour to display. Some normal-sighted people will complete their selection by taking greens which incline to yellow, and greens which incline to blue, while others will reject both; but this is a difference depending sometimes upon imperfect colour education, sometimes upon the interpretation placed upon the directions of the examiner, but the person who so selects sees the green element in the yellow-greens and in the blue-greens, and is not colour-blind. The completely colour-blind, whether to red or to green, will proceed with almost as much speed and confidence as the colour-sighted; and will rapidly pick out a number of drabs, fawns, stone-colours, pinks, or yellows. Between the foregoing classes, we meet with a few people who declare the imperfection of their colour-sense by the extreme care with which they select, by their slowness, by their hesitation, and by their desire to compare this or that skein with the pattern more narrowly than the conditions of the trial permit. They may or may not ultimately add one or more of the confusion colours to the green, but they have a manifest tendency to do so, and a general uncertainty in their choice. One of the great advantages of Holmgren's method over every other is the way in which the examiner is able to judge, not only by the final choice of matches, but also by the manner in which the choice is made, by the action of the hands, and by the gestures and general deportment of the examinee.

When confusion colours have been selected, or when an unnatural slowness and hesitation have been shown in selecting, the examinee must be regarded as either completely or incompletely colour-blind. In order to determine which, and also to which colour he is defective, he is subjected to the second test. For this, the wool is mixed again, and the pattern this time is a skein of light purple—that is, of a mixture of red and violet, much diluted with white. To match this, the colour-blind always selects deeper colours. If he puts only deeper purples, he is incompletely colour-blind. If he takes blue or violet, either with or without purple, he is completely red-blind. If he

takes green or gray, or one alone, with or without purple, he is completely green-blind. If he takes red or orange, with or without purple, he is violet-blind. If there be any doubt, the examinee may be subjected to a third test, which is not necessary for the satisfaction of an expert, but which sometimes strengthens the proof in the eyes of a bystander. The pattern for this third test is a skein of bright red, to be used in the same way as the green and the purple. The red-blind selects for this dark greens and browns, which are much darker than the pattern; while the green-blind selects greens and browns which are lighter than the pattern.

The method of examination thus described is, I believe, absolutely trustworthy. It requires no apparatus beyond the bundle of skeins of wool, no arrangements beyond a room with a good window, and a table with a white cloth. In examining large numbers of men, they may be admitted into the room fifty or so at a time, may all receive their instructions together, and may then make their selections one by one, all not yet examined watching the actions of those who come up in their turn, and thus learning how to proceed. The time required for large numbers averages about a minute a person. I have heard and read of instances of colour-blind people who had passed the wool test satisfactorily, and had afterwards been detected by other methods, but I confess that I do not believe in them. I do not believe that in such cases the wool test was applied properly, or in accordance with Holmgren's very precise instructions; and I know that it is often applied in a way which can lead to nothing but erroneous results. Railway foremen, for example, receive out of store a small collection of coloured wools selected on no principle, and they use it by pulling out a single thread, and by asking the examinee, "What colour do you call that?" Men of greater scientific pretensions than railway foremen have not always selected their pattern colours accurately, and have allowed those whom they examined, and passed, to make narrow comparisons between the skeins in all sorts of lights, in a way which should of itself have afforded sufficient evidence of defect.

Although, however, the expert may be fully satisfied by the wool test that the examinee is not capable of distinguishing with certainty between red and green flags or lights in all the circumstances in which they can be displayed, it may still remain for him to satisfy the employer who is not an expert, the railway manager, or the ship-owner, and to convince him that the colour-blind person is unfit for certain kinds of employment. It may be equally necessary to convince other workmen that the examinee has been fairly and rightly dealt with. Both these objects may be easily attained, by the use of slight modifications of the lights which are employed. Lanterns for this special purpose were contrived, some years ago, by Holmgren himself, and by the late Prof. Donders of Utrecht, and what are substantially their contrivances have been brought forward within the last few months as novelties, by gentleman in this country who have re-invented them. The principle of all is the same—namely, that light of varying intensity may be displayed through apertures of varying magnitude, and through coloured glass of varying tints, so as to imitate the appearances of signal lamps at different distances, and under different conditions of illumination, of weather, and of atmosphere. To the colour-blind, the difference between a red light and a green one is not a difference of colour, but of luminosity; the colour to which he is blind appearing the less luminous of the two. He may therefore be correct in his guess as to which of the two is exhibited on any given occasion, and he is by no means certain to mistake one for the other when they are exhibited in immediate succession. His liability to error is chiefly conspicuous when he sees one light only, and when the conditions which govern its luminosity depart in any degree from those to which he is most accustomed. With the lanterns of which I have spoken, it is always possible to deceive a colour-blind person by altering the luminosity of a light without altering its colour. This may be done by diminishing the light behind the glass, by increasing the thickness of the red or green glass, or by placing a piece of neutral tint, more or less dark, in front of either. The most incredulous employer may be convinced, by expedients of this kind, that the colour-blind are not to be relied upon for the safe control of ships or of locomotives. With regard to the whole question, there are many points of great interest, both physical and physiological, which are still more or less uncertain; but the practical elements have, I think, been well-nigh exhausted, and the means of securing safety are fully in the hands of those who choose to master and to employ them. The lanterns, in their various forms, are useful for the purpose of thoroughly exposing the colour-blind, and for bringing home the character of their incapacity to unskilled spectators; but they are both cumbrous and superfluous for the detection of the defect, which may be accomplished with far greater ease, and with equal certainty, by the wool test alone.

I have already mentioned that the examinations which have been conducted in the United States, thanks to the indefatigable labours of Dr. Joy Jeffries, have led to the discovery of an enormous and previously quite unsuspected amount of colour-ignorance, the condition which is frequently mistaken for colour-blindness by the methods of examination which are in favour with railway companies and with the Board of Trade; and this colour-ignorance has been justly regarded as a blot on the American system of national education. It has therefore, in some of the States, led to the adoption of systematic colour-teaching in the schools; and, for this purpose, Dr. Joy Jeffries has introduced a wall-chart and coloured cards. The children are taught, in the first instance, to match the colours in the chart with those of the cards distributed to them; and, when they are tolerably expert at matching, they are further taught the names of the colours. It must, nevertheless, always be remembered that a knowledge of names does not necessarily imply a knowledge of the things designated; and that colour-vision stands in no definite relation to colour-nomenclature. Even this system of teaching may leave a colour-blind pupil undetected.

COMPOUND LOCOMOTIVES.

THE present position of locomotive engineering in this country is of a very interesting nature; owing to the gradual increase of weight of trains hauled and the higher speeds now in use, it has been necessary to increase the power of the locomotive by leaps and bounds to cope with these demands. This naturally has not been done without great scheming on the part of the designers, for, with the standard gauge of railway of 4 feet 8½ inches, the engines are tied down to certain dimensions between the frame plates; in total length, to a certain extent, by the turn-tables in use; and in height of boiler for reasons of stability. These questions of design are interesting because they are intimately connected with the economical working of the engines, especially in the consumption of fuel, a question which of late years has taken a prominent position in the economical management of locomotives. For several years the highly economical results obtained at sea with the use of high pressures coupled with the compound or triple expansion engine have caused engineers to look in that direction for further improvements, with the result that two different types of compound locomotives were designed, and are considerably past the experimental stage. These engines are now working successfully on two of the English railways, and are being adopted on many foreign ones.

The type of compound locomotives first used in any number is that patented by Mr. F. W. Webb, the able Mechanical Superintendent of the London and North-Western Railway. This design is interesting because it is to a certain extent an example of really original work in locomotive practice. Mr. Webb had several very good ideas to work on in this design, all of very great importance from an engineer's point of view. These ideas were as follows:—The engine must not have a double-throw crank-axle, this being certainly the weak point of all inside-cylinder engines; the coupling rods between driving and trailing wheels must be done away with, since these also sometimes break, and may cause serious accidents. The doing away with the coupling rods enables a longer fire-box to be used, because the coupled wheels should be near together to obtain a minimum length of coupling rod, for reasons of safety. Thus, to design an engine to be more powerful than an ordinary four-coupled express of the North-Western heavy pattern, and having at the same time fewer parts liable to accident, as stated above, requires some clever scheming and much thought. The engine ultimately adopted by Mr. Webb for use on the London and North-Western Railway has three cylinders, viz. two high-pressure cylinders and one low-pressure cylinder. The high-pressure cylinders are placed one on each side of the engine, and are connected to the trailing or hind pair of wheels. The low-pressure cylinder is placed between the frames at the front end of the engine, and is connected with the front pair of driving wheels by a single-throw crank-axle. It will be noticed that in this arrangement each pair of wheels are driving wheels, that the side or coupling rods are done away with, and that the ordinary double-throw crank-axle has given place to a single-throw crank-axle, which may be made of ample dimensions and practically unbreakable. The course of the steam through the cylinders is easily understood: steam passes from the boiler to each high-pressure cylinder, and, after doing a certain amount of work, it is led from each high-pressure cylinder into the steam-chest of the low-pressure cylinder; it is there expanded down to a still lower pressure, and then exhausted finally up the chimney.

The cylinders of the *Dreadnought* type—that is, the most powerful type of the compounds on the London and North-Western Railway—are: high-pressure cylinders, 14 inches in diameter and 24 inches stroke; the low-pressure cylinder has a diameter of 30 inches and 24 inches stroke. These engines are designed in such a manner that, when working at their usual speed, the power developed by the high-pressure cylinders, and applied through the hind pair of wheels, shall be about equal to the power of the low-pressure cylinder, and applied to the front pair of driving wheels.

Through the kindness of Mr. Webb I am able to give an account of the working of a special train, run in order to test the fuel and water consumption of this class of engine.

On April 17, 1887, the engine *Dreadnought*, No. 503, worked a special train of coaches and dynamometer car from Crewe to Wolverton, a distance of 105¾ miles, at a speed of 24 miles per hour, including stoppages, which were made every 15 miles on the journey; 24 cwt. of coal were put into the fire-box during the trip, which gives a consumption of 25·4 lbs. of coal per mile; 2629 gallons of water were evaporated, which equals 9·78 lbs. of water per pound of coal consumed. The weight of the train, exclusive of engine and tender, was 259 tons, 3 cwts. 3 qrs, and the mean weight of engine and tender 62 tons 13 cwts., or a total of 321 tons 16 cwts. 3 qrs. for the whole train. This is equivalent to 4·13 tons of train hauled to 1 ton of engine and tender hauling it, and 1·26 oz. of coal per ton per mile.

The same locomotive worked a similar train between the same points on January 1, 1888, but at a speed of 44 miles per hour, with one stoppage only at Rugby, the results being as follows:—30 cwts. of coal were put into the fire-box during the trip, which gives a consumption of 41·3 lbs. of coal per mile; 3608 gallons of water were evaporated, which equals 8·26 lbs. of water per pound of coal consumed. The weight of the train, exclusive of engine and tender, was 256 tons, 18 cwts., and the mean weight of the engine and tender 62 tons 13 cwts., or a total of 319 tons 11 cwts. for the whole train. This is equivalent to 4·1 tons hauled to 1 ton of engine and tender hauling it, and 2·06 oz. of coal per ton per mile.

When the first trip was made, the weather was warm and dry, but during the latter a hard frost prevailed. With this exception the conditions under which the trips were made were practically alike. The difference in fuel consumption between the two trips may be taken as that due to the difference in speed. There are 77 compound locomotives now at work on the London and North-Western Railway, which have run to the end of December 1889 a total of 13,423,798 miles, and several more of the same type are now being built at Crewe Works.

It will be observed that in the Webb type of compound locomotive the design is such that the sizes of the cylinders can be easily increased if necessary to obtain a still more powerful engine, provided, of course, a larger boiler is used, and there is no reason why even the *Dreadnought* should not be the forerunner of still more powerful compounds on the London and North-Western Railway when their use becomes a necessity. It is evident that the use of a third cylinder, with motion and gear, must entail a greater cost for repairs as well as a larger consumption of oil when at work, and that the type of engine does not easily lend itself to a speedy and economical rebuild of ordinary locomotives to the Webb compound type; the system, therefore, is one quite unique in its way, and unlike any of the earlier attempts at compounding locomotives.

Another successful design of compound locomotives is that due to Mr. T. W. Worsdell, the Locomotive Engineer of the North-Eastern Railway, on which railway a large number of compound locomotives are at work. The Worsdell compound is the outcome of many experiments both at home and abroad. There are two cylinders used, and to all appearances the locomotives are similar to the ordinary non-compound locomotive. These two cylinders are of different diameters, and the steam, after doing work in the smaller one, is exhausted into the steam-chest of the larger or low-pressure cylinder, where it is further reduced in pressure by expansion in the cylinder, and afterwards is exhausted. It will be observed that in the Webb system there are two high-pressure cylinders connected to the hind pair of wheels, with the crank-pins, of course, at right angles, and that the low-pressure piston receives no steam from the high-pressure cylinders until the engine has commenced to move. Thus, all the work of starting the train for the first few revolutions of the driving wheels has to be done by the high-pressure pistons, and these are always able to start, in whatever position the wheels may be, because they are in duplicate and have no dead point. In the Worsdell system this is not possible without some special appliance, and it is this particular appliance which constitutes the patented device in the engine which makes it the success it is. In any two cylindered compound engine with cranks at right angles, which is the usual practice, it is possible to easily observe that there are two positions from which the engine cannot start on the admission of steam, because the admission of steam to the low-pressure cylinder depends on the exhaust from the high-pressure cylinder, and the high-pressure piston may happen to be at exactly the end of its stroke, either at the front or the back end—known as being on the centre or dead points. As the Worsdell engine is constructed with two cylinders, as before stated, it will be

interesting to know how the difficulty has been got over. When the high-pressure piston is at the end of its stroke, the low-pressure piston will be at the middle of its stroke, the cranks being at right angles; and if by any means steam could be admitted to the low-pressure cylinder without affecting the high-pressure piston, the engine would, of course, be able to turn round half a revolution, and so place the high-pressure piston immediately in a position to commence its stroke. The "intercepting valve," as it is called, is an arrangement by which the passage between the high- and low-pressure cylinders can be closed, and at the same time admits steam to the low-pressure cylinder when the high-pressure piston is on one or other of its dead points. This arrangement consists of a valve in the passage between the cylinders connected to a small piston in a cylinder placed in a suitable position. The steam supply is taken from the main steam-pipe, and regulated in its passage to the small cylinder by a valve worked from the foot-plate. If the engine refuses to start when the regulator is opened, the lever connected to the intercepting valve apparatus is pulled over. This admits steam behind the small piston, which immediately is forced forward and closes the intercepting valve, at the same time opening a port through which the steam is admitted to the low-pressure cylinder. This starts the engine, and the lever is returned to the running position by means of a spring. The rise of pressure in the passage between the cylinders, owing to the exhaust from the high-pressure cylinder, opens the intercepting valve, and compound working commences. This arrangement is very simple and trustworthy in practice. A large number of Worsdell compounds are now in use in India and elsewhere with admirable results. Where coal costs forty shillings and more per ton, it is very important that the most economical engine should be used.

On the Brighton Railway very economical results have long been obtained with the ordinary locomotives designed by the late Mr. Wm. Stroudley, and are due to the general excellence of design of boiler and engine, coupled with careful driving, induced by the coal premium. If locomotives were generally worked more by the reversing lever and less by the regulator, more economical results would be recorded; or, in other words, expansive working means economical working, which in the ordinary engine depends on the driver. In this manner, to work steam expansively in the non-compound locomotive, it is necessary for the driver to regulate the power of the engine by varying the quantity of steam used in the cylinders by means of an earlier or later cut-off, regulated by means of the reversing gear, the supply from the boiler not being checked in any way when running. On the other hand, the engine can be regulated by varying the steam supply at the regulator, the degree of expansion in this case being such as the driver chooses to generally use. Under the first conditions all the steam used is worked expansively, and under the latter the cylinders are choked with steam at one minute, and have an insufficient supply at the next. On the other hand, with the compound engine the steam must be expanded to a certain extent whether the driver likes it or not, and a result may be obtained with careless driving from the compound which would be passable when shown by a fairly well driven ordinary engine.

Mr. Drummond, the Locomotive Superintendent of the Caledonian Railway, has been making extensive experiments with steam-pressures varying from 150 to 200 lbs. per square inch, with identical engines doing practically the same work, the results of which will be given to the Institution of Civil Engineers. Without dealing with the practical difficulties involved in the use of such high pressures in non-compound locomotives, it will be highly interesting to know the results of these experiments. Whether the saving in fuel will equal or exceed the compound results obtained by Messrs. Webb and Worsdell is a moot point.

It has been observed that the saving of fuel due to a compound locomotive when working similar trains with the non-compound engine is due to the higher pressure used, and that when the pressure is reduced to the same level as that used in the non-compound engine the saving in fuel at once drops considerably, and the results give a little saving in favour of the compound. From this it is evident that to alter an ordinary engine to the compound system, without raising the working pressure, will be of little good, and not worth the cost.

The many statements made in order to prove the more economical working of the compound over the non-compound locomotive are misleading in the extreme, and as a fair comparison of the two types they are of no value. The compound locomotives have large boilers, ample heating surface, and all recent improvements, besides the all-important feature of a working pressure of 175 lbs. per square inch. This engine is compared with an ordinary non-compound locomotive having a smaller boiler, generally hard pressed for steam, because it has to haul its maximum load, with a working pressure of about 150 to 160 lbs. to the square inch. To put two such engines into competition is absurd, and therefore the results obtained by the compound locomotives in every-day working cannot fairly be compared with the non-compound engine's records.

For these and other reasons engineers are anxiously waiting to learn the results of Mr. Drummond's experiments, for then for the first time will it be possible to fairly compare the two systems.

It must not be imagined that because the compound and triple expansion marine engine is so successful in fuel economy, the compound locomotive is also likely to be so: the conditions of working are so totally different; for instance, the engines of an Atlantic liner work for seven or more days, doing practically the same amount of work the whole time, and since the work is constant the engines are designed to do that work in the most economical manner. With the locomotive, on the other hand, the work is never constant, and for that reason the steam supply is an ever-varying quantity, besides the constant stopping and reversing always going on when any shunting has to be done. These conditions are fatal to very economical working, and more especially when applied to a compound locomotive.

The compound principle is a sound one, but one not likely to be generally adopted, on account of extra complication. The present consumption of fuel by ordinary well-designed non-compound locomotives (take, for instance, the Brighton average consumption of 24·75 lbs. per mile for all their passenger engines) has not been beaten by the compound locomotive records; and until it can be demonstrated that a distinct economy is possible by their general use, they are not likely to increase largely in number.

N. J. L.

NEW ZOOLOGICAL PARK AT WASHINGTON.

By an Act of Congress passed on March 2 last year, an "appropriation" was made for the establishment of a Zoological Park in the district of Columbia "for the advancement of science and the instruction and recreation of the people." The control of the establishment was intrusted to a Commission composed of the Secretary of the Interior, the President of the Board of Commissioners of the District of Columbia, and the Secretary of the Smithsonian Institution.

Although the Commission was thus established only a year ago, the three Commissioners have already set to work, and, as we learn from their report, transmitted in January last to the Senate and House of Representatives

have accomplished the first object of the constitution—namely, the purchase of the necessary land.

The site selected for the Zoological Park is about two miles from the centre of Washington. It contains an area of 166 acres, traversed by the stream called Rock Creek, and is stated to possess most attractive features which render it well adapted for the purpose.

There is already a Zoological Garden at Philadelphia in good working order, and there is a smaller establishment at New York, in the Central Park, under the charge of Mr. W. A. Conklin, who is well known to many naturalists on this side of the Atlantic. The new institution at the metropolis of the United States, to be inaugurated and carried on by the Central Government for the "recreation and instruction" of the American people, will evidently be on a much larger scale. It will also have the advantage of the unlimited support always accorded by the Americans to their great national undertakings. If the Commissioners are inclined to take advice from Europe—and we have no reason to suppose the contrary—we should recommend that, before planning and commencing the necessary buildings, they should visit the Gardens of the Zoological Society in London, and the principal institutions of a like nature on the Continent, and take advantage of the experience gained by previous workers in the same field. No amount of plans and estimates, which, we are told, they are now asking for from the older institutions, will give them the advantages to be derived from a personal examination of these establishments and a few weeks' study of the mode in which they are worked.

JAMES NASMYTH.

EVERYONE was sorry to hear of the death of Mr. James Nasmyth, the great engineer. His name is familiar to the entire English-speaking world, and there can be no doubt that he stands in the front rank of those who have advanced the material interests of mankind by the application of science to industrial methods.

So far as outward events were concerned, there was nothing very remarkable in his career. The real history of his life is the history of his inventions. He was born at Edinburgh on August 19, 1808, and was the youngest child of a family of eleven. His father was Alexander Nasmyth, who achieved considerable distinction as a painter. In a good summary of the facts of his life, printed in the *Times* of May 8, it is said that the boy gave very early evidence of a decided taste for mechanical pursuits. At school this taste was strengthened by intimacy with the son of an ironfounder, whose works young Nasmyth was never tired of visiting. He displayed so much aptitude for model-making that when he began to attend scientific classes at the University of Edinburgh he was able to pay his own fees by the sale of models of steam-engines, and other mechanical contrivances.

In 1829, Mr. Nasmyth came to London, and the two following years he spent in the service of Mr. Maudslay, the founder of the well-known firm of engineers. He then returned to Edinburgh, where he devoted himself for a short time to the construction of a set of engineering tools. With these tools, and a very small capital, he ventured to begin business on his own account in Manchester; and so many orders for work were received that new premises soon became necessary. He accordingly secured a plot of ground, 12 acres in extent, at Particroft, near Manchester; and this site he covered with the collection of workshops known as the Bridgewater Foundry. It was at this establishment that Mr. Nasmyth invented and perfected the mechanical tools with which his name is associated. The most important of them is the steam-hammer, the power and delicacy of which are universally known. It was invented in 1839, when he was still a young man. The *Times* says:—"The first idea of the hammer occurred to its inventor when he was asked by the Great Western Railway Company to construct a wrought-iron intermediate paddle shaft for a proposed ship called the *Great Britain*. Other firms had declined to undertake the construction of a shaft with a size and diameter never before attempted. The paddle shaft was never forged, as the screw was invented about this time. But meanwhile Nasmyth had invented a means of raising an enormous block of iron to a sufficient height and of regulating and directing its descent upon the anvil below."

Among Mr. Nasmyth's other inventions we may mention his "reversing direct-acting rolling mill."

In 1857, at the age of 48, he retired from business; and from that time he lived at Penshurst, where he found an outlet for his energies in the enthusiastic study of astronomy—a study which led to the publication of "The Moon considered as a Planet, a World, and a Satellite," written by him in conjunction with Dr. James Carpenter. Mr. Nasmyth wrote also "Remarks on Tools and Machinery," in Baker's "Elements of Mechanism" (1858). An autobiography, edited by Dr. Smiles, was published in 1883. He inherited to some extent his father's artistic faculty, and the exercise of his talent for drawing was a constant source of genuine pleasure.

Mr. Nasmyth used to say that he had never known what it was to be ill. For some time, however, his health was manifestly failing; and several weeks ago he came to town. He stayed at Bailey's Hotel, Gloucester Road; and there, in his eighty-second year, he died, on Wednesday, May 7.

NOTES.

MR. ALFRED GILBERT, A.R.A., has been commissioned to execute the Joule Memorial at Manchester.

PROF. W. K. SULLIVAN, President of the Cork Queen's College, and well known as a chemist, died on Monday at the College. He was 68 years of age, and had held the position of President since 1872, in succession to the late Sir Robert Kane.

IT is announced that Sir Frederick Mappin, M.P., has handed over to his co-trustees of the Sheffield Technical School £1000 for the purpose of founding two scholarships, each of the value of £15 per annum, in perpetuity.

THE Paris Academy of Sciences has offered a prize of 3000 francs for the best essay on the phenomena of fertilization in Phanerogams, especially in reference to the division and translation of the nucleus, and the relation between these phenomena and those which occur in the animal kingdom, to be sent in before June 1, 1891.

PROF. VON NORDENSKIÖLD lately announced to the Stockholm Academy of Sciences that a scientific expedition would start during the summer for Spitzbergen. Among the party will be his son, M. G. Nordenskiöld, and MM. Klinckowström and Bahaman. The expenses of the expedition will be defrayed by Baron Dickson and M. F. Beijer, the publisher.

THE ethnological collections made by Prof. Bastian during his journey through Russian Central Asia, have been brought to Berlin by the Professor's companion, Herr A. Dsirne. Prof. Bastian is at present at Madras.

DR. THORODDSEN, of Reikjavik, to whom the Linné Memorial Medal has been given by the Stockholm Academy of Sciences for his collection of fossil plants, has received 1200 kronen (£65) from Baron Dickson to enable him to investigate the Icelandic peninsula of Sneefieldness. Dr. Thoroddsen hopes soon to conclude his geological researches concerning this ancient Norse settlement.

Mr. T. G. Paterson, of Edinburgh, has sent to the *Daily News* the following information regarding what will be the most northerly telephone in Europe :—"My brother, Mr. Spence Paterson, H.M. Consul for Iceland, writes to me: 'It is proposed to put up a telephone line between Reikjavik and Havnefiord. The cost of apparatus and construction is reckoned under Kr. 30,000, and a small company is to raise the capital.'"

We learn from *Humboldt* that in connection with the tenth International Medical Congress, to be held this year in Berlin from August 4 to 9, there is to be an International Medico-Scientific Exhibition. The following kinds of objects will be exhibited: new or improved scientific instruments and apparatus for biological and especially medical purposes, including apparatus for photography and spectrum analysis so far as they are of service to medicine; new pharmaceutical and chemical stuffs and preparations; new or improved instruments for operative purposes of medicine, including electrotherapy; new plans and models of hospitals, convalescent homes, disinfection arrangements, baths, &c.; new arrangements for care of the sick, including means of transport, and baths for invalids; newest apparatus for hygienic purposes, &c. Communications (marked "Ausstellungsangelegenheit") should be sent to the office of the Congress, Dr. Lassar, Berlin, N.W., Karlstrasse 19.

The dinner given in honour of M. de Lacaze-Duthiers by the Club called *Scientia*, on April 30, seems to have been a great success. It took place in the Hôtel Continental. M. Charles Richet, who presided, delivered an eloquent speech on the achievements of the Club's guest—"that conqueror of the sea, and apostle of zoology"—calling attention especially to his services as the founder of the marine laboratories of Roscoff and Banyuls.

The *Kew Bulletin* for May opens with an interesting collection of facts relating to efforts which are being made to obtain commercial rubber from the "Abba" tree of West Africa. There are also sections on a mealy bug which has lately been very destructive to cultivated plants at Alexandria; on Mauritius hemp machines; on Siberian perennial flax; and on Liberian coffee.

The several Australian Governments have completed their arrangements with regard to the Mining Exhibition which is to be held this year at the Crystal Palace. According to the *Australian Mining Standard*, the best display will probably be made by New South Wales. Mr. Wilkinson, the Government Geologist, will visit England as the official representative of that colony; and the collections to which high honours were awarded at the New Zealand Exhibition will be sent to London.

The first number of "Records of the Australian Museum," edited by Dr. E. P. Ramsay, the Curator, has been issued. The editor explains that the rapid increase in the collections of the Museum, and the gradual acquisition of extensive series of new, or little known, forms from Australia, New Guinea, and the Pacific islands, have "forcibly brought under the notice of the trustees the necessity of officially publishing the investigations of their scientific staff." Accordingly the "Records" will appear as an occasional periodical. Among the contents of the first number are a report on a zoological collection from the Solomon Islands, by E. P. Ramsay and J. D. Ogilby; a re-description of an Australian skink, by the same writers; a report of a collecting trip to Mount Kosciusko, by R. Helms; general notes made during a visit to Mount Sassafras, Shoalhaven district, by R. Etheridge, Jun., and J. A. Thorpe; and a report on a collecting trip to North-Eastern Queensland during April to September 1889, by E. J. Cairn and R. Grant.

The Aëronautical Society of Great Britain will hold a meeting at the Society of Arts, on Friday, May 16, when a lecture will be delivered by Mr. Henry Middleton, of Slough, on "the fundamental principles of flight, and their application to the construction of flying machines." Mr. Edgar Stuart Bruce will read a short paper on electric balloon signalling, with details of some late experiments in Belgium.

An obituary notice of Theodor Kirsch has been issued as the fifth of the series of "Abhandlungen und Berichte" of the Dresden Zoological and Anthropological Museum, edited by Dr. A. B. Meyer. Herr Kirsch had charge of the entomological department of the Museum. The notice is accompanied by a portrait, and by a list of his writings.

The buildings of the Botanical Museum and Laboratory of the Michigan Agricultural College have been entirely destroyed by fire, and with them the whole of the Wheeler Herbarium, containing over 7000 species, the most complete collection of Michigan plants ever brought together.

The well-known botanical explorer Mr. C. C. Parry died at Davenport, Iowa, on February 20, from an illness following on an attack of influenza.

Dr. Tschirch, of Berlin, has been appointed Professor of Pharmacology in the University of Berne.

Dr. Istvànffy-Schaarschmidt has been appointed Curator of the botanical collections in the National Museum of Buda-Pesth.

Herr J. Bornmüller was engaged during April in a botanical investigation in Asia Minor. He began with the mountainous region in the neighbourhood of Amasia, and proceeded westwards into Galatia and Paphlagonia.

Two slight shocks of earthquake were felt at Sofia on May 10, at half-past 2 and at 3 o'clock in the afternoon. The seismic disturbances travelled in a vertical direction.

The Deutsche Seewarte has just published Part III. of "Ueberseeische meteorologische Beobachtungen," containing a valuable series of observations from distant stations, carefully compiled in the most desirable form. Full particulars are given about the positions of the stations, and the construction of the instruments.

We have received the Report of the Meteorological Service of the Dominion of Canada for the year 1886. It contains, as before, very clear tables of the daily, monthly, and quarterly means, for a large number of stations, and values of bright sunshine for 14 stations. The storm signal service seems to be much appreciated, and to be very successful; Mr. Carpmael states that, whenever a storm of any magnitude occurred, due warning was given to the shipping. The issue of daily weather forecasts has also been very successful—88·6 per cent. having been fully verified. The system of disseminating weather information, by attaching metal disks to the railway cars, has been perfected, and Mr. Carpmael states that these forecasts are as eagerly sought for by farmers and people resident in country districts as by the inhabitants of the towns where they are published. The Report also contains five coloured plates, showing the quarterly and annual distribution of rainfall in Ontario.

The daily and yearly variation, and the distribution, of wind-velocities in the Russian Empire have been fully investigated by Kiersnowski (*Repert. f. Meteor.*). The highest velocities (mean 6·3 metres per second) occur in the Baltic provinces. On the White Sea, on the Caspian, in the region of the North Russian lakes, and on the Steppe, the values are also high; in the forest region and the Caucasus they are low. Towards the interior of Asia the velocity decreases, and in Transbaikalia is the mini-

mum (1·5 m. per second). Further east, towards the Pacific, the velocity increases. In the annual period, the maximum is pretty uniformly in winter, the minimum in summer. A maximum in spring, and a minimum in summer or autumn, are peculiar to the Caspian region, the Ural, and West Siberia, with Central Asia. In Eastern Siberia the minimum is in winter. The daily variation shows distinctly the connection with cloudiness. The greatest amplitude occurs in the brighter part of the year; in East Siberia in winter, and in the rest of the country in summer. In general the amplitude increases regularly with the clearness of the sky eastward, and on land it is greater than on the sea.

A STATISTICAL investigation of lightning-strokes in Central Germany, covering a period of 26 years, has been recently carried out by Herr Kastner (*Globus*). The number of cases has increased about 129 per cent., and last year (1889) it amounted to 1145. The author distinguishes four thunderstorm paths. The starting-points of all these are in hills, and in their course, the woodless districts and flat country, river-valleys and low meadow-ground about lakes, seem specially liable, while the wooded and hilly parts generally escape. The hottest months (June, and especially July) and the hottest hours of the day, or those immediately following them (3 to 4 p.m.), show the most lightning strokes.

IN *Le Globe* for March, M. E. Chaix has an article on the general circulation of the ocean. He enters into the various means adopted for determining ocean currents, and the history of the various theories from the earliest times, and gives a brief summary of those which are now generally adopted. The author adopts the opinion expressed by Humboldt, that several causes must be sought for, and that they cannot be explained by any single one. His conclusions are: (1) that differences of density, especially those caused by temperature, induce a slow progression of the water at a depth towards the equator, but that their action is apparently nothing at the surface; (2) that the prevailing winds cause sensible currents at the surface, and these movements in time penetrate to a certain depth, but that their agency does not explain everything; (3) every motion, whether on the surface or at a depth, causes a compensating movement, either slow or rapid. These movements play the second part in the superficial circulation, and explain generally what cannot be attributed to the direct action of the wind; therefore they afford a key to a number of apparent anomalies.

IN the last report of the Central Park Menagerie, New York, it is noted that the principal cause of death among the animals in 1889 was congestion of the lungs. Among the most valuable specimens lost by this disease were—a lioness, purchased March 4, 1886; two pumas, one received in 1883, the other in 1885; one llama and one emu, both purchased in 1888; one sea-eagle and one migratory pigeon, the former of which had been in the collection for eighteen, the latter for thirteen, years. The death of a young hippopotamus, four days after birth, was also attributed to congestion of the lungs. In describing what happened in the case of this interesting creature, Mr. W. A. Conklin, the Director, points out that the Zoological Gardens of Europe have been particularly unfortunate in regard to the first-born of the hippopotamus. "The first two born in the London Garden lived two and four days respectively. The first two born in the Jardin des Plantes, Paris, were killed by their parents shortly after birth. In the Amsterdam Garden the first two died from the neglect of their parents, and in St. Petersburg Garden the first three died from the same cause."

IN a note in the current number of the *American Naturalist*, Mr. F. F. Payne, of Toronto, records an interesting fact which often came under his notice during a prolonged stay at Hudson's Strait. "Here," he says, "the Eskimo might often be seen lying at full length at the edge of an ice-floe, and, although no seals could be seen, they persistently whistled in a low note similar to that often used in calling tame pigeons, or, if words can express my meaning, like a plaintive phe-ew, few-few, the first note being prolonged at least three seconds. If there were any seals within hearing distance they were invariably attracted to the spot, and it was amusing to see them lifting themselves as high as possible out of the water, and slowly shaking their heads, as though highly delighted with the music. Here they would remain for some time, until one perhaps more venturesome than the rest, would come within striking distance of the Eskimo, who, starting to his feet with gun or harpoon, would often change the seal's tune of joy to one of sorrow, the others making off as fast as possible. The whistling had to be continuous, and was more effective if performed by another Eskimo a short distance back from the one lying motionless at the edge of the ice. I may add that the experiment was often tried by myself with the same result."

A NEW instalment of the "Palæontologia Indica" has been published. It forms the first part of vol. iv. of the series dealing with "salt-range fossils," by Dr. William Waagen. This volume is being written in fulfilment of a promise made by Dr. Waagen when, in 1879, he began his publications on the different rock-groups of the salt-range and the fossils contained therein. He then undertook to collect in a special volume "all the geological conclusions that may be drawn from the detailed study of the different faunæ, and to give at the same time geological details as to the occurrence of the single forms."

THE Straits Branch of the Royal Asiatic Society, has published the twentieth number of its Journal. It contains a report on the destruction of coco-nut palms by beetles, by H. N. Ridley; British Borneo—sketches of Brunai, Sarawak, Labuan, and North-Borneo, by W. H. Treacher; notes on names of places in the island of Singapore and its vicinity, by H. T. Haughton; journal of a trip to Pahang, &c., by W. Davison; and a list of the birds of the Bornean group of islands, by A. H. Everett. A map of Borneo, and a map of Palawan and adjacent islands, are given. The former shows roughly the distribution of highlands and lowlands in Borneo, and the localities at which collections of birds have been made are indicated.

CLOSE to the Hungarian village of Toszeg, on low ground often flooded by the Theiss, are the remains of a prehistoric settlement, which have been recently described by a Scandinavian man of science, M. Undset. While in Upper Italy a sort of basin seems often to have been made with an earth-wall, and dwellings built in this on a pile-supported platform; the buildings near Tozseg have been similarly raised in two long parallel trenches. The hollow space under the platform served as a place for refuse of all sorts, and it must often have held stagnant water. When it got full, the settlement appears to have been burnt down, and a new set of buildings raised on new and higher piles. Among the remains are bones of cattle, stags, goats, swine, &c., vessels made of horn, stone, baked clay, a few bronze articles (needles, knives), polished stone hammers, wedges, chisels, tooth-ornaments, &c. The settlers seem to have practised agriculture, hunting, and fishing. Discussing this "find," M. Undset has some remarks on the relations of the prehistoric civilization of Hungary to that of Upper Italy and other European regions. In Northern Italy the bronze period proper appears to have corresponded pretty nearly with the *terramare* settlements; but in Hungary it was much longer, and was in great part contemporaneous with the iron period in Italy. When the bronze period began in Hungary is very doubtful, but M. Undset considers it to have been not later than in Upper Italy. It is highly probable that the very early migration of Italians into the Apennine peninsula, and the migrations into the Balkan

peninsula culminating in that of the Dorians, came from the middle or lower Danube valley. Hence the importance of prehistoric remains in Hungary for a knowledge of prehistoric events in Central Europe.

ANOTHER important paper is contributed by M. Moissan to the current number of the *Comptes rendus* upon carbon tetrafluoride, CF_4. Five modes of preparing the gas are described, together with several new properties which have been investigated since the publication of the preliminary notice a few weeks ago. When gaseous fluorine is allowed to enter a platinum tube filled with marsh gas, CH_4, a violent combination, accompanied by incandescence, takes place, carbon being deposited and a mixture of various fluorides including carbon tetrafluoride formed. Fluorine also reacts somewhat violently with chloroform, $CHCl_3$. When the free element is led into cooled chloroform it is largely absorbed, carbon tetrafluoride being again produced, and for the most part remaining dissolved in the excess of chloroform. If the fluorine is heated to 100° before passing into the chloroform incandescence occurs, a flame appears at the exit opening of the platinum apparatus, carbon is again deposited, and the tetrafluoride largely found in the gaseous product. Fluorine also expels chlorine from tetrachloride of carbon, CCl_4, for if it is led into a quantity of the tetrachloride contained in a gently-warmed platinum flask, the issuing gas is found to be a mixture of free chlorine and carbon tetrafluoride. A large proportion of the latter gas remains dissolved in the excess of carbon tetrachloride, and may be readily obtained fairly pure by gently boiling the residual liquid in a glass vessel and collecting the gas over mercury. As described in our notice of the preliminary paper the lighter varieties of amorphous carbon, such as wood charcoal and lamp black, take fire in a stream of fluorine and continue burning as long as combination occurs, the product consisting of several gaseous fluorides, of which the tetrachloride is present in greatest proportion. The method, however, by which carbon tetrafluoride can be prepared most conveniently and in the purest form is as follows. A quantity of silver fluoride, AgF, is placed in a brass U-tube fitted with two side tubes. Through one of these latter a stream of vapour of carbon tetrachloride is driven; the other serves as exit tube for the products of the reaction. The apparatus is first filled with carbon tetrachloride vapour, the portion containing the fluoride of silver is then heated to 195°–220° C. and a steady stream of the tetrachloride maintained as long as gas is evolved at the mercury trough. It is advisable to add to the apparatus a small metallic spiral tube which can be cooled to − 23° in order to condense any escaping vapour of the tetrachloride, and which is so arranged that the condensed liquid can be returned to the vessel in which the tetrachloride is being vapourized and so passed again into the reaction tube. The last traces of carbon tetrachloride may be removed by allowing the gas to stand twenty-four hours over mercury in contact with a few scraps of caoutchouc. In order to free it from admixed heavier fluorides advantage is taken of the fact that large quantities of the tetrafluoride are absorbed by absolute alcohol. On agitation with a little absolute alcohol, therefore, the tetrafluoride is absorbed, and may be again liberated either by addition of water, in which the gas is scarcely perceptibly soluble, or by ebullition. If the latter plan is adopted the alcohol vapour may be removed by washing through sulphuric acid. It is important to use a metallic reaction tube in the preparation, inasmuch as glass is rapidly attacked by carbon tetrafluoride, the product of the reaction in a glass vessel consisting of a mixture of silicon and carbon tetrafluorides, carbon dioxide, and a heavier fluoride of carbon, $CF_4 + SiO_2 = CO_2 + SiF_4$. Carbon tetrafluoride liquefies at − 15° at the ordinary atmospheric pressure, and under a pressure of four atmospheres at 20°. When passed over heated sodium it is completely absorbed, carbon being deposited and sodium fluoride formed. Aqueous potash appears to be without action upon it, but alcoholic potash slowly absorbs it with formation of carbonate and fluoride of potassium.

THE additions to the Zoological Society's Gardens during the past week include a Blossom-headed Parrakeet (*Palæornis cyanocephalus* ♂), two Red-eared Bulbuls (*Pycnonotus jocosus*) two Red-vented Bulbuls (*Pycnonotus hæmorrhous*), a Large Hill-Mynah (*Gracula intermedia*) from India, a Red-sided Eclectus (*Eclectus pectoralis* ♀) from New Guinea; two King Parrakeets (*Aprosmictus scapulatus* ♂ ♀), a Pennant's Parrakeet (*Platycercus pennanti*), a Chestnut-eared Finch (*Amadina castanotis* ♀) from Australia, a Ceylonese Hanging Parrakeet (*Loriculus asiaticus*) from Ceylon, a Mealy Amazon (*Chrysotis farinosa*), two Yellow-shouldered Amazons (*Chrysotis ochroptera*), a Blue-fronted Amazon (*Chrysotis æstiva*), a Red-crested Cardinal (*Paroaria cucullata*) from South America, a Levaillant's Amazon (*Chrysotis levaillanti*) from Mexico, two Panama Amazons (*Chrysotis panamensis*) from Panama, a Yellow-vented Bulbul (*Pycnonotus crocorrhous*) from Sumatra, two Orange-cheeked Waxbills (*Estrelda melpoda*), two Red-bellied Waxbills (*Estrelda rubriventris*), a Cut-throat Finch (*Amadina fasciata* ♂), a Shining Weaver Bird (*Hypochera nitens*), an Olive Weaver Bird (*Hyphantornis olivaceus*) from South Africa, a Crimson-crowned Weaver Bird (*Euplectes flammiceps*), a Grenadier Weaver Bird (*Euplectes oryx*), a Green Glossy Starling (*Lamprocolius chalybeus*) from West Africa, two Madagascar Weaver Birds (*Foudia madagascariensis* ♂ ♀) from Madagascar, a Red-headed Cardinal (*Paroaria larvata*) from Brazil, a Cardinal Grosbeak (*Cardinalis virginianus* ♀) from North America, presented by Dr. Seton; a Red-eared Bulbul (*Pycnonotus jocosus*), a Red-vented Bulbul (*Pycnonotus hæmorrhous*) from India, presented by Lieut.-General Sir H. B. Lumsden, K.C.S.I.; a Ring-necked Parrakeet (*Palæornis torquatus* ♀) from India, presented by Mrs. O. Harvey; a Redwing (*Turdus iliacus*), British, presented by Mr. J. Newton Hayley; a Common Viper (*Vipera berus*), a Slowworm (*Anguis fragilis*), British, presented by Dr. W. K. Sibley; three Green Tree Frogs (*Hyla arborea*) from France, presented by Mrs. Humphreys; two Hartebeests (*Alcelaphus caama* ♂ ♀) from South Africa, a Bennett's Wallaby (*Halmaturus bennetti* ♂) from Tasmania, a Black Wallaby (*Halmaturus walabatus* ♀), two Brush-tailed Kangaroos (*Petrogale penicillata* ♂ ♂) from New South Wales, four Common Quails (*Coturnix communis*), European, deposited; two Demoiselle Cranes (*Grus virgo*) from North Africa, purchased; a Japanese Deer (*Cervus sika* ♂), a Hog Deer (*Cervus porcinus* ♀), ten Cuming's Octodons (*Octodon cumingi*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on May 15 = 13h. 34m. 18s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) Uranus, May 15 ...	—	Bluish-green.	13 27 40	− 8 33
,, ,, 29 ...	—	—	13 26 4	− 8 24
(2) G.C. 3615	—	—	13 32 4	+ 9 27
(3) 163 Schj.	7	Reddish-yellow.	13 48 29	+40 47
(4) 84 Virginis	6	Yellow.	13 37 30	+ 4 6
(5) ζ Virginis	3	White.	13 29 12	− 0 2
(6) T Ursæ Maj.	Var.	Red-yellow.	12 31 23	+60 6

Remarks.

(1) A question of great interest was raised last year by spectroscopic observations of the planet Uranus. As is well known, the spectra of Uranus and Neptune differ very widely from those of the other planets. They show no solar lines in the visible spectrum even when telescopes of very large apertures are em-

ployed; but in place of them there are several broad dark bands to which no origins have yet been assigned. Secchi first observed the spectrum in 1869, and he pointed out that, if the luminosity of the planet be due to solar light, it must undergo great modifications in the atmosphere of the planet. Vogel and Huggins re-observed the spectrum and mapped it in considerable detail, Vogel giving the positions of no less than twelve bands. The five principal bands have the following positions:—

618. Darkest part of a broad band, ill-defined towards the red.
596. Middle of a faint narrow band.
573·8. Darkest part of a broad band.
542·5. Middle of darkest band in spectrum.
486·1. Middle of a dark band.

It has not yet been possible to explain any of the dark bands by comparisons with known absorption spectra; one band is certainly coincident with the F line of the solar spectrum, but it is much too broad to be due to reflected sunlight. Prof. Lockyer gave his attention to the spectrum last year, and thought it possible that many of the apparent dark bands were simply contrast appearances due to the presence of radiation flutings. At his suggestion I examined the spectrum, and came to the conclusion that he was right. I afterwards made observations, in conjunction with Mr. Taylor, with Mr. Common's 5-foot reflector. A full account of the results is given in Mr. Taylor's paper on the subject (*Monthly Notices*, vol. xlix. p. 405). We decided in favour of bright flutings, and Mr. Taylor afterwards mapped them very carefully. The brightest fluting is near *b*, and it is remarkable that Secchi noted this brightening in his light-curve of the spectrum. Mr. Espin has since observed that the blue end of the spectrum is broken up into bright bands and dull shadings. If the apparent bright flutings are not contrast effects, as has been suggested, the planet must be to a great extent self-luminous. Dr. Huggins has since photographed the violet end of the spectrum, and finds nothing but solar lines—a fact which is very difficult to explain, when the remarkable character of the visible spectrum is considered.

It is highly important that further observations, by as many different observers as possible, should be made. The apparent diameter of the planet is so small that a Maclean spectroscope shows the bands very well, but the brightnesses are best seen when the spectrum is narrow, as is the case with bright-line stars.

(2) This nebula is thus described in the General Catalogue: "Bright; large; extended in a direction 150°; pretty suddenly brighter in the middle to a resolvable nucleus." The apparent size of the nebula, according to the Harvard College observations, is 3′ × 1′. The spectrum has not been recorded.

(3) This star is one of considerable interest. Dunér says: "It appears to have a narrow band in the red, and a wide one in the green. Perhaps III.*a* (Group II.), but by no means III.*b* (Group VI.)." It has been suggested, from a discussion of the other members of the group, that the star is a representative of the very earliest stage of Group II., but further details are necessary before it can be said with certainty. The condition here should be almost cometary, and hence, in further observations, the bright flutings of carbon should be particularly looked for. So far, this is the only observed star which may possibly belong to the first species of the group.

(4 and 5) These stars are included in Vogel's spectroscopic catalogue. The first is stated to be of the solar type, and the second of Group IV. The usual observations are required in each case.

(6) This variable will reach a maximum about May 20. The range is from 6·4–8·5 to 13 in 255·6 days. The spectrum is one of Group II. (Dunér), the bands being wide, but not very dark. The usual observations for bright lines and other variations are suggested. A. Fowler.

Changes in the Magnitudes of Stars.—At the April meeting of the Royal Astronomical Society Mr. Isaac Roberts presented a photograph of stars in the regions of Tycho Brahe's Nova taken on January 12, with an exposure of 2 hours 55 minutes. D'Arrest charted the stars in the region of the Nova in 1864 down to the 16th magnitude, and this chart has been used by Mr. Roberts to compare with his photograph. He finds no appearance of either a nebula or of a star on the photograph in or about the position indicated by D'Arrest, but a comparison of the chart and catalogue with the photograph shows that changes have taken place both in the positions and magnitudes of several of the stars since 1864. The changes particularized are important when it is considered that they apply to less than half a degree in right ascension, and one degree in declination. That six of the stars shown on D'Arrest's chart and not shown on the photograph, are absent on the latter on account of some physical change having taken place in the stars, receives confirmation from the fact that the photograph shows more than 400 stars on a sky space where D'Arrest has charted only 212 stars.

A Mechanical Theory of the Solar Corona.—Prof. Schaeberle of the Lick Observatory, has propounded an entirely novel theory of the solar corona, a discussion of which will appear in the report of the eclipse of December 22, 1889. His investigations seem to prove that the corona is caused by light emitted and reflected from streams of matter ejected from the sun by forces which in general act along lines normal to the surface. These forces are most active near the centre of each sun-spot zone. Owing to the change of the position of the observer with reference to the plane of the sun's equator, the perspective overlapping and interlacing of the two sets of streamers at these zones causes the observed apparent change in the type of the corona. To roughly test the theory Prof. Schaeberle has stuck a lot of needles in a ball to represent the streams of matter, placed the model in a beam of parallel rays, and allowed its shadow to fall upon a screen, the result being that an infinite variety of forms similar to the coronal structure can be reproduced by simply revolving the model. It remains to be proved whether a comparison of the forms that are seen according as the observer is above, below, or in the plane of the sun's equator, agree with those that should be seen on this theory.

THE IRON AND STEEL INSTITUTE.

The annual meeting of the Iron and Steel Institute was held on Wednesday and Thursday of last week, in the theatre of the Institution of Civil Engineers, the President, Sir James Kitson, occupying the chair. There was a fair programme of ten papers, and another was added after the list had been printed. The following were the papers read:—

On a new form of Siemens furnace, arranged to recover waste gases as well as waste heat, by Mr. John Head, London, and M. P. Pouff, Nevers.

Calculations concerning the possibility of regenerating the gas in the new Siemens furnace, by Prof. Åkerman, Stockholm.

On the critical points of iron and steel, by M. F. Osmond, Paris.

On the carburization of iron by the diamond, by Prof. W. C. Roberts-Austen, London.

The changes in iron produced by thermal treatment, by Dr. E. J. Ball, London.

On the Robert-Bessemer steel process, by Mr. F. Lynwood Garrison, Philadelphia.

Aluminium in carburetted iron, by Mr. W. J. Keep, Detroit.

On certain chemical phenomena in the manufacture of steel, by Mr. W. Galbraith, Chesterfield.

The estimation of phosphorus in the basic Siemens steel bath, by Mr. W. Galbraith, Chesterfield.

On the Rollet process for producing purified castings, by Mr. A. Rollet, St. Etienne.

The first six of these papers were read and discussed at the first day's sitting (Wednesday); and the remaining four were disposed of before lunch-time on Thursday. It is seldom that we have seen papers "rattled off"—the phrase most aptly describes the procedure—in so rapid a manner. The members who were present may certainly be congratulated upon having got through a great many papers in a very short space of time; but it is a question whether there would not have been a gain to knowledge had the discussions been of a somewhat more deliberate nature.

In addition to the above papers there was on the agenda a memoir by Sir Henry Roscoe, on the action of aluminium on iron and steel. This, however, was not forthcoming; a fact which is to be regretted, as also is the cause which led to it, the subject being one of considerable scientific and industrial importance at the present time, when the production of aluminium is being so much cheapened, and such great things are promised by those who advocate its use in the metallurgy of iron and steel. Fortunately Mr. Keep's paper was forthcoming, and this elicited a brief but useful discussion, in the course of which Mr. James Riley, of the Steel Company of

Scotland, and others, gave the valuable information drawn from the practical experience they had gained in the alloying of iron with aluminium on a large scale. We will, however, take the papers so far as space will permit, in the order in which they were read.

Mr. Head's paper was read at the Paris meeting of the Institute, held last autumn, but was not then discussed. The author first points out that, in 1817, the Rev. Robert Stirling and his brother James Stirling applied the regenerative principle to air-engines, and that both they and J. Slater, in 1837, and R. Laming in 1847, foresaw the possibility of its application to metallurgical operations. The new Siemens furnace, which was the subject of the paper, was described and illustrated by wall-diagrams, without reproducing which it is not possible to make the arrangement clear. The chief point is that the waste gases are reconverted into combustible gases by being taken partly through an air-regenerator, and partly under the grate of the producer, so that they distil the hydrocarbons from the coal; in fact, the gas-producer utilizes the heat formerly deposited in the air regenerators. A steam jet is used for starting the action. This new form of regenerative gas-furnace has been applied to the heating and welding of iron. It is to be used for puddling, and for copper and steel melting. It is claimed that it effects a saving in fuel of about two-thirds the weight, a reduction in the weight of iron equal to 5 per cent., and a saving in labour and repairs. Figures were quoted supporting these claims.

Prof. Åkerman's paper was a discussion on the theory of combustion raised by the process. The subject is one of considerable interest, and is well put forward by the author. His conclusions are of considerable interest from a philosophical point of view, but are to a great extent robbed of their importance from an operative standpoint, from the uncertainty existing upon the specific heat of gases at high temperatures; which is only one more fact emphasizing the want of a proved and trustworthy pyrometer.

A brief discussion followed, in which Sir Lowthian Bell took the chief part.

M. Osmond's paper was one of those which must be the despair of the writers of brief notices such as this. It consisted of 33 pages, giving results of experiments made to ascertain the effects of varying temperature on different alloys of iron. In the presence of such a mass of matter as this we can only refer our readers to the Transactions of the Institute, where they will find the facts detailed and the diagrams by which they were illustrated fully set forth. We will content ourselves with simply stating that the "critical points" are points of arrestation in the cooling of iron and steel. It is interesting to notice the effect of various alloys on this phenomenon. Perhaps, to those members who were not previously acquainted with the instrument, the description of the thermo-electric pyrometer of Le Chatelier was not the least interesting part of the paper. A valuable bibliography is given in an appendix. In the discussion Mr. Wrightson gave some particulars of experiments he had made to ascertain the change of volume of iron at different temperatures, which he did by plunging an iron ball into liquid iron. The ball would at first sink, but rise as it acquired heat, and indications were thus obtained, which appeared to correspond with the "critical points" of the author. Mr. Hadfield also made some interesting remarks on the state of carbon in iron.

Prof. Roberts-Austen's paper followed. The Professor is not, of course, the first to carburize iron by means of the diamond; indeed, it has been a somewhat favourite experiment, with which the name of more than one eminent physicist in times past has been associated. But Prof. Roberts-Austen is, we believe, the first to perform the operation *in vacuo*, the iron itself being previously heated *in vacuo* to deprive it of its occluded gas. The author of the paper refers to the experiments of Hempel, who heated diamond and iron in an atmosphere of nitrogen perfectly free from oxygen, and points out that his, the author's, experiments are interesting from the assertions made by a certain school of chemists that no two elements can react on each other unless a third be present. "It would appear, however," Prof. Roberts-Austen says, "that a mere 'trace' of such additional element is sufficient to insure combination; for, in the experiments I have described, carbon and iron in their purest obtainable forms were used, and the only additional matter which could have been present was the trace of occluded gas which the iron may possibly have retained." The author is satisfied that combination does not take place until a full red heat is reached.

Dr. Ball's paper dealt with the changes in the magnetic capacity and tensile strength of steel which occur at definite temperatures, and showed how these changes may be made evident when the metal is rapidly cooled in water or in oil. Two samples of steel, one basic Bessemer and the other acid open-hearth, were submitted. Analysis showed that all the elements for which tests are usually made were almost identical, except manganese, of which the percentages were 0·284 for Bessemer, and 0·546 for open-hearth. The results are plotted on three sets of diagrams, one diagram in each set showing the results obtained with tests hardened, from varying temperatures, in water, in oil, and annealed respectively; the sets of diagrams refer to unstrained bars, the same bars strained to the yield point, and the same bars strained almost to the breaking point. These last two papers were discussed together.

Mr. Garrison's paper was read at the Paris meeting of September last, but not then discussed. It describes an elliptical converter in which the *tuyères* are so arranged that they blow air at the surface of the metal in a manner which causes a rotary motion of the bath, combustion taking place at the surface. The device is not altogether new, as surface blowing was suggested, and, indeed, patented, by Sir Henry Bessemer in the early days of the Bessemer process.

Mr. Keep's paper on aluminium, in carburetted iron, was the first taken on the Thursday morning. In it the results of certain tests were given, the details being set forth in graphic form. The points noted were strength to resist both weight and impact, deflection, set, elasticity for stresses applied, shrinkage for cast metals, hardness, and rigidity. This paper must be read with others that have been brought forward by the author, whose work in connection with the subject is well known. As a general result the tests go to show that the effect of a proper quantity of aluminium on commercially pure iron is to produce a material which is soft, easily bent, and flows readily. Aluminium diminishes deflection by decreasing the set and elasticity. Rigidity is also increased, the grain is closer and more uniform; in short, the author claims that by aluminium the metal is improved in every way when considered as a structural material.

In the discussion which followed the reading of the paper, Mr. James Riley, the manager of the Steel Company of Scotland, said that he had tried the effect of aluminium in steel on a large scale, but had been disappointed in the results. There were advantages, but these were so slight as to be insufficient to pay for the additional expense of one to two pounds a ton. Fluidity was gained, tensile strength was very slightly increased, the elastic limit was raised considerably, and ductility was increased. If aluminium could be reduced sufficiently in price it would be good to use it, but Mr. Riley had not considered the game worth the candle, and had ceased to use it a year ago. He had, however, been induced lately, by being told of the wonderful results obtained, to make further experiments, but his present frame of mind was not to use aluminium excepting for very fine thin castings. Mr. Spencer, of Newburn, another large steel-maker who has achieved great success in certain special branches of manufacture, endorsed what Mr. Riley had said. Mr. Allen pointed out that there might be traces of aluminium in pig-iron without its being discovered, as chemists only tried, as a rule, for the usual alloys. It was also important to remember that although aluminium might be put into the pot it did not necessarily appear in the product, as it might be removed by chemical reaction during the process. The latter point was supported by Dr. E. Riley and Mr. Stead.

Mr. Galbraith's two contributions were next read and discussed, but do not call for any special mention. Finally, Mr. A. Rollet's paper was read, in which his process of obtaining purified metal for castings was described. The process was illustrated by diagrams, and may be said briefly to consist of eliminating from pig-iron, to be used for the manufacture of particular qualities of steel, sulphur, phosphorus, and silicon. The pig is placed in a special cupola, and is maintained at a very high temperature under a double action, slightly reducing and slightly oxidizing, in the presence of a slag obtained by admixtures of limestone and lime, iron ores, and fluor-spar. By the arrangement of the cupola the metal is separated from the slag as soon as they are removed from the action of the blast in tapping. In this way the phosphorus already eliminated is prevented from going back into the metal, and too great a recarburization is also avoided. The elimination of sulphur is complete up to 99 per cent. and even more; that of phosphorus amounts to 80 or 85 per cent., or even 90 per cent. and more.

A short discussion followed the reading of the paper. The only important point brought forward, however, was a statement by Mr. Hugh Bell that, at Clarence, they had been carrying on a process almost identical with that described by the author. Had he, the speaker, been aware that the plan was in use elsewhere, and had he known a paper was to be read on the subject, he would have come provided with certain figures bearing on the matter.

The meeting then broke up after the usual votes of thanks had been duly passed.

The autumn meeting of the Institute is this year to be held in America. The meeting will be held in New York, and we hear rumours of vast preparations that are being made by the hospitable metallurgists and engineers of the United States to welcome their British *confrères*. Members are left to make their own way to New York, but upon landing they become the guests of the American Institute of Mining Engineers. From an outline programme we have seen, it would appear that the only limit to the excursion will be the time at the disposal of members, which, those who know American hospitality best will agree, is sure to be exhausted long before the good-nature of their hosts.

We should have stated before that Mr. W. D. Allen, of Sheffield, this year has been awarded the Bessemer Gold Medal. Mr. Allen was associated with Sir Henry Bessemer in the manufacture of Bessemer steel from the very first. Indeed, he may be said to have been present at the birth of the invention, and was fully acquainted with the whole process before a single patent was taken out.

A MONUMENT TO A FAMOUS JAPANESE CARTOGRAPHER AND SURVEYOR.

THE *Japan Weekly Mail* contains a report of the unveiling of a monument in Tokio on December 14, 1889, to the memory of Ino Chukei, a Japanese cartographer and surveyor of the early part of the present century. The ceremony was performed by Prince Kitashirakawa, President of the Tokio Geographical Society. The name of Ino Chukei was first made familiar to the Western world by Dr. Naumann, the organizer, and for many years the head of the Geological Survey Bureau of Japan. More lately, Dr. Knott wrote two short biographies of Ino, the one published in the Transactions of the Asiatic Society of Japan (vol. xvi., 1888), and the other as an appendix to the memoir on the recent Magnetic Survey of Japan, published in the Journal of the College of Science, Imperial University (vol. xi., 1888). Ino was by profession originally a brewer, and did not begin his scientific life till he was past fifty. The story of the enthusiastic septuagenarian travelling over the length and breadth of Japan with his quadrant, his azimuth circle, his compass, and his clock is almost a romance. His latitude measurements are still of importance to the cartographer, and his map of Japan has formed the basis of every map since constructed. He finished his grand survey in 1818, after 17 years of travelling and observing. And now, nearly seventy years after his death, a lasting memorial has been raised at Shiba, in Tokio. The ceremony of unveiling the monument began at 2 p.m. on December 14, in the presence of a large company. Amongst those present were Prince Kitashirakawa, Viscount Sano, Viscount Enomoto, Admirals Akamatsu, Nakamuta, and Yanagi, Mr. Hanabusa (Councillor), Mr. Arai, Director of the Meteorological Office, Mr. Watanabe, President of the Imperial University, many of the University Professors, and others. The Chinese Representative, the German Minister, M. Dautremer, of the French Legation, and Profs. Burton, Divers, and Knott, may be named as the diplomatic and scientific representatives of foreign nations. The Naval Band was in attendance, and filled the intervals between the different parts of the celebration with selections of music. Four Shinto priests first went through a religious ceremony, which consisted chiefly of purificatory rites, and an invocation to the spirit of Ino. Mr. Watanabe then read a report, giving a history of the movement, which originated seven years ago with the members of the Tokio Geographical Society, and culminated in the ceremony of the day. The original desire had been to put up the monument on the site of the spot where Ino made the first observations in his grand survey—that is, the point through which the zero meridian was taken. This was at Shinagawa. But it had been found more convenient to raise the memorial at Shiba, within sight of this first station. The monument, designed by Prof. Tatsuna, of the Imperial University, and cast in bronze at the Kawaguchi Foundry, had cost nearly 3800 dollars. The whole of the expenses had amounted to about 4000 dollars, which had been met by voluntary subscriptions from the members of the Geographical Society and many others who desired to contribute their mite. The monument, a graceful obelisk of a dull green tint, was unveiled by Prince Kitashirakawa, a translation of whose speech runs thus:—"What an achievement in cartography was that of learned Ino Chukei! During the eras of *Kansei* and *Bunsei* (1790 to 1820), when Japan, at peace within her own borders, isolated from intercourse with the outer world, divided into a number of mutually-secluded fiefs, and, undisturbed by the cares of coast defence, was content with her own littleness, Ino, his fiftieth year already passed, commenced the study of geodesy, and, equipped with instruments of his own manufacture, devoted eighteen years of toil and suffering to the survey of the empire, bequeathing to posterity the memory of a truly great work. From the point of view of strategical advantage, from the point of view of the progress of civilization, from a domestic as well as from a foreign point of view, Ino undoubtedly was a credit to his country. His name is on the lips of the whole nation. The Emperor himself has bestowed posthumous rank on him and presents on his descendants. Japanese and foreigners have contributed to erect to his memory a monument of dimensions unparalleled in Japan. And it is a privilege conferred on me in this enlightened era that, as President of the Tokio Geographical Society, I am permitted to speak of his achievements and to unveil his monument. I rejoice greatly to take part in this imposing ceremony, and I am persuaded that the spirit of Ino in heaven will share the satisfaction which his posterity must feel on such an occasion. Reverentially, on behalf of this Society, I unveil the monument. May the fame of the illustrious dead grow with the growth of our country's civilization."

After some minutes' interval, Viscount Sano advanced to the foot of the steps that lead up to the pedestal, and introduced to the audience the great-great-grandson of Ino, who bowed and expressed the gratitude of the family for the honour done to their ancestor. Viscount Sano then gave a short biographical sketch of Ino, and an account of his great labours, for which he had earned the never-dying gratitude of his countrymen. This ended the ceremony. Later on, in the rooms of the Geographical Society, a select party assembled to inspect the rude instruments with which Ino carried out his observations. The obelisk is very graceful in form, and beautiful in its setting. As already mentioned, the colour is pleasing, and the inscription is artistic as only an ideographic inscription can be. The monument is 34 feet high, the obelisk itself being 27 feet. A flight of steps ascends to a square platform of masonry in the centre of which the pedestal rests. A railing, the bars of which are curved and puckered up so as to represent sea and clouds according to a common Japanese convention, runs round the outer edge of the platform and down the sides of the steps, allowing free ingress and egress to the pedestal and obelisk. The obelisk faces nearly south, and in its back is a door by which access can be gained to the interior. It is intended to place inside the instruments already spoken of, which were used in Ino's survey.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 1.—"The Development of the Sympathetic Nervous System in Mammals." By A. M. Paterson, M.D.

At the present time two opposite views exist among embryologists regarding the development of the sympathetic system. In both, the segmental formation of the sympathetic cord is upheld. According to the view of Remak and others, it is mesodermal, and formed *in situ*. According to the other view, it is ectodermal. Balfour and Onodi, who have maintained the latter view, differ, however, as to the fundamental origin of the sympathetic system—Balfour regarding each sympathetic ganglion as an offshoot from the spinal nerve, while Onodi considers it as a direct proliferation from the spinal ganglion.

For the present research, mammalian embryos were exclusively employed. The stage in development was first considered in

which the sympathetic system was plainly visible; and from this point the earlier and later steps in the process were traced.

The first event to occur is the formation of the main sympathetic cord, which arises in the mesoblast on either side of the aorta, as a solid, unsegmented rod of fusiform cells produced by the differentiation of cells *in situ*, and not at first connected with the spinal nerves. In front, it ends abruptly at the level of the first vertebral segment; behind the suprarenal body (to which it sends a considerable cellular bundle) it becomes indistinct, terminating at the level of the hind limbs.

This cellular column is, secondly, connected to the spinal nerves by the formation of the white *rami communicantes*. This is effected by the gradual growth of the inferior primary divisions of the nerves, and their final division into *somatic* and *splanchnic* branches. The splanchnic branch extends into the splanchnic area, where it meets and joins the cellular sympathetic cord. In the anterior part of the thorax it appears to end wholly in the cord; in the posterior thoracic and lumbar regions it divides into two parts, of which one joins the cord, the other passes beyond it. In both cases the fibres joining the cord are directly connected with the component cells. Behind the loins the splanchnic branches cease, and in the neck they do not join the sympathetic cord.

The formation of ganglia in the main sympathetic cord occurs subsequently, and is due to (1) the function of the splanchnic branches, the accession of a large number of nerve-fibres at the point of entrance, and the consequent persistence of the component cells (which are joined by these nerves) as ganglion cells; and (2) the anatomical relations of the cord to the bony segments, vessels, &c., over which it passes, and which indent it at certain points. This view is supported by the evidence obtained from dissections of human embryos in the 3rd, 4th, 5th, and 6th months, where the cord forms a band, constricted irregularly at considerable intervals, and from the adult structure, where the "segmentation" of the sympathetic cord is apparent rather than real.

The cervical portion of the embryonic sympathetic cord separates at the origin of the vertebral artery into two unequal parts. The smaller forms a fibro-cellular cord, and accompanies that artery as the vertebral plexus; the larger portion becomes constricted off from the main sympathetic cord by the formation of a fibro-cellular commissure, and forms the "superior cervical ganglion." When the middle cervical ganglion is present, it may be looked upon as a mass of the original cells of the sympathetic cord which have been included in the growth of the commissure.

Posteriorly the sympathetic cord gradually extends from the level of the hind limbs, until in older embryos it can be traced for a considerable distance along the middle sacral artery. It is not joined by splanchnic branches behind the loins.

The peripheral branches from the sympathetic cord arise as cellular outgrowths which accompany the parts of the splanchnic branches which do not join the sympathetic cord into the splanchnic area. They form considerable nerves, which follow the main vessels, and produce parts of the splanchnic nerves, the solar plexuses, &c., as well as the medullary portion of the suprarenal body. The gray *rami communicantes* appear to arise in the same way, and to belong to the same category.

The main conclusions derived from the above investigations are that in its development the sympathetic cord in mammals is mesoblastic, formed *in situ*, and primarily unsegmented, and unconnected with the spinal nervous system.

Linnean Society, May 1.—Mr. J. G. Baker, F.R.S., Vice-President, in the chair.—Mr. Miller Christy exhibited and made remarks on specimens of the so-called Bardfield oxlip, which he had found growing abundantly not only in the neighbourhood of Bardfield, Essex, but over a considerable area to the north and west of it.—Mr. Buffham exhibited under the microscope specimens of *Myristrichia claviformis* with plurolocular sporangia, and conjugation of *Rhabdomena arctuatum*, found upon *Zostera marina*.—The Rev. Prof. Henslow exhibited a collection of edible Mollusca which he had recently brought from Malta, and described the native methods of collecting and cooking them.—Prof. Stewart exhibited some spirit specimens of a lizard, in which the pineal eye was clearly apparent.—Mr. Sherring exhibited a series of excellent photographs which he had taken near Falmouth, and which showed the effects of climatic influence on the growth of several subtropical and rare plants cultivated in the open air.—A paper was then read by Prof. W. Fream, on a quantitative examination of water-meadow herbage.—This was followed by a paper from Mr. R. I. Pocock, on some Old World species of scorpions.

Zoological Society, May 6.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of April 1890; and called special attention to two examples of Simony's Lizard (*Lacerta simonyi*) from the rock of Zalmo, Canaries, obtained by Canon Tristram, F.R.S., and presented to the Society by Lord Lilford.—Mr. Sclater exhibited and made remarks upon the stuffed head of an Antelope, shot by Commander R. A. J. Montgomerie, R.N., of H.M.S. *Boadicea*, in June 1890, near Malimdi, on the East African coast, north of Zanzibar. Mr. Sclater referred this head to what is commonly called the Korrigum Antelope (*Damalis senegalensis*).—Prof. Howes made remarks on a dissection of the cephalic skeleton of *Hatteria*, and pointed out some features of special interest exhibited by this specimen. These were the presence of a pro-atlas and the existence of vomerine teeth, as in *Palæohatteria*.—Two letters were read from Dr. Emin Pasha, dated Bagamoyo, March 1890, and announced that he had forwarded certain zoological specimens for the Society's acceptance.—Mr. H. Seebohm exhibited and made remarks on a specimen of the Eastern Turtle (*Turtur orientalis*), killed near Scarborough, in Yorkshire.—Prof. F. Jeffrey Bell read the first of a series of contributions to our knowledge of the Antipatharian Corals. The present communication contained the description of a particularly fine example of the Black Coral of the Mediterranean, and an account of a very remarkable Antipathid from the neighbourhood of the island of Mauritius.—A communication was read from Mr. E. N. Buxton, containing notes on the Wild Sheep and Mountain Antelope of the Algerian Atlas, taken during a recent excursion into that country. These notes were illustrated by the exhibition of fine mounted specimens of the heads of these animals.—Mr. R. Lydekker read a note on a remarkable specimen of an antler of a large Deer from Asia Minor, which he was inclined to refer to an abnormal form of the Red Deer (*Cervus elaphus*).—Mr. F. E. Beddard read a paper on the minute structure of the eye in some shallow-water and deep-sea species of the Isopod genus *Arcturus*. He pointed out that in all the deep-sea forms there was some change in the visual elements which indicated degeneration.—Mr. E. T. Newton gave an account of the bones of some small birds obtained by Prof. Nation from beneath the nitrate beds of Peru. These bones seemed to occur in considerable abundance, and nearly all appeared to belong to one small species of Petrel, which it was thought most nearly resembled *Cymochorea leucorrhoa* or *C. markhami*, the latter of these being now found living on the coast of Chili.—A communication was read from Dr. Mivart, F.R.S., containing notes on some singular Canine dental abnormalities.—Mr. H. Elwes read descriptions of some new Indian Moths.

Chemical Society, May 1.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—An investigation of the conditions under which hydrogen peroxide is formed from ether, by Prof. W. R. Dunstan and Mr. T. S. Dymond. The authors have investigated the conditions under which hydrogen peroxide is formed from ether (compare Richardson, Chem. Soc. Proc., 1889, 134), and found that ordinary ether, prepared from methylated spirit, yields hydrogen peroxide when exposed for several months to sunlight or the electric light. Contrary, however, to the usual statements, pure ether (either wet or dry) and ordinary ether which has been purified by treatment with dilute chromic acid do not give a trace of hydrogen peroxide when exposed to light under similar conditions. An experiment shows that neither water nor dilute sulphuric acid form hydrogen peroxide when exposed to light in contact with air; the authors refer the production of the peroxide from ether to the presence of a minute quantity of some impurity in the ether employed. Hydrogen peroxide is formed when ozone acts on ether in the presence of water, and is also produced under certain conditions during the slow combustion of ether in contact with water.—Paradesylphenol, by Dr. F. R. Japp, F.R.S., and Mr. G. H. Wadsworth.—Note on Benedikt's acetyl values, by Dr. J. Lewkowitsch.

Mathematical Society, May 8.—J. J. Walker, F.R.S., President, in the chair.—The President announced that a member of the Society, Lieut.-Colonel J. R. Campbell, had asked to be allowed to give a donation of £500 to the Society, the sum to be invested, or otherwise made use of, for the good of the Society, in any way the Council should judge best. On the

motion of the Treasurer, (A. B. Kempe, F.R.S.), seconded by S. Roberts, F.R.S., the following resolution was carried unanimously: That the cordial thanks of the London Mathematical Society be given to Lieut.-Colonel Campbell for his generous gift of £500 to the general fund of the Society.—The following communications were made:—On the function which denotes the excess of the divisors of a number which $\equiv 1$, mod. 3, over those of a number which $\equiv 2$, mod. 3, by Dr. Glaisher, F.R.S.—A table of complex multiplication moduli, by Prof. Greenhill, F.R.S..—On bicircular quartics, by R. Lachlan.—On the genesis of binodal quartic curves from conics, by H. M. Jeffery, F.R.S.—On the arithmetical theory of the form $x^3 + ny^3 + n^2z^3 - 3nxyz$, by Prof. G. B. Mathews.

PARIS.

Academy of Sciences, May 6.—M. Hermite, President, in the chair.—Heats of combustion of the principal nitrogen compounds contained in living bodies, and their *rôle* in the production of animal heat, by MM. Berthelot and André. The data and results are given for sixteen nitrogenous bodies. The average heat of combustion is 9400 cal. for fatty bodies, 5700 cal. for albumenoids, and 4200 cal. for carbohydrates, taking 1 gram of each substance. The conclusion is drawn that a weakening of the organism with diminution of power of consumption of the food digested shows itself first by general deposition of the most difficultly eliminated substances, fatty matters, then by failure to get rid of nitrogenous bodies, and finally by incapacity to consume the carbohydrates.—Some remarks on the subject of spherical functions, by M. E. Beltrami.—Remarks on the loss of virulence in cultures of *Bacillus anthracis*, and on the insufficiency of inoculation as a means of estimating it, by M. S. Arloing. It is known that in a culture of the *Bacillus anthracis* left to itself the virulence after a time disappears. The author gives details of the phenomenon and some results of an examination of various cultures.—MM. Bertrand, Tisserand, and Poincaré reported on a memoir by M. Cellérier entitled "On Variations of Eccentricities and Inclinations." The memoir deals with equations of movement, planetary perturbations, the development of the perturbing function, the study of secular variations, and the differential equations which define them.—On fields of magnetic rotation, by M. W. de Fonvielle.—On algebraical integrals of differential equations of the first order, by M. Painlevé.—Solar phenomena observed during 1889, by M. Tacchini. The distribution in latitude of protuberances, faculæ, spots, and eruptions is given.—On the polarization of electrodes, by M. Lucien Poincaré. The author shows that in the case of melted salts the maximum polarization decreases with the temperature, and becomes *nil* at the temperature of decomposition of the salt, the change is gradual with silver poles, but with gold electrodes there is a sudden fall at the point of decomposition of the electrolyte. Admitting that the maximum of polarization is equal or superior to the equivalent of the energy expended in the electrolytic action, the results point to the theory that an elevation of temperature tends to dissociate a salt by the separation of the two ions of which it is composed, just as occurs, according to M. Arrhenius, in a weak solution.—On the preparation and properties of tetrafluoride of carbon, by M. H. Moissan.—On the reduction of nitric acid to ammonia and a method of estimation of this acid, by M. E. Boyer. The author indicates the exact conditions under which nitric acid may be entirely reduced to ammonia when acted upon by hydrogen liberated in the solution by the action of Zn upon hydrochloric acid, and gives analyses which show that his method yields trustworthy quantitative results.—On the molecular refracting power of salts in solution, by M. E. Doumer. It is shown that the law of molecular refraction is best exemplified when one considers the solutions in a state of dilution such that the density of the salt in the solution, taken in relation to the density of hydrogen, may be equal to the molecular weight of the salt.—The action of oxygenated water upon the oxygen compounds of manganese; Part 2, action upon permanganic acid and the permanganates, by M. A. Gorgeu.—On the amethylcamphophenolsulphonate and a derived tetranitrated yellow colouring-matter, by M. P. Cazeneuve.—Note on tridymite and christobalite, by M. Er. Mallard.—On the zeolites of gneiss from Cambo (Basses Pyrénées), by M. A. Lacroix. It is noted that the zeolites are remarkable for their abundance and the beauty of their crystals. They occur in two distinct beds: (1) in acid gneisses, (2) in basic gneisses. Descriptions of the crystals are given.—On a new method for the analysis of straw, by M. Alexandre Hébert.—On the *rôle* of green manures as nitrogenous dressing, by M. A. Muntz. The author concludes from the results of some experiments that the efficacy of green manures as nitrogenous dressing depends especially on the facility with which the fresh vegetable matters allow the nitrification of the proteids and on the favourable influence which they exercise on the physical properties of soils.—Experiments relative to the transmissibility of hæmoglobinuria to animals, by M. V. Babes.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Class-Book of Geography: W. B. Irvine (Relfe).—A Hand-book of European Birds: J. Backhouse (Gurney and Jackson).—Larva Collecting and Breeding: Rev. J. S. St. John (Wesley).—A Course of Lectures on the Growth and Means of Training the Mental Faculty: Dr. F. Warner (Camb. University Press).—Pure Logic, and other Minor Works: W. S. Jevons (Macmillan).—Terminologia Medica Polyglotta: T. Maxwell (Churchill).—A Guide to the Exhibition Galleries of the Department of Geology and Palæontology in the British Museum (Natural History), Parts 1 and 2 (London).—Geologisk kart over de Skandinaviske Lande og Finland: H. Reusch (Kristiania).—The Elements of Machine Design; Part 1, new edition: W. C. Unwin (Longmans).—Annual Report of the Department of Mines, N.S.W., for the year 1888 (Sydney, Potter).—Seventh Annual Report of the U.S. Geological Survey 1885-86: J. W. Powell (Washington).—The Chemistry of Paints and Painting: A. H. Church (Seeley).—A Smaller Commercial Geography: G. G. Chisholm (Longmans).—Les Aguas Minerales de Chile: Dr. L. Darapsky (Valparaiso, Helfmann).—Notes upon a Proposed Photographic Survey of Warwickshire: W. J. Harrison (Birmingham).—Fjeld og Jordarter i de Skandinaviske Lande og Finland: H. Reusch (Kristiania).—Report of Mr. Tebbutt's Observatory, 1889: J. Tebbutt (Sydney).—Notes on Electric Lighting: Rev. G. Molloy (Dublin, Gill).—Imperial College of Agriculture and Dendrology, Tokyo, Japan, Bulletin No. 7: Y. Kozai (Tokyo).

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THURSDAY, MAY 22, 1890.

THE FUTURE UNIVERSITY FOR LONDON.

THE latest news of the negotiations between the various institutions whose co-operation is necessary for the establishment of a satisfactory system of graduation for London University students is decidedly good. Lord Cranbrook, as the Minister in charge of educational legislation, has intimated to the University that he is prepared to take up the question, and is in expectation of receiving an application for a new charter for the purpose of instituting such a system. The scheme which was drafted by a Committee of the Senate, which was communicated to the University Colleges and to the Royal Colleges of Physicians and Surgeons, and was under discussion at the meeting of the Convocation on the 14th inst., contains some novel features, which show that the University is prepared to move forward, in order to meet the immediate necessities of the situation, beyond the recommendations of the abortive Royal Commission of 1888, and far beyond the *non possumus* of the University witnesses before that Commission. It practically embodies the concession of a separate system of graduation, to be conducted by an administrative Committee of the Senate, upon which the teaching institutions shall be adequately represented, independently of the present system of graduation by open examinations. It contains a further excellent suggestion, that this present system shall also be conducted by a Committee of the Senate, the Senate itself remaining the ultimate authority for both systems, but leaving the details of administration to the two Committees. This plan, which is due to the initiative of the Senate Committee, appears likely to meet objections of Convocation and of the country University Colleges, and must render it easier for the London institutions to accept the Senate as the ultimate authority on the teaching side.

Accordingly we are not surprised to hear that the University Colleges have expressed themselves ready to accept the proposal, and to abandon, subject to a satisfactory settlement of details, their petition for a separate University. We trust that a spirit of mutual concession will continue to sway the counsels of the contending parties, and that we may be able to hail the establishment of the teaching side of the University of London, which will be the real University for London, during the present year. The new system of graduation will follow the teaching in the London colleges and schools, which will be organized for the purpose by the London Committee of the Senate. We trust that it will be complete in itself, and that its administrators will receive powers to develop it without unnecessary restrictions. The development, in particular, by means of what is known as University Extension lectures has been recognized by the Senate Committee as work which properly belongs to the teaching side for London, and should be placed under the London Committee. This removes a difficulty, which might have been serious, in the way of agreement with the University Colleges. Another, which arose from the embodiment by the Senate Committee in their scheme of the Scottish system of examinations—a system considered in England to leave too much to the discretion of the individual professor, and unsuited to the circumstances of London, where there will be, in most subjects, at least two professors—has also, as we are informed, been removed by concessions from the Senate Committee.

Of the points which remain for settlement the most important are the composition of the Committee for London, and the place in the University of the London Medical Faculty. The first is matter for mutual discussion and arrangement between the various institutions and interests concerned. The University Colleges claim that, besides the "Faculty" representatives, or professors, there shall be three representatives upon the Committee of the Council of each of the Colleges. Since the University is not willing that there should be any members on the Committee who are not also members of the Senate, this involves the further point that the six Council members shall be admitted to the Senate. By our latest advices the Senate Committee appear not unwilling to make this further concession, which is deemed indispensable by the Colleges. It can hardly be said to be an extravagant demand, if the importance of the two great Colleges in the teaching system is considered.

With regard to the Medical Faculty, the representatives of the Royal Colleges of Physicians and Surgeons, and those of the hospital schools unconnected with a University College, besides the two University Colleges, will be consulted. The plan recently put forward by a Committee of the Royal Colleges, which had not been in communication with the University Colleges, involved the constitution of a Joint Committee of the University and the Royal Colleges only, for the purpose of administering a system of "pass" degrees in medicine, in which the examinations of the Conjoint Board of the Royal Colleges should be recognized as an equivalent for the present intermediate examinations and B.M. examinations of the University, and a new M.D. degree should afterwards be given, upon a University examination. We are glad to find that the proposal of the Royal Commissioners to hand over the preliminary scientific examination to the Royal Colleges, which has been condemned in these columns, is entirely disapproved by the Royal Colleges themselves. The severance of the scientific education of medical students from that of scientific students generally is to be deprecated in the interests of scientific study. The same argument seems to us to make for the inclusion of the system of medical graduation for London students in the work of the general London Committee; and we should by no means view with favour the proposal for assigning it to a separate Committee, whether constituted jointly by the University and the Royal Colleges, or as a third Committee of the Senate. In either case representatives of the Royal Colleges and of the medical schools may properly find places on the Senate. Why should they not also form part of the General Committee for London, which would thus become the single administering body for all the Faculties, so far as the teaching side was concerned? The proposal of the Royal Colleges to limit the medical degrees, upon the teaching side, to "pass" degrees, and to bar the University in this respect from conferring honours, appears inadmissible. It probably would not have been made by the Royal Colleges had they been aware of the willingness of the Senate Committee to concede the

point to the University Colleges, so far, at all events, as regards honours in arts and science.

We are informed by a legal correspondent that a strong Committee has been formed at Lincoln's Inn to promote reforms in legal education. We trust this may prove the first step to the constitution, on the teaching side of the University, with the co-operation of the Inns of Court, of a real Legal Faculty, on a basis similar to that above recommended for medicine. To separate the professional Faculties from the academical, in a University of the nineteenth century, savours of anachronism.

RECENT ORNITHOLOGICAL WORKS.

Classification of Birds; an Attempt to diagnose the Subclasses, Orders, Sub-orders, and some of the Families of Existing Birds. By Henry Seebohm. Pp. i–xi., 1–53. (London: R. H. Porter, 1890.)

A Hand-book of European Birds, for the use of Field Naturalists and Collectors. By James Backhouse, Junr. Pp. i–viii., 1–334. (London: Gurney and Jackson, 1890.)

THE most important ornithological work which has recently appeared is undoubtedly Mr. Henry Seebohm's "Classification of Birds." Any attempt to arrange the class "Aves" is always warmly welcomed by ornithologists; and whether they agree or not with all Mr. Seebohm's conclusions, they have every reason to be grateful to him for an honest effort to diagnose the existing orders of birds. It has been known to most of us that Mr. Seebohm has been engaged, with his usual energy, in a close study of avian osteology for the last two years, and the present "Classification" is the result of his original studies, combined with a careful digest of the work of his predecessors in the same field—Parker, Fürbringer, Garrod, Forbes, and others.

The author starts with a high purpose, and with a resolve that diagnoses shall be found which shall hold good for each group of birds, and that the combination of characters set forth shall be diagnostic of that group, and of that group alone. No one, therefore, can grumble at the arrangement, because the order can be altered at will, each order and sub-order possessing their absolutely special characters. Two schemes for the higher classification of birds are proposed. In the first one the author recognizes six sub-classes, as follows:—I. Passeriformes; II. Falconiformes; III. Coraciiformes; IV. Anseriformes; V. Galliformes; and VI. Struthioniformes.

In his "Alternative Scheme" he reduces the number of sub-classes into *five*, by merging the Falconiformes, the Anseriformes, and the Galliformes into the sub-classes Ciconiiformes and Galliformes, the latter taking in the Lamellirostres of the first classification, and sending in return the Tubinares and Impennes back to the Ciconiiformes.

The condition of the young at birth forms the groundwork of this second method of classification, which the author approves, but the subject is treated in a method different from that of Sundevall, who also thought highly of the condition of the nestling bird as an element of primary classification, but, according to Mr. Seebohm, he attached an exaggerated importance to some of the facts. That the character of the nestling is bound to play a significant part in the classification of birds we can well understand, but at present the various developments of the downy young are, we believe, but imperfectly understood. Thus we may remark that in the Passeriformes we know at least two exceptions to their diagnosis as given by Mr. Seebohm, viz. in the Shore Lark (*Otocorys alpestris*), and in a curious bird from Ecuador, *Ptilochloris buckleyi*, belonging to the family *Pipridæ*. Other examples will doubtless be found, and yet closer examination will probably demonstrate that the downy stage through which these Passerine birds pass will be of a different fundamental character from the downy stages of other birds.

There can be but little doubt that of the two schemes provided by Mr. Seebohm the second one is the best, but a stumbling-block at first sight appears to be the position of the *Columbæ* in the Passeriformes, and that of the *Cathartes* (*lege* Cathartides) in the Coraciiformes with the Kingfishers and Hornbills. It is perhaps the novelty of these allocations that causes our hesitation in accepting them, for after all a Turkey Vulture and a Ground Hornbill (*Bucorax*) have considerable resemblance. In any case Mr. Seebohm gives characters for the diagnosis of all his Orders and Sub-orders, and their linear arrangement can be shifted at will. Each order and sub-order is not only defined, but a table accompanies every one of them, showing the whole of the thirty-six minor divisions, exhibiting by an asterisk the want of any specified character, and so narrowing the issue of definition in each instance. The author is greatly to be congratulated on the result of his two years' labour, which will doubtless be the stepping-stone to further treatises on the classification of birds.

We cannot congratulate Mr. James Backhouse on his "Hand-book of European Birds." The author's intention doubtless is good, but though "many of the finest bird collections in the Kingdom have been carefully examined, and the best modern authorities have been consulted," the result of all this compilation is not satisfactory, and a want of practical acquaintance with the manner in which a "Hand-book" should be written is apparent at every step. We fear that the outline figure of a bird, drawn by Mr. R. E. Holding, in order to show the nomenclature of the different parts of a bird, will not commend itself to any experienced field naturalist or collector, who will probably know more of his subject than did the artist who perpetrated this figure. We will do no more than point out that the "cervix" is called the "hind neck" by most ornithological writers, that the "malar region" is generally spoken of as "the cheeks," that the positions of the "breast," "abdomen," and "anal region" are all placed wrongly in the figure, and that the "crissum" is not the same as the "lower tail-coverts." The divisions of the back are also wrongly defined. Luckily, the author himself does not recognize the terminology of his own "bird-map," or the confusion of parts would have been disastrous.

We had fondly hoped that, having started the "Birds of Europe" in 1871 (since completed by Mr. Dresser), with the idea that a work of that character should include all the species of the Western Palæarctic region, which is at least a natural division of the globe, it would not

occur to future authors to return to the old idea of treating the avifauna of Europe on political ideas, and fencing in the ranges of the birds with political boundaries. Yet it is on these old lines that Mr. Backhouse has written his "Hand-book," and he must be held responsible for a very retrograde step. From his preface, with the short definition of the six zoogeographical divisions of the earth, one would expect to find that he recognized the value of writing on the birds of a well-defined zoological area, but a glance at the countries which he assigns to the Ethiopian and African regions shows that he does not really understand the subject of geographical regions, for, after stating that the Western Palæarctic sub-region includes the countries *west* of the Jordan, he apparently wishes us to believe that Palestine *east* of the Jordan belongs to the Eastern Palæarctic sub-region, while Asia Minor is to remain in the western part. We should like to know where the regional differences between Asia Minor and Persia, and, for that matter, Palestine and Syria, begin and end. Arabia seems to be left out in the cold, finding a place neither in the Palæarctic nor in the Ethiopian regions, while the Indian region includes Asia south of the Himalayas with the Indo-Malayan Islands and Formosa, as well as *Madagascar!* With such crude notions as to the limits of the regions which adjoin the Palæarctic, it is not to be wondered at that Mr. Backhouse's ideas of the natural limits of the latter are also ill defined. The mischievous results of these notions of the limits of "Europe" are seen in the appendices of North American birds which are "*stated*" to have occurred in Europe. Many of the birds mentioned in his list have undoubtedly occurred more than once, and the incompleteness of the plan of the work is shown by their omission from the body of it, because these species may occur again at any time to the "field naturalist" or "collector," for whom the author specially caters, and these will look in vain for them in this "Hand-book." The same with the list of Asiatic and African species which are *stated* to have occurred in Europe. Many of them *have* occurred in Europe, beyond the shadow of a doubt, and *Certhilauda duponti* (of *C. lusitanica* the author apparently knows nothing), *Sturnus purpurascens*, and *Falco minor*, have as much right to be considered European birds (even in Mr. Backhouse's acceptation of the term), as *Picus lilfordi* or *Cypselus pallidus* (whose range is *not* "probably similar to that of *C. apus*," or anything like it).

The main idea running through Mr. Backhouse's "Hand-book" seems to be the same as was exemplified in Colonel Irby's "Key List to British Birds," but we greatly prefer the plan of the latter pamphlet for its method of execution to the more ambitious work of Mr. Backhouse, wherein most of the mistakes of Dresser's "Birds of Europe" are reproduced, even to the omission of the Astrachan Horned Lark (*Otocorys brandti*)! Besides the faults we have noted, all of which are easily capable of rectification in a future edition, there is one cardinal defect in this "Hand-book," and that is in the assumption that the "field-naturalist" and "collector," for whom the author writes, is minutely acquainted with Palæarctic *genera*, and will know instinctively whether he has a *Hypolais*, an *Acrocephalus*, or a *Lus-ciniola* in his hands.

R. BOWDLER SHARPE.

CRIMINAL ANTHROPOLOGY.

The Criminal. By Havelock Ellis. Illustrated. (London: W. Scott, 1890.)

CRIMINAL anthropology has of late years attracted much attention abroad, where its problems have been largely and often very loosely discussed. Mr. Havelock Ellis performs the useful task of making English readers acquainted with the results. It cannot be said that much progress has been made on the psychological side of the subject since the publication of Despine's "Psychologie" in 1868, but the main conclusions of that author have been abundantly confirmed. On the physical side, numerous dissections and measurements seem to have led to no well established and important fact; they have, however, narrowed the limits within which speculation may legitimately ramble. It is well ascertained that many persons are born with such natures that they are almost certain to become criminals. The instincts of most children are those of primæval man; in many respects thoroughly savage, and such as would deliver an adult very quickly into the hands of the law. The natural criminal retains those same characteristics in his adult life. The author has a very true but not complimentary passage upon the ways of children. He says that the child lives in the present, the desire of the moment blotting out everything else from his mind. That he has no foresight to restrain him from acting according to impulse. That he is a thorough egoist, and will commit any enormity to obtain what he wants. That he is cruel and enjoys the manifestations of pain. That he is a thief for the gratification of his appetites, chiefly of gluttony; and that he is an unscrupulous and often cunning liar, not hesitating to put the blame on innocent persons when his own misdeeds are discovered. In the large majority of our countrymen the savagedom of childhood becomes gradually in part repressed, in part outgrown, and in part transformed. Discipline is one agent, another is the larger growth of sympathetic feelings, and another is the education of a habit of forethought, which prompts selfishness to be wise, and induces many persons to assume throughout life the appearance of virtues for which they have no care, solely through the fear of social or legal punishment. We may freely allow that everybody is liable under some circumstances to fall into crime, for, in the words of the liturgy, "we are set in the midst of so many and great dangers that by reason of the frailty of our nature we cannot always stand upright," but the difference between ordinary persons and natural criminals is that the latter are unable to stand upright even under favourable conditions. There are numerous human beings who have an instinctive aptitude to various forms of ill-doing, no sense of remorse for the sufferings they may have caused, and who possess too little forethought and self-restraint for the fear of retribution to become effective. Abundant evidence of all this is to be found in Mr. Ellis's book, and there seems to be a consensus among experts as to its trustworthiness.

It is easy to understand that ordinary men who are thrown among criminal associates will soon acquire their furtive expression and other peculiarities of demeanour; but after making all allowance for these acquired characteristics there remain certain natural ones that

predominate among all large groups of criminals. These are well set forth by Mr. Ellis, chiefly under the titles of cranial characteristics, physical insensibility, moral insensibility, and emotional instability. A fresh indication of frequent misshape in their heads may be derived from the three composite portraits of criminals (who were by no means of a bad order) that are given in this volume. Here the outlines of the heads of the composites are very hazy, testifying to large and *various* differences in the component portraits. These composites show no prevalence of any *special* deformity in head or features.

The hope of the criminal anthropologist is to increase the power of discriminating between the natural and accidental criminal. He aims at being able to say with well-founded confidence of certain men that it is impossible to make them safe members of a free society by any reasonable amount of discipline, instruction, and watchfulness, and that they must be locked up wholly out of the way. Also, to say of some others that it would be both cruel and unwise to treat them as ordinary criminals, because they have been victims of exceptional circumstances: they are not naturally unfit, and therefore still admit of being turned into useful members of society. Extracts are given in this book from the official reports of the prison at Elmira in the United States, where experiments are made in educating prisoners of the latter class. They describe a system of massages and Turkish baths three times a week, courses of literature, æsthetics, and ethics, including a study of Jowett's translation of the "Republic" of Plato, and of the works of Herbert Spencer, together with a gymnasium and a drum corps, suggesting to the unprepared reader a chapter in Gulliver's account of the institutions of Laputa.

Francis Galton.

ELEMENTARY PHYSIOGRAPHIC ASTRONOMY.

Lessons on Elementary Physiographic Astronomy. By John Mills. (London: Chapman and Hall, 1889.)

THE expressions of approval of the physiography syllabus of the Science and Art Department by the British Association Committee on science teaching lend an additional interest to new text-books of this subject. The book before us covers the portion of the syllabus dealing with the movements of the earth. We believe Mr. Mills has occasionally been employed as an Assistant Demonstrator at the Normal School of Science, and on the strength of this he claims to have had four years' experience as a teacher of the subject in that institution. It is rather late for Mr. Mills to state that, "in the hope of encouraging teachers and students to make the subject a more practical one, instructions have been given for making some inexpensive apparatus," considering that all the practical work given is taken from the book of instructions supplied to students at the Normal School, and which was distributed by the authorities of the Science and Art Department to teachers throughout the country some months ago, with the sole object of encouraging practical demonstrations in classes. Anyone can now obtain the same for twopence. There are many indications that the author is only acquainted with a limited part of the subject. The article on the use of the micrometer, for example (p. 25), is sure to impart the idea that a definite fraction of an inch represents a definite amount of arc, irrespective of the telescope employed; and that, in consequence, the distance between two stars or the apparent diameter of a planet can be stated in inches; further, the zero for position angles is given as "the normally horizontal wire," which is obviously an inconstant, and therefore useless one. Wrong impressions are also given as to the functions of the "Nautical Almanac," for p. 81 distinctly implies that it is a record of actual observations, whereas it is published three or four years in advance. Again, on p. 20, it is stated that the transit circle is made to read 90° when the telescope is pointing to the Pole, and therefore that "when the telescope is directed to any star crossing the meridian we obtain the north polar distance of the star, and this being known, we can easily determine its declination," which is neither clear nor correct.

After deducting the practical instructions, the most casual comparison with Prof. Norman Lockyer's "Movements of the Earth," will show the source of inspiration of the remainder, although there is not a word of acknowledgment. The head-lines, diagrams, and occasionally the language, remind one of that book. The order of things has certainly been slightly changed, but the only result is to introduce disconnections and anticipations. The micrometer, for example, is described before the chapter on angular measurements, and the chronograph precedes that on the measurement of time. The terms "right ascension" and "declination" are frequently used, although the explanation of them is reserved for the very last page. Further instances might be multiplied almost without limit.

The whole book is of a very sketchy character, and the only redeeming feature is the excellent series of diagrams.

A. F.

OUR BOOK SHELF.

Theoretical and Practical Treatise on the Strength of Beams and Columns. By Robert H. Cousins, Civil Engineer, formerly Assistant Professor of Mathematics at the Virginia Military Institute, Lexington, Va. (London and New York: E. and F. N. Spon, 1889.)

THE author of this treatise comes forward with an attempt at an explanation of the *paradox of the beam*, which is that a beam is about double as strong as theory makes out it should be, when the resistance of the beam to bending is calculated from the tension and pressure of the fibres, considered as acting independently and without lateral support.

To account for this discrepancy, which is well known to practical men, a paper by W. H. Barlow, in the Phil. Trans., 1855, proposed a theory of lateral support of the fibres to account for the extra strength, while his careful experiments showed that the neutral plane was certainly very close to the position which theory assigned to it. Previously it had been usual for practical men to place the neutral plane at the top or bottom of the beam, and thence to calculate the strength; a better agreement with theory being thus obtained.

The author of the present treatise adopts the more modern method of taking a different tenacity and modulus of elasticity of the material for extension and for compression; his calculations are principally directed to finding the breaking load of the beam; but as all the

laws of elasticity break down long before breaking takes place, it is not surprising that he should find himself in disagreement with the results of theoretical elasticity.

A summary of the author's theory is given on p. 31, in the shape of ten hypotheses, most of which are of general acceptation, except perhaps number 8, which asserts that "The algebraic sum of the direct forces of compression and extension can never become zero;" while number 4 is redundant, and opposed to the principles of elementary statics.

After the length of time the theory of the beam has been worked at, it is natural to expect the treatment to have fallen into a conventional groove; but there is an unfamiliar appearance about the present pages, which makes it difficult to find out where the originality claimed by the author for his theory comes in; while many of his statements about the position of the neutral line (p. 27) "at the inception of the loading being at the bottom or extended side of the beam, and moved upwards by reason of the deflection and equally with it," are in direct opposition to the careful observations of Mr. W. H. Barlow.

A great many additional pages, reaching to number 166, are devoted to applications to beams of different materials, cast-iron, wrought-iron and steel, and timber; but the method is the same throughout, so that the essence of the book would go into very few pages. The treatise is a great contrast in this respect to most recent American publications on practical subjects.

A. G. G.

Chambers's Encyclopædia. New Edition. Vol. V. (London and Edinburgh: W. and R. Chambers, 1890.)

THE new volume of the present edition of "Chambers's Encyclopædia" deserves in all respects as cordial a reception as that which has been given to the preceding volumes. The editor has done his work with admirable care, selecting for the various subjects writers competent to deal with them, and setting apart for each subject, as nearly as possible, the space that properly belongs to it in accordance with the scheme of the work as a whole. Of the strictly scientific contributions, we need only say that those of them we have been able to examine are sound and concise. With regard to the articles on geology and heat, it may be enough to mention that the latter is by Prof. Tait, the former by Prof. James Geikie, to whom also have been intrusted the articles on the Glacial period and the geology of Great Britain. The climate of Great Britain is the subject of a short but luminous paper by Dr. Buchan. An excellent account of gas and gas-lighting is given by Dr. Alfred Daniell, and Prof. Ewing describes the gas-engine. Mr. Keltie contributes an interesting paper on geography, and Dr. J. S. Mackay writes with his usual clearness on geometry. Mr. F. Hindes Groome's article on the gypsies may be noted as a capital summary of many curious facts and theories. Mr. J. Arthur Thomson, in his article on heredity, displays wide reading and an impartial judgment; and Dr. J. Anderson's article on hill-forts shows how much solid information may be packed into a small space by a writer who knows his subject thoroughly.

Essays of an Americanist. By Daniel G. Brinton, M.D. (Philadelphia: Porter and Coates, 1890.)

MOST of the papers in this volume have already been printed, but some have been substantially re-written, and each of them derives an added value from the fact that it appears in association with other essays on kindred subjects. Dr. Brinton classifies the various papers under the four headings, "ethnologic and archæologic," "mythology and folk-lore," "graphic systems and literature," and "linguistic." To those who are familiar with his contributions to ethnology and anthropology we need scarcely say that the volume sets forth the results of much fresh thought and solid work. In some respects the conclusions at which Dr. Brinton has arrived differ widely from those of most other anthropologists. He holds, for example, what he calls "the specific distinction of an American race," and "the generic similarity of its languages." He is also persuaded that the tribes of this race "possessed considerable poetic feeling," and maintains "the absolute autochthony of their culture." These and other positions he defends with much ingenuity, and even those readers whom he may fail to convince will find that it is worth while to master his arguments. As an example of the thorough way in which he works at his subject, we may note his chapter on the Toltecs, whose far-famed empire he describes as "a baseless fable."

Esquisse Historique sur la Marche du Développement de la Géométrie du Triangle. By E. Vigarié. (Association Française pour l'Avancement des Sciences—Congrès de Paris, 1889.)

THIS is a full and carefully drawn-up sketch of what is sometimes called the modern geometry of the triangle. It carries on the bibliographical notice contributed by M. E. Lemoine to the same Association (1885) up to the present time, and supplies some of the lacunæ in that notice. The author appears to be very fair towards foreign mathematicians, and any deficiencies in noticing English contributions are due to there being at present no account of results which may be buried in such journals as the *Mathematician*, the *Lady's and Gentleman's Diary*, and similar works. We have little doubt that an examination of these would lead to the unearthing of many anticipations of recently obtained results.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Uniform System of Russian Transliteration.

IN NATURE, vol. xli. pp. 396–97, which has only now reached Tashkend, there is a very interesting note under the above title. It is stated that "the recommended system will be adopted without delay." How is this to be understood? Does it mean that the system is finally settled? It would be a pity if this were so, because the proposed method of transliteration contains a point which would be a source of perplexing difficulties when used in practice.

The suppression of the semi-vowels ъ and ь (hard and soft pronunciation) at the ends of words, would make many of them indistinguishable. For instance, with the proposed system, the words

	пыль (dust)	would be	= *puil*
and	пылъ (heat)	,,	= *puil*
	билъ (did heat)	,,	= *bil*
and	биль (a bill)	,,	= *bil*
	яръ (precipice)	,,	= *yar*
and	ярь (verdigris)	,,	= *yar*
	данъ (given)	,,	= *dan*
and	дань (tribute)	,,	= *dan*

and so on.

The differences of these sounds exist for some purpose in the Russian language, and they ought to be rendered in some way in the transliteration. Perhaps the simplest plan would be to adopt the Polish method of denoting the soft pronunciation with an *accent* above the letter in question. The words just given would then be written *dan* and *dań*, *yar* and *yaŕ*, &c.

I may be allowed to make some further suggestions. They are of minor importance, but would tend to improve, in my opinion, the proposed system.

Would it not be more convenient to transliterate the Russian ы with *y*, as is done in the Polish language? The proposed symbol *ui* does not even remotely represent the right sound, and

may cause numerous cases of confusion with the very similar transliterations of the Russian sounds *yu* and *yĭ*. Moreover, English readers are already accustomed to render *ы* with *y*, as, for instance, in the name of the Pribylow Islands. Why should this name be changed now to *Pribuilow*, which is at the same time unfamiliar and misleading? If the change is made, *ѣ*, *ю*, *я*, must be written as *ie*, *iu*, *ia*.

The Russian *ж* might perhaps be better represented by *jh*. The symbol would then represent a softened *j*, equivalent to the French *j*, and reproduce the right sound. A. Wilkins.

Tashkend, April 2/14, 1890.

I assume that the following three conditions must be fulfilled:—

(1) The object aimed at is *principally transliteration*, combined with the possibility of recovering, by its means, the original Russian spelling.

(2) Correct *pronunciation* is only a secondary object, as oral teaching alone can convey it in perfection. Nevertheless, the transliteration adopted should come as near the correct pronunciation as possible, without sacrificing the principal object, transliteration.

(3) The system adopted should satisfy a want, not only of the English-speaking nations, but also, as far as possible, of all those which use the Latin alphabet. This object can be reached only by some mutual concessions.

Now, the system adopted in the article in Nature (vol. xli. p. 397) would, it seems to me, fulfil these requirements as nearly as possible, if the following comparatively slight modifications were admitted:—

(1) The Russian ж would be better rendered by *j* than by *zh*. ж has not quite the same sound as the English *j*, which in most cases has a sound of *d* in it (as in journey, jay, jam). But ж corresponds exactly to the French *j*, and is not too far remote from the German *jot*. For this reason, as a compromise between the three languages, *j*, as an equivalent of ж, would answer better than *zh*. I mean to say that, by means of the *j*, it will be easier for Englishmen, Germans, and Frenchmen to get at the right pronunciation of the Russian ж than through the medium of *zh*.

(2) The Russian ч (*tcha*) should be rendered by *tch* instead of *ch*, and that for the sake of the French and Germans, whose *ch* is pronounced differently from the English *ch*. For the English reader the adoption of either *tch* or *ch* would not involve any difference of pronunciation.

Thus, the Russian *чай* (tea) should be transliterated into *tchaĭ*, which the three nations would pronounce nearly in the same way; whereas, according to the proposed plan, it would be spelt *chaĭ*, which a Frenchman would pronounce *shaĭ* and a German something like *Khaĭ*. *Чихачевъ* should be spelt *Tchikhatchev*, and not *Chikhachev*.

(3) For a similar reason I would propose *stch* for the Russian щ instead of the *shch* of the proposed system. In Russian сч is pronounced exactly like щ (счастіе, *happiness*, is pronounced щастіе, and счетъ, *an account*, щетъ); and for this reason if ч (*tcha*) is rendered by *tch*, the addition of an *s* would make it щ (*stcha*). The *stch* would be more palatable for the French and Germans than the very puzzling *shch*.

(4) I should propose to use the sign ⁀ for indicating the compound letters—thus, $\overset{\frown}{tch}$, $\overset{\frown}{stch}$, &c. This would much facilitate the eventual recovery of the Russian letters.

(5) The last letter of the Russian alphabet, Ѵ (called *ijitza*), is rendered by *oe* in the table (*loc. cit.*). This must be a misprint.

This letter has become almost obsolete in Russian, and is used in the Church Slavonic only. It is the exact equivalent of the Greek *ypsilon*, and should be rendered by *y*.

The requirements of the Italian pronunciation (with its *c* and *ch*[1]) and of the Spanish (with its *j*) are more difficult to satisfy; but most of the educated Italians and Spaniards understand other languages. Ch. R. Osten-Sacken.

Heidelberg, Germany, May 5.

The Eruption of Vulcano Island.

In the pages of Nature two notes have appeared from my pen describing the phenomena of the eruption that commenced on August 3, 1888, which *apparently* is now coming to an end. I have not been able to visit the spot recently, but my friend and pupil Mr. Lewis Sambon, who helped me in conducting the party of English geologists through the Lipari Islands last autumn, and on whom I can thoroughly depend, has given me the information that I make use of in these notes. Mr. J. P. Iddings, whom Mr. L. Sambon accompanied, also kindly confirmed some of the latter, besides which Mr. Sambon brought back a few very good whole-plate negatives.

From September 1889, when I and the geologists were at Vulcano, the eruption has continued with very varying activity. On March 15, 1890, at 9 p.m., there was a very violent explosion resembling the blowing up of a mine. Some windows were broken at Lipari, which is about seven kilometres distant, whilst lapilli reaching the size of large peas, with drops of condensed vapour, were showered upon the town. Behind Monte della Guardia, which hides Vulcano from the town of Lipari, for upwards of three minutes a bright red reflection was seen, which is of importance as indicating the presence of incandescent matter in the volcanic chimney; for there are floating about a number of extraordinary hypotheses, some verging on the magical, to explain this eruption.

After the evening of the 15th, Vulcano was very active, but the explosions were gradually diminishing in force, and completely stopped on the 17th.

On March 25 my two friends visited the island. They found at the base of the cone an enormous number of the *bread-crust bombs*, the mode of formation of which I have already described and explained. These were of recent ejection, and Mr. L. Sambon says they much resemble those of the earlier period of the eruption; and the specimens which have been kindly brought to me thoroughly confirm this view. Both those examined on the island, and the smaller ones I received still contain numerous fragments of dolerite, which, as I have shown, give origin to much of the pyroxenes, magnetite, olivine, and triclinic felspar distributed throughout the paste, and the origin of which is proved by the fact that they are rarely without a bit of the old microlitic dolerite base still attached to them even when very small crystals nearly isolated occur. Such is the amount of impurity of the paste, that any attempt at a chemical analysis would be a waste of time, and even the microscope can afford us little information as to the group of rocks to which the magma belongs. The general facies of the projectiles, the earlier products of this cone, all point to the rock being near to if not really a rhyolitic obsidian. Referring to a discovery I made last autumn Mr. Sambon says:—"I broke a great number of the bombs, but I found in none of them that white agglomeration of quartz and felspar that we often met with in September 1889." These inclusions much resemble numerous similar ones that I found in 1887 in an old lava stream of Stromboli, and which have been sliced, and the examination of which will be published soon. In the meantime they may be said to be composed chiefly of milky quartz and felspar of metamorphic or plutonic origin, and are no doubt the remnants of the sub-volcanic platform.

Some of the recent bombs reach gigantic sizes for such a small volcano. One of these, possibly shot out on March 15, 1890, was, above ground, 9 feet high, 6 feet broad, and 6 feet thick. The obsidian crust was 4 inches thick, and the main fissure, through which the pumiceous interior protruded, was 2 feet broad, forming, as it were, a monster crusty loaf.

So violent were the explosions on March 15, 1889, that Signor Jacono, Mr. Narlian's factor, had to fly for protection with his family to the caves near the Faraglioni, because great stones were falling in considerable numbers near Mr. Narlian's villa, which is about a kilometre from the crater.

Mr. L. Sambon describes the crater as some metres deeper than when we visited it together six months before; but, comparing his and my photographs, there has been very little change. The crater walls were covered with yellow sublimations, which were not so in September 1889, and he judges their inclination at from 40° to 45°. In the centre of the small floor was a great white patch with yellow border, which my experience would lead me to suppose to be due to boric acid, with the edges of a mixture of seleno-sulphur and realgar. A good deal of smoke (which is again new) was issuing from the bottom, especially to the north-north-east; and a few metres only from the edge of the slope beneath the highest point, and extending to where we took our photographs, were a considerable number of fumaroles. One of those nearer the last point (north-north-west) was much larger, issuing from a fissure, and so violent and

[1] In De Gubernatis's "Dizionario Biographico," 1879, *Tchikhatchev* is spelt *Cihaceff*.

menacing as to resemble the old *Caputo*. All of them were roaring, and emitting white fumes.

The fumaroles of the outer rim, including *Caputo*, were very active. These latter worked continuously, whilst the new one on the inner edge would stop and start afresh—a phenomenon I have occasionally seen at Vesuvius, in fumaroles which are in direct communication with the lava. The intermittence, then, seems to be due to the surging up of the lava so as to block from time to time the lower inlet, or to be in other cases dependent upon the bursting of the great vapour bubbles as they rise in the viscous paste.

If this is really the termination of the eruption, we have gained some considerable advance in the interpretation of the eruptive phenomena of a highly acid magma, which is of such feeble character as to be incapable on the one hand of producing a typical pumice, and on the other of giving rise to an outflow of lava. As before stated, differences of opinion will probably be raised as to the nature of the *essential* ejectamenta, and I have little doubt that it will be dubbed as being more basic than it really is in consequence of the presence of impurities of olivine, augite, &c. It may be wise, therefore, that the reasons that lead me to conclude its acid nature should be given. First and foremost, we have the intense viscosity indicated by the long intervals of the explosions and the *bread-crust structure* in the ejectamenta. Secondly, these *bread-crust bombs* I have only met with in the ejectamenta accompanying either rhyolitic or trachytic glassy eruption, such as the obsidians of Rocche Rosse, Forgia Vecchia in Lipari, and Monte Rotaro in Ischia. In the former locality we have a beautiful illustration of the formation of these bombs outside the crater. Towards the end of the Rocche Rosse explosive stage, during which the great crater was drilled and the white pumice erupted, a large mass of obsidian was hurled up, and fell on the crater edge at Monte Pelato. In consequence of the sudden shock on reaching the ground, the semi-plastic mass cracked, and each fragment, relieved from the surrounding pressure, expanded into a small *bread-crust bomb*.

In the third place, the glass of these Vulcano bombs is exceedingly light and transparent, and indicates anything rather than an abundance of any basic iron silicate.

On looking back through the records of fairly well described eruptions, I cannot resist the impression that the duration of an eruption, other proportions being maintained, is in direct ratio to the basicity of the magma which in fact brings about such a result in consequence of the higher viscosity as the proportion of SiO_2 increases. Of course more or less advanced crystallization will also have an influence, as well as the relative higher or lower temperature, in eruptions of pure glass, beside the greater or less abundance of dissolved water.

The appearance of so many new fumaroles which we did not see six months ago all indicates that Vulcano tends (provided there are no more active signs) to pass into a solfataric stage such as is its usual state.

In fine, I must thank Mr. L. Sambon, for so kindly observing carefully the phenomena at Vulcano and transmitting to me his notes, and also Mr. J. P. Iddings for information on the same subject. H. J. JOHNSTON-LAVIS.

Naples, April 18.

Panmixia.

I AM glad to observe that his private correspondence has led Prof. Lankester to regard the doctrine of "panmixia," or "cessation of selection," in a much more favourable light than heretofore.

The form in which I stated this doctrine in 1874, and again in the present correspondence, is the form in which it has likewise been stated by Mr. Galton in 1875, by Prof. Weismann in several of his essays during the past decade, and by Mr. Poulton in his recent lectures. But, speaking for myself, I can see no objection to the form in which it is now presented by Prof. Lankester. For it seems to me immaterial whether we say that panmixia leads to a degeneration of size, shape, or structure, because the previously sustaining power of selection has been withdrawn; or whether we proceed to say that the reason why selection *has* a sustaining power is because, so long as it continues operative, its operation consists in eliminating variations below the standard of full efficiency. But although it appears to me that the latter point goes without saying, if its expression changes the whole aspect of the case in the view of Prof. Lankester, I can only regret that I did not express it in the first instance. I did not, however, understand that there was any question touching the fact of variations occurring below the standard of full efficiency, even as regards fully-developed organs of "well-established species." Therefore my argument was directed to show that, upon the "assumption" of such variability, under cessation of selection the standard will not rise *above* the previous "selection-mean," but always tend to fall *below* it, on account of reversion, &c.

Obviously, however, if we disallow that selection has any sustaining power, the doctrine of degeneration as due to its cessation becomes "absurd." Or, which is the same thing, if we "eliminate altogether" the "assumption" of congenital variations occurring below the standard of full efficiency (when once the parts in question have been completely developed by natural selection), and if we substitute a logically "*possible*" denial of such variations in respect of such parts by "assuming the ratio of birth-mean and selection-mean to be one of equality"—then, indeed, "the point of interest shifts." But surely the burden of proof lies on the side of anyone who denies this variability to fully-evolved organs. Even in the case of "well-established species" it *is* "improbable that there is identity between these two means"—or, in other words, that when once an organ has been fully *evolved* by natural selection, it no longer requires to be *maintained* by natural selection.

Again, "that some cases must occur in which the selection-mean-size is [actually] *smaller* than the birth-mean-size," appears to me true only of cases in which selection has been *reversed*—as, for instance, in flightless insects of oceanic islands. In such cases natural selection is actively engaged in pulling down its previous work. If natural selection be then withdrawn altogether, the adult-mean-size will probably increase. For not only will there now be no reversal of selection, but cessation of the *newer* selection will enable atavism in some measure to re-establish the state of matters which previously existed under the *older* selection. Such, at any rate, are the only cases in which I can imagine even the abstract "possibility" of the cessation of selection leading to an *increase* in size.

In short, the cessation of selection must always produce the opposite results to those which were produced by the selection which has ceased—unless, of course, there be any cases in which there is an "identity between the birth-mean and the selection-mean" (*i.e.* an absence of specific mutability). But even as regards such cases, if they are "assumed" to occur, the assumption amounts to a begging of the question by supposing that the selection has *already* ceased, and ceased when the parts had reached the point of their *maximum* development—an assumption which requires to deny any further mutability in respect of such parts, and therefore seems to me well-nigh incredible. Nevertheless, I fully allow that the more "well-established"—*i.e.* the *less variable*—a species, the smaller will be the necessity for the maintaining power of selection, and hence the smaller effect will result from its withdrawal. This, indeed, we see to be the case even in our domesticated animals—the "inflexible" goose, for instance, having suffered less change at the hands of panmixia than any of our other farm-yard animals.[1]

1 Nearly all our other domesticated animals yield abundant proof of the potency of panmixia (witness the care with which "methodical selection" is practised on the progeny of pedigree strains), and if we distrust the analogy between artificial and natural selection in this case, we seem to be rather aiming a blow at the principal evidence of the whole Darwinian theory. But as panmixia must act *more rapidly*, and *more completely*, in the case of such newly-acquired products of heredity than it is likely to act in wild species, I agree that experiments ought to be tried upon the latter. Moreover, I fully accept the distinction which Prof. Lankester has drawn in his letter of the 1st inst. between "size" and "structure." But I may remark that the effect of this distinction is not to indicate that panmixia will have no power to reduce size, while it is capable of entirely abolishing structure. What it does indicate is, that because there are greater potentialities of variation in the case of "complex" structures than in the case of mere "bulk," the sustaining power of natural selection is of correspondingly more importance: hence the cessation of selection will lead to the disintegration of structure *more rapidly* and *more completely* than it will to the reduction of bulk—as I have already pointed out elsewhere in relation to the eyeless peduncles of dark-cave Crustacea. Touching other minor points, I may further remark that while in his earlier letters Prof. Lankester accepted Darwin's view that parts are highly variable when selection is withdrawn, in his letter of May 1 he says it is "incontrovertible" that the "only effect" of such withdrawal must be to increase the number of individuals near the average mean—*i.e.* that panmixia both *permits* and *prevents* variability. Again, with regard to what he says about there being no proof that the economy of growth is absent in highly-fed domesticated animals, see Darwin, "Variation," &c., vol. ii. pp. 345 46, where the best imaginable "proof" of this fact is given. And with regard to his criticism on my use of the terms "essence" and "cause," see Mill, "Logic," vol. i. pp. 53 and 378 *et seq.*, where the popular *abuse* of both these terms is shown to be exactly that which I have avoided.

Upon the whole, however, we have ended by reaching a much more satisfactory state of agreement than seemed possible when we began. For Prof. Lankester now says he deems it "certain that some cases must sometimes occur in which the selection-mean is larger than the birth-mean," and that as regards such cases I have his "full concurrence in stating that the cessation of selection leads to dwindling." And as he previously agreed that cessation of selection leads also to a loss of shape and disintegration of structure, the only question that remains between us is as to whether there are any cases in which completely developed organs cease to present variations of size below the standard of full efficiency, and therefore will remain unaffected by the withdrawal of the selection by which they were evolved. But this is a question which does not vitally affect the *principle* of panmixia; and it only remains to add that I do fully "reciprocate" what he has said as to there being "no ill-feeling between us."

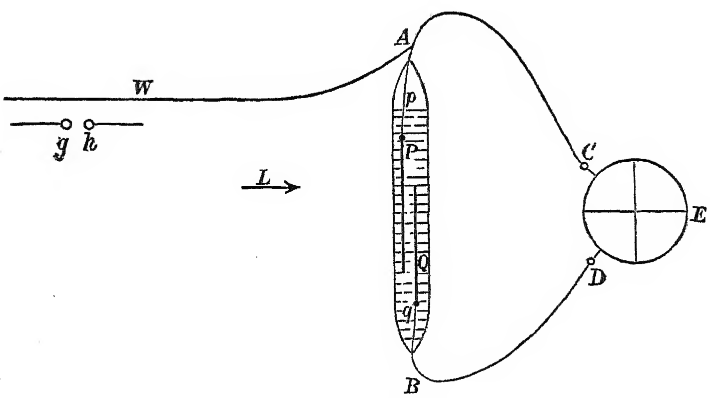
George J. Romanes.

Photo-electric Impulsion Cells.

Before publishing in detail the results of many experiments on the generation of electricity by the action of light falling on certain sensitive substances, I wish to make known a result which seems to be of a most remarkable character.

In this communication I shall give merely enough information to enable a reader to understand the special result which I desire now to make known.

The photo-electric cell which I employ consists of a small glass tube, represented in the figure, filled with an alcohol; two metallic plates, P and Q, are immersed in the liquid; each plate is connected with a platinum wire which may either be *soldered to the plate* or passed through a small hole in the plate and *pinched tightly to it*; these wires pass through the ends of the glass tube and are sealed into it. The poles of the cell are A, B, and these are connected with the poles of a quadrant electrometer (Clifton's form of Thomson's).



The plate P is sensitized by a peculiar process, the mere publication of the details of which would not enable a reader to make it successfully. The publication of the process is therefore reserved for a future occasion. The plate Q is quite clean—not sensitized to light. The cell is fixed vertically in a clamp (not represented in the figure). When the cell is of the "impulsion" kind, what happens is as follows. Daylight (represented by the arrow L) being allowed to fall on the sensitive plate P, the spot on the scale of the electrometer moves, and after a few seconds comes to rest, indicating an electromotive force varying with the intensity of the light, its amount for such diffused daylight as we have at present (May 10) at noon being between $\frac{1}{2}$ a volt and $\frac{3}{4}$ of a volt—which is, I submit, a surprisingly great magnitude. On the withdrawal of the light, the deflection falls, and there are means of rapidly getting rid of the deflection without injury to the cell. Either before or after this deflection caused by light ceases, let a slight tap (sometimes inaudible) be given to the base or clamp in which the cell rests, and then results a remarkable change in the cell. *It is no longer sensitive to light.* This insensitive state is indicated by a rapid return motion of the spot on the scale; it is merely *indicated* by this motion, there being no necessary connection between this motion and the insensitive state, for if the cell were now left for some time (perhaps an hour or so) in the dark, the disturbing E.M.F. of the cell would vanish, and there would be nothing to tell us that the cell remains insensitive; but that it is really still in the insensitive state we find at once on again exposing it to light. Another gentle tap given to the clamp, or the stone table on which the whole apparatus rests, will restore the sensitive state; and so on indefinitely, the sensitive and insensitive states following each other and being produced, in the case of many such cells, with great ease.

These results I found a long time ago, and they have been seen by or communicated to several scientific friends. From the first, I maintained that the results are due to an alteration of the molecular state of the sensitive surface, or of the layer of contact of this surface with the liquid, and that in one arrangement of the molecules the light energy can be taken up electrically, while it cannot be so taken up in the other. In my first experiments the plates were tightly pinched to the platinum wires—not soldered, as soldering endangered the sensitive layer—and the obvious objection was made that "loose contacts" were unsatisfactory. I have several results, however, which dispose of this objection even in the case of very loose contacts; but I may set the matter at rest by saying that I have been able to make soldered junctions, and with them to obtain the results.

I now come to the special point which is the occasion of this communication. A few days ago I was investigating the effect of static charges communicated to the plates on the sensitive and insensitive states, and in the course of these experiments I found that if a Voss machine, not in any way connected with the cell or the electrometer, was worked in the room while the cell was in the insensitive state, *the moment a spark passed between the poles of the Voss, the insensitive state was altered to the sensitive*, whether the cell was connected with the electrometer or not. Finally, I found that the best method of showing the inductive effect of the spark is to connect an insulated wire, W, apparently of any length, to either pole (A in the figure) of the cell, and to place the poles, *g*, *h*, of the Voss near the wire (a distance of several feet will do with a spark about half an inch long). If *g* and *h* are two or three feet from any part of the wire W, a spark about one-eighth of an inch long suffices to change the cell from the insensitive to the sensitive state.

The effect is not one on the electrometer, nor is it due to sound, and I have repeated the results with several cells many scores of times before people interested in them. At present I am endeavouring to produce by *electro-magnetic induction* the reverse change, viz. that from the sensitive to the insensitive state; but, although such must apparently be possible, I have not yet succeeded.

The sudden alteration of the insensitive to the sensitive state is produced in a most marked manner by the spark of a Hertz oscillator at as great a distance as the laboratory room in which I work allows. This distance is usually only about eight or ten feet, but I observed the change effected occasionally when the oscillator was at a distance of some thirty feet or more. In this latter case, however, the action was interfered with by the unavoidable presence of wires along the walls, &c., intervening between the Hertz and my impulsion cell.

If the cause to which I have assigned the change from the photo-electrically insensitive to the photo-electrically sensitive state of the cell is the true one, it is impossible to avoid the speculation that impulsion results of this kind may be very common in the economy of Nature; and that the mode in which solar energy is taken up by plants may be affected, and even altered in kind, by sudden electro-magnetic disturbances. The effect of a Hertz oscillation is, indeed, not confined to an alteration of a plate from the insensitive to the sensitive state; for I have cells in which if the sensitive plate is, on exposure to light, electrically negative to the back plate, a Hertz oscillator at a distance will reverse the relation when the plate is again exposed to light.

George M. Minchin.

Royal Indian Engineering College, Cooper's Hill, May 10.

P.S.—While the above communication was going through the press, I made an experiment which renders it almost certain that in the impulsion cells the results are due to the formation of some

oscillating layer at the surface of the sensitive plate. Being anxious to keep the alcohol in the cell (which in this instance was closed by a ground glass cap), I sealed the cell into a glass tube through the extremities of which the wires of the cell passed. The effect of the disturbance thus resulting was that no amount of tapping the support of the cell would change it from the sensitive to the insensitive state, although before being thus treated it was sensitive to the most minute disturbance. I suspected, however, that after some hours the liquid and the plate would again enter into the peculiar relation on which the impulsion results depend, and so it turned out—after three hours the cell could be rendered insensitive by taps and sensitive by the inductive effect of a Voss machine. The platinum wires were *soldered* to the plates. I see that the distances at which I found the Hertz oscillator effective in influencing the cells were greater than those above stated; but I have not been able to renew work with the oscillator, which belongs to Mr. Gregory, who removed it for exhibition at the Royal Society's meeting.

May 16.

Bison not Aurochs.

I AM glad that Mr. Lydekker accedes (NATURE, May 15, p. 53) to the correction of which I had pointed out the need. But the "vulgar error"—if the Editor will allow me to use a phrase made classical nearly 250 years ago by Sir Thomas Browne—is of more ancient date than my friend seems to suppose; and Dr. Gadow has kindly referred me to Prof. Wrześniowski's "Studien zur Geschichte des polnischen Tur," published in May 1878 (*Zeitschr. für wissenschaftl. Zoologie*, xxx. pp. 493-555). Therein will be seen reduced copies of the engravings in an edition of Herberstein's "Rerum Moscoviticarum Commentarii" (Basileæ: 1571), giving a figure of each of the animals. The first is inscribed

VRVS SVM, POLONIS TVR, GERMANIS AVROX: IGNARI BISONTIS NOMEN DEDERANT.

Over the second may be read

BISONS SVM, POLONIS SVBER, GERMANIS BISONT: IGNARI VRI NOMEN DEDERANT.

This paper is well worth reading from the amount of curious information to be found in it. I have been able to consult only one copy of this work, of an earlier edition indeed, for it was published at Antwerp in 1557; but it does not contain these figures, though the passages quoted by the Polish Professor of course occur (*ff.* 117 *verso et seqq.*). The figures are not remarkable for beauty, and if anyone were to call them caricatures I should hardly complain; but they are certainly of interest, and that of the Urus, which I think I have seen copied elsewhere, is perhaps the only approach to an original representation extant. If so it deserves to be better known. Allow me to remark that this is not the first time that I have noticed this error. I did so many years ago in a little pamphlet "On the Zoology of Ancient Europe" (p. 14), published by Messrs. Macmillan in 1862; and I may add that any visitor to the Museum of Zoology of this University may see therein a skeleton of the Aurochs and of the Bison, as well as of the American "Buffalo"—all standing side by side. ALFRED NEWTON.

Magdalene College, Cambridge, May 18.

Sudden Rises of Temperature.

IN NATURE, vol. xli. p. 550, it is stated that sudden rises of temperature of large amount in Great Britain "are more frequent and more extensive in amount than sudden falls—the reverse to what obtains in India." There appears to be a somewhat similar condition of affairs in North America. Extremely sudden and large rises of temperature attend the warm Chinook winds, as they are called, which occur over the western part of the continent, but are unknown further east. Equally pronounced are the sudden falls of temperature in the eastern half of the country popularly termed "cold waves." M. A. VEEDER.

Lyons, N.Y., May 7.

Coral Reefs, Fossil and Recent.

IN Dr. von Lendenfeld's communication to NATURE of May 8 (p. 30), occurs the following:—

"Dr. Murray goes on to say . . . and an isolated atoll rising precipitously, perhaps 10,000 feet from the sea-bottom, will be formed." And again—"and far less will it enable an atoll rising 10,000 feet or more from the bottom of the sea . . ."

I cannot think that the author quoted has committed himself to any such figures as these, but if either he or Dr. von Lendenfeld can tell me where to find such a formation in existing seas, I shall be obliged; as I have sought in vain for instances yet known of any slopes that could be called "*steep*" descending to more than 4000 feet or so, while *precipitous* slopes are unknown to me beyond 1200 feet; and these are, so far as I know, very exceptional.

While I am writing on this subject, I should be glad if anyone would explain how, on the assumption that atolls are formed during subsidence, it comes about that, while the outer slopes descend to great depths, the depth of the largest lagoons inclosed is generally confined to about 45 fathoms, and in one or two cases to 60 fathoms, but is never more. Why should not the lagoon of an atoll twenty or thirty miles in diameter, which rises steeply from depths of 200 or 300 fathoms or more, have a depth of at any rate 100 fathoms, allowing for the most extravagant amount of silt from the *débris* of the rim.

W. J. L. WHARTON.

Doppler's Principle.

A COMPLETE solution of the questions about which your correspondents are puzzling themselves has been before the public for some ten years in several successive editions of my "Deschanel." It occurs in the last paragraph of the chapter entitled "Numerical Evaluation of Sound," and is as follows:—

"Let the source make n vibrations per second. Let the observer move towards the source with velocity a. Let the source move away from the observer with velocity a'. Let the medium move from the observer towards the source with velocity m, and let the velocity of sound in the medium be v.

"Then the velocity of the observer relative to the medium is $a - m$ towards the source, and the velocity of the source relative to the medium is $a' - m$ away from the observer. The velocity of the sound relative to the source will be different in different directions, its greatest amount being $v + a' - m$ towards the observer, and its least being $v - a' + m$ away from the observer. The length of a wave will vary with direction, being $\frac{1}{n}$ of the velocity of the sound relative to the source. The length of those waves which meet the observer will be $\frac{v + a' - m}{n}$; and the velocity of these waves relative to the observer will be $v + a - m$; hence the number of waves that meet him in a second will be $\frac{v + a - m}{v + a' - m} n$."

The three quantities a, a', m may of course be either positive or negative. J. D. EVERETT.

5 Princess Gardens, Belfast, May 17.

THE SHAPES OF LEAVES AND COTYLEDONS.[1]

ATTEMPTS to explain the forms, colours, and other characteristics of animals and plants, though not new, were until recent years far from successful. Our Teutonic forefathers had a pretty story which explained certain characteristics of several common plants.

Balder, the God of Mirth and Merriment, was, characteristically enough, regarded as deficient in the possession of immortality. The other divinities, fearing to lose him, petitioned Thor to make him immortal, and the prayer was granted on condition that every animal and plant would swear not to injure him. To secure this object, Nanna, Balder's wife, descended upon the earth. Loki, the God of Envy, attended her disguised as a crow (crows at that time were white), and settled on a little blue flower, hoping to cover it up so that she might overlook it. The flower, however, cried out "Forget-me-not, forget-me-not" (and has ever since been known under that name). Loki then flew up into an oak and sat on a mistletoe. Here he was more successful. Nanna carried off the

[1] Lecture delivered at the Royal Institution on April 25, by Sir John Lubbock, Bart., M.P., D.C.L., F.R.S., &c.

oath of the oak, but overlooked the mistletoe. She thought, however, and the divinities thought, that she had successfully accomplished her mission, and that Balder had received the gift of immortality.

One day, thinking Balder proof, they amused themselves by shooting at him, posting him against a holly. Loki tipped an arrow with a piece of mistletoe, against which Balder was not proof. This unfortunately pierced him to the heart, and he fell dead. Some drops of his blood dropped on the holly, which accounts for the redness of the berries; the mistletoe was so grieved that she has ever since borne fruit like tears, and the crow, whose form Loki had taken, and which till then had been white, was turned black.

This pretty myth accounts for several things, but is open to fatal objections. You will judge whether I am more fortunate. In the first place I need hardly observe that the forms of leaves are almost infinitely varied. To quote Ruskin's vivid words, they "take all kinds of strange shapes, as if to invite us to examine them. Star-shaped, heart-shaped, spear-shaped, arrow-shaped, fretted, fringed, cleft, furrowed, serrated, sinuated, in whorls, in tufts, in spires, in wreaths, endlessly expressive, deceptive, fantastic, never the same from footstalk to blossom, they seem perpetually to tempt our watchfulness, and take delight in outstripping our wonder."

Now, why is this marvellous variety, this inexhaustible treasury of beautiful forms? Does it result from some innate tendency of each species? Is it intentionally designed to delight the eye of man? Or has the form and size and texture some reference to the structure and organization, the habits and requirements, of the whole plant?

The leaf, although so thin, is no mere membrane, but is built up of many layers of cells, and the interior communicates with the external air by millions of little mouths, called stomata, which are generally situated on the under side of the leaf. The structure of leaves varies as much as their forms.

It is, of course, principally in hot and dry countries that leaves require protection from too much evaporation.

The surface is in some cases protected by a covering of varnish, in others by saline or calcareous excretions. In others, again, the same object is attained by increased viscidity of the sap; in some, the leaves assume a vertical position, thus presenting a smaller surface to the rays of the sun. In other cases the leaves become fleshy. Woolly hairs are also a common and effective mode of protection. The plants of deserts are very frequently covered with a thick felt of hair. Some species, again, which are smooth in the north tend to become woolly in the south. Species of the cool spring again tend to be glabrous. The uses of hairs to plants are indeed very various. They serve, as just mentioned, to check too rapid evaporation. They form a protection for the stomata or breathing holes, and consequently, as these are mainly on the under side of leaves, we find that when one side of the leaf is covered with white felted hairs, as the white poplar, this is always the under size.

In other cases the use of hair is to throw off water. In some Alpine and marsh plants this is important. If the breathing holes became clogged with moisture—with fog, for instance, or dew—they would be unable to fulfil their functions. The covering of hair, however, throws off the moisture, and thus keeps them dry. Thus these hairs form a protection both against too much drought, and too much moisture.

Another function of hairs which cannot be omitted is to serve as shades against too brilliant light, and too much heat. Again, hairs serve as a protection against insects, and even against larger animals. The stinging hairs of the common nettle are a familiar example, and coarse woolly hairs are often distasteful to herbivorous quadrupeds.

Deciduous leaves especially characterize the comparatively cool and moist atmosphere of temperate regions. For different reasons evergreen leaves become more numerous in the Alps and in the tropics.

In the Alps it is necessary for plants to make the most of the short summer. Hence, perennial and evergreen species are more numerous in proportion than with us. Everybody must have noticed how our trees are broken if we have snow early in the season and when they are still in leaf.

The comparatively tough and leathery leaves, such as those of the evergreen oak and olive, are protected against animals by their texture, and often, as in the holly, by spines; they are better able to resist the heat and dryness of the south than the comparatively tender leaves of our deciduous trees, which would part too rapidly with their moisture. It is perhaps an advantage to evergreen leaves to be glossy, because it enables them better to throw off snow. Moreover, their stomata are often placed in pits, and protected with hair, which prevents too rapid evaporation. The texture and structure of leaves is indeed a wide and very interesting subject, but to-night I must confine myself to the shape.

It is impossible to classify plants by the form of the leaf, which often differs greatly in very nearly allied species. Thus the common plantain of our lawn (*Plantago major*) has broad leaves, *P. lanceolata* narrow ones. The width or narrowness of leaves depends on various considerations. In herbaceous and stalkless plants, such as the plantain, prostrate leaves tend to be broad, those which are upright to be narrow. Thus, grasses, for instance, have more or less upright narrow leaves.

In other cases the width is determined by the distance between the buds, and in others again by the number of leaves in a whorl.

Cordate and Lobed Leaves.

Among broad leaves we may observe two distinct types, according as they are oval or palmate. Monocotyledonous plants, such as grasses, sedges, lilies, hyacinths, very generally have upright and narrow leaves. When they are wider, as, for instance, in the black bryony, this is mainly at the base, where, consequently, the veins are further apart, coming together again towards the apex. This we are tempted therefore to regard as the primitive type of a broad leaf.

There is, however, a totally different one, where the leaf is palmate, like a hand, widening towards the free end. Here the veins pursue a straight, diverging course; and as they not only serve to strengthen the leaf, but also to carry the nourishment, this is doubtless an advantage. Another reason perhaps for this arrangement is found in the fact that these leaves are generally folded up, like a fan, while they are in the bud.

I have elsewhere dwelt on the case of the beech, and perhaps I may briefly refer to it again. The weight of leaves which a branch can carry will of course depend on its position and strength. The mode of growth of the beech and the hornbeam are very similar, but the twigs of the latter are slenderer, and the leaves smaller. If we cut off a beech branch below the sixth leaf we shall find that the superficial leaf area which it carries is about 18 square inches. But in our climate most leaves are glad of as much sunshine as they can secure, and are arranged with reference to it. The width of the beech leaves, about $1\frac{3}{4}$ inch, is regulated by the average distance between the buds. If the leaves were wider they would overlap. If they were narrower there would be a waste of space. The area on the one hand, and the width on the other, being thus determined, the length is fixed, because, to secure an area of 18 inches, the width being about $1\frac{3}{4}$ inch, the length must be about 2 inches. This, then, explains the form of the beech leaf.

Let us apply these considerations in other cases. I

will take, for instance, the Spanish chestnut and the black poplar. In the Spanish chestnut the stem is much stronger than that of the beech. Consequently it can carry a greater leaf-surface. But the distance between the buds being about the same the leaves cannot be much wider; hence they are much longer in proportion, and this gives them their peculiar sword-blade-like shape.

Now, if we look at the end of a branch of black poplar and compare it with one of white poplar, we are struck with two things: in the first place, the branch cannot be laid out on a sheet of paper so that the leaves shall not overlap; the leaves are too numerous and large. Secondly, in the white poplar the upper and under surfaces of the leaf are very different, the lower one being covered with a thick felt of hair, which gives it its white colour; in the black poplar, on the other hand, the two surfaces are nearly similar.

These two characteristics are correlated, for while in the white poplar the leaves are horizontal, in the black poplar, on the contrary, they hang vertically. Hence the two surfaces are under very similar conditions, and consequently present a similar structure; while for the same reason they hang free from one another.

Let us again look for a moment at the great group of Conifers. Why, for instance, do some have long leaves and some short ones? This, I believe, depends on the strength of the twigs and the number of years which the leaves last; long leaves dropping after one, two, or three years, while species with shorter ones retained them many years—the spruce fir, for instance, 8 or 10, *Abies Pinsapo* even as many as 18.

[Here Sir John dwelt on and explained the forms of several familiar leaves.]

Seedlings.

I now come to the second part of my lecture—the forms of cotyledons. Anyone who has ever looked at a seedling plant must have been struck by the fact that the first leaves differ entirely from those which follow—not merely from the final form, but even from those which immediately follow. These first leaves are called cotyledons. The forms of many cotyledons have been carefully described, but no reason had been given for the forms assumed, nor any explanation offered why they should differ so much from the subsequent leaves. Klebs, indeed, in his interesting memoir on "Germination," characterizes it as quite an enigma.

Mustard and cress were the delight and wonder of our childhood, but it never then occurred to me at least to ask why they were formed as they are. So they grew, and beyond that it did not occur to me, nor I think to most, that it was possible to inquire. I have, however, I think, suggested plausible reasons in many cases, some of which I will now submit for your consideration. Cotyledons differ greatly in form.

Some are narrow, in illustration of which I may mention the fennel and ferula, in the stalk or ferule of which Prometheus is fabled to have brought down fire from heaven.

Some are broad, as in the beech and mustard. Moreover, some species have narrow cotyledons and broad leaves, while others have broad cotyledons and narrow leaves.

Some are emarginate, as in the mustard; lobed, as in the lime; bifid, as in Eschscholtzia; trifid, as in the cress; or with four long lobes, as in Pterocarya.

Some are unequal, as in the mustard; or unsymmetrical, as in the geranium.

Some are sessile, and some are stalked; some are large, some small.

Generally, they are green, leaf-like, and aërial, but sometimes they are thick and fleshy, as in the oak, nut, walnut, peas, beans, and many others, in which they never quit the seed at all.

Let us see, then, whether we can throw any light on these differences, and why they should be so unlike the true leaves.

If we cut open a seed, we find within it the future plant: sometimes, as in the larkspur, a very small oval body; sometimes, as in the ash, or the castor-oil, a lovely little miniature plant, with a short stout root and two well-formed leaves, inclosing between them the rudiment of the future stem; the whole lying embedded in food-material or perisperm; while sometimes the embryo occupies the whole interior of the seed, the food-material being stored up, not round, but in the seed-leaves or cotyledons themselves. Peas and beans, almonds, nuts, and walnuts are familiar cases In split peas, for instance,—who split the peas? If you look at them you will see that it is too regularly and beautifully done for human hands. In fact, the two halves are the two fleshy cotyledons: strictly speaking, they are not split, for they never were united.

Narrow Cotyledons.

Let us now begin with such species as have narrow cotyledons, and see if we can throw any light on this characteristic. The problem is simple enough in such cases as the plane, where we have, on the one hand, narrow cotyledons, and, on the other hand, a long narrow seed fully occupied by a straight embryo. Again, in the ash, the cotyledons lie parallel to the longer axis of the seed, which is narrow and elongated. Such cases are, however, comparatively few; and there are a large number of species in which the seeds are broad and even orbicular, while yet the cotyledons are narrow.

In these it will generally be found that the cotyledons lie transversely to the seed.

The sycamore has also narrow cotyledons, but the arrangement is very different. The fruit is winged, the seed somewhat obovoid and aperispermic—that is to say, the embryo, instead of lying embedded in food-material, occupies the whole cavity of the seed. Now, if we wished to pack a leaf into a cavity of this form, it would be found convenient to choose one of a long strap-like shape, and then roll it up into a sort of ball. This is, I believe, the reason why this form of cotyledon is most suitable in the case of the sycamore.

Broad Cotyledons.

I now pass to species with broad cotyledons. In the castor-oil plant, Euonymus, or the apple, for instance, the young plant lies the broad way of the seed, and the cotyledons conform to it. In the genus Coreopsis, *Coreopsis auriculata* has broad cotyledons, and *Coreopsis filifolia* has narrow ones—the first having broad, the second narrow seeds.

In a great many species the cotyledons are emarginate—that is to say, they are more or less deeply notched at the end. This is due to a variety of causes. One of the simplest cases is that of the oak, where the two fleshy cotyledons fill the seed; and as the walls of the seed are somewhat thickened at the end, and project slightly into the hollow of the seed, this causes a corresponding depression in the cotyledons.

In such cases as the mustard, cabbage, and radish, the emargination is due to a very different cause. The seed is oblong, thick, and slightly narrower at one end than the other. There is no perisperm, so that the embryo occupies the whole seed, and as this is somewhat deep, the cotyledons, in order to occupy the whole space, are folded and arranged one over the other like two sheets of note-paper, the radicle being folded along the edge. To this folding the emargination is due. If a piece of paper be taken, folded on itself, cut into the form of the seed, and then unfolded, the reason for the form of the cotyledon becomes clear at once.

But it may be said that in the wallflower the seed has a

similar outline, and yet the cotyledons are not emarginate. The reason of this is that in the wall-flower, Cheiranthus, the seed is more compressed than in the mustard and radish, and consequently the cotyledons are not folded; so that the whole, not the half, of each cotyledon corresponds to the form of the seed.

Lobed Cotyledons.

The great majority of cotyledons are entire, but some are more or less lobed. For instance, those of the mallow are broadly ovate, minutely emarginate, cordate at the base, and three-lobed or angled towards the apex, with three veins, each running into one of the lobes.

The embryo is green, curved, and occupies a great part of the seed. The cotyledons are applied face to face; then, as growth continues, the tip becomes curved and depressed into a median longitudinal furrow, the fold of the one lying in that of the other.

[Sir John then showed clearly by diagrams and paper how the emargination arises, but it cannot be made clear without illustrations.]

The cotyledons of the lime are very peculiar. They are deeply five-lobed, the central lobe being the longest; so that they are roughly shaped like a hand. The seed is an oblate spheroid, resembling an orange in form, and the embryo is embedded in semi-transparent albumen.

The embryo is at first straight; the radicle is stout and obtuse; the cotyledons ovate-obtuse, plano-convex, fleshy, pale green, and applied face to face. They grow, however, considerably, and when they meet the wall of the seed, they bend back on themselves, and then curve round, following the general outline of the seed. If anyone will take a common tea-cup and try to place in it a sheet of paper, the paper will, of course, be thrown into ridges. If these ridges be removed and so much left as will lie smoothly inside the cup, it will be found that the paper has been cut into lobes more or less resembling those of the cotyledons of Tilia. Or if, conversely, a piece of paper be cut into lobes resembling those of the cotyledons, it will be found that the paper will fit the concavity of the cup. The case is almost like that of our own hand, which can be opened and closed conveniently owing to the division of the five fingers.

Unequal Cotyledons.

In most cases the two cotyledons are equal, but there are several cases in which one of them is larger than the other. They had not escaped the attention of Darwin, who attributed the difference to the fact "of a store of nutriment being laid up in some other part, as in the hypocotyl, or one of the cotyledons." I confess that I do not quite see how this affords any explanation of the fact. The suggestion I have thrown out is that the difference is due to the relative position of the two cotyledons in the seed, which in some cases favours one of them at the expense of the other. Thus in the mustard they are unequal, and, as we have already seen, they are folded up, one inside the other. The outer one, therefore, has more space, and becomes larger. In many other Crucifers, though the cotyledons are not folded, they are what is called "incumbent"—that is to say, they are folded on the radicle, and the outer one has therefore more room than the other.

Unsymmetrical Cotyledons.

In other cases, as in the geraniums, laburnum, lupines, &c., there is inequality, not between the two cotyledons, but between the two halves of each cotyledon. In the geraniums this is due to the manner in which the cotyledons are folded. In cabbage and mustard we have seen that one cotyledon is folded inside the other; in the geranium they are convolute, one half of each being folded inside one half of the other, the two inner halves being the smaller, the two outer the larger ones.

In the laburnum, where the arrangement is very similar, the inequality in the two sides of the cotyledon is due to the inequality between the two sides of the seed.

Subterranean Cotyledons.

I have already observed that in some cases the cotyledons occupy the whole of the seed, which, in more or less spherical seeds is effected either by a process of folding and packing, or by the cotyledons becoming themselves more or less thickened, as in peas and beans, nuts and chestnuts. This is the reason why such seeds fall more or less readily into two halves, the radicle or plumule being so small in comparison as generally to escape notice, though, if a horse-chestnut is peeled, the radicle appears as a sort of tail.

In some beans the cotyledons sometimes emerge from the seed, sometimes remain underground. In others, as also in the oak and horse-chestnut, they never leave the seed, or come above ground: they have lost the function of leaves and become mere receptacles of nourishment.

Did it ever occur to you to think, when you have been eating walnuts, why their structure is so complex, and why the edible part is thrown into those complicated lobes and folds? The history is very interesting.

In the walnut, the cotyledons now never leave the seed, but in an allied genus, Pterocarya, they come above ground as usual, and are very peculiar in form, being deeply four-lobed. The reason of this is very curious. The fruit is originally much larger than the seed, but, as it approaches maturity, the hard woody tissue disintegrates at four places, leaving thus four hollow spaces. Into these spaces the seed sends four projections, and into these four projections each cotyledon sends a lobe. Hence the four lobes.

Now in the walnut a very similar process takes place, only the hollow spaces are much larger, so that, instead of a solid wall, with hollow spaces occupied by the seed, it gives the impression as if the seed was thrown into folds occupied by the wall of the fruit. To occupy these spaces fully, the cotyledons themselves were thrown into folds as we now see them. The fruit of Pterocarya is much smaller than that of the horse-chestnut, which doubtless was itself formerly not so large as it now is. As it increased, the cotyledons became fleshier and fleshier, and found it more and more difficult to make their exit from the seed, until at last they have given up any attempt to do so. Hence these curious folds, with which we are so familiar, are the efforts made by the originally leafy cotyledons to occupy the interior of the nut. If you separate them, you will easily find the little rootlet, and the plumule with from five to seven pairs of minute leaves.

But perhaps you will ask me why I have assumed that in these cases the cotyledons have conformed to the seeds? May it not be that the seed is determined, on the contrary, with reference to the cotyledons? The size, form, &c., of the seeds, however, evidently have relation to the habits, conditions, &c., of the parent plant.

Let me, in illustration, take one case. The cotyledons of the sycamore are long, narrow, and strap-like; those of the beech are short, very broad, and fan-like. Both species are aperispermic, the embryo occupying the whole interior of the seed.

Now, in the sycamore, the seed is more or less an oblate spheroid, and the long ribbon-like cotyledons, being rolled up into a ball, fit it closely, the inner cotyledon being often somewhat shorter than the other. On the other hand, the nuts of the beech are more or less triangular: an arrangement like that of the sycamore would therefore be utterly unsuitable, as it would necessarily leave great gaps. The cotyledons, however, are folded up like a fan, but with more complication, and in such a manner that they fit beautifully into the triangular nut.

Can we, however, carry the argument one stage further? Why should the seed of the sycamore be globular, and that of the beech triangular? Is it clear that the cotyledons are constituted so as to suit the seed? May it not be that it is the seed which is adapted to the cotyledons? In answer to this we must examine the fruit, and we shall find that in both cases the cavity of the fruit is approximately spherical. That of the sycamore, however, is comparatively small, say $\frac{1}{5}$ inch in diameter, and contains one seed, which exactly conforms to the cavity in which it lies. In the beech, on the contrary, the fruit is at least twice the size, and contains from two to four fruits, which consequently, in order to occupy the space, are compelled (to give a familiar illustration, like the segments of an orange) to take a more or less triangular form.

Thus, then, in these cases, starting with the form of the fruit, we see that it governs that of the seed, and that of the seed, again, determines that of the cotyledons. But though the cotyledons often follow the form of the seed, this is not invariably the case: other factors must also be taken into consideration; but when this is done, we can, I venture to think, throw much light on the varied forms which seedlings assume.

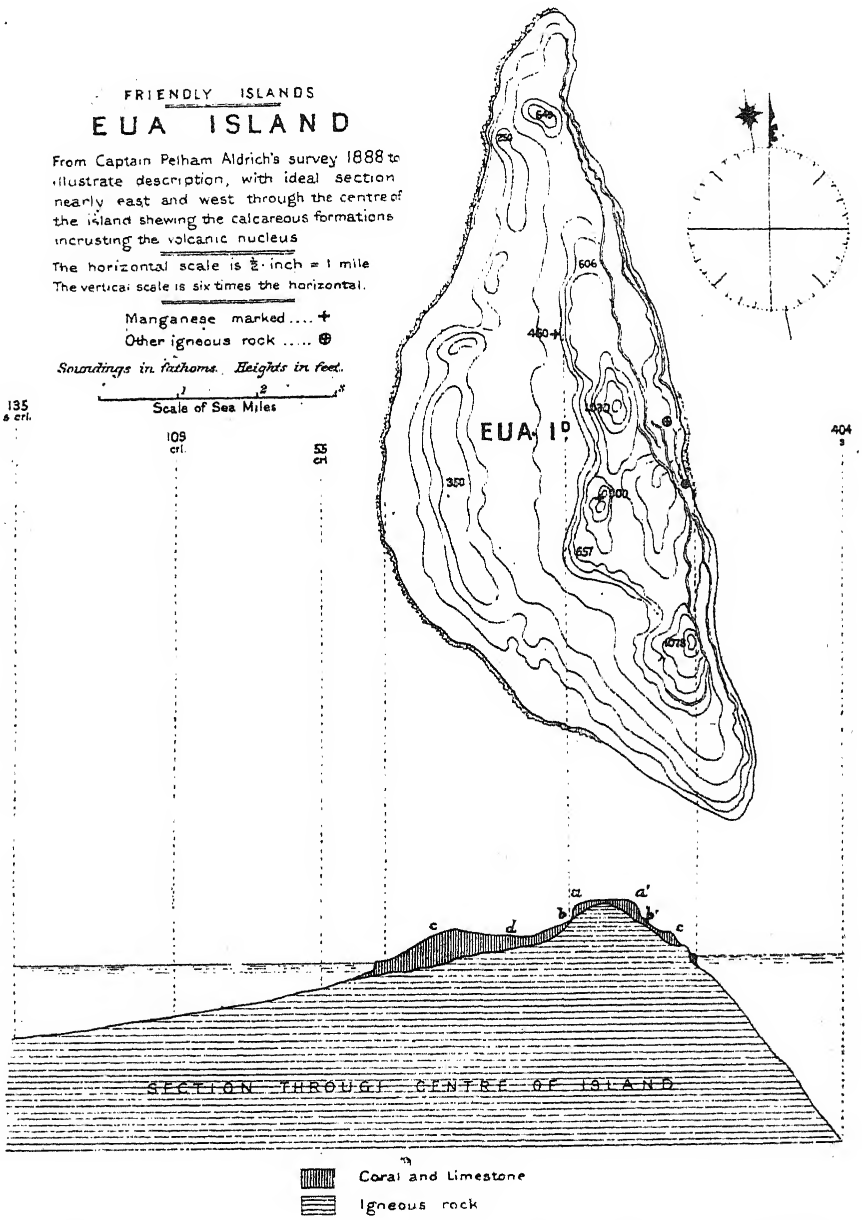
I have thus attempted to indicate some of the principles on which, as it seems to me, the shapes of leaves and seedlings depend, and to apply them in certain cases, but the study is only in its infancy: the number and variety of leaves is almost infinite, and the whole question offers, I venture to think, a very interesting field for observation and research—one, indeed, of the most fascinating in the whole of natural history.

EUA ISLAND, TONGA GROUP.

THE following description of Eua Island (one of the higher members of the Friendly or Tonga Islands, and familiar to readers of "Cook's Voyages" as Middleburgh), written by Commander Oldham, of H.M. surveying-ship *Egeria*, will be of interest to geologists and those interested in the coral controversy.

W. J. L. WHARTON.

"When viewed from the westward, Eua is seen to consist of grassy table-lands and slopes, having clumps of dark-green trees dotted here and there, giving it a park-like appearance. It is formed of two coral terraces resting on a volcanic nucleus. The upper terrace, about 600 feet above the level of the sea, rises to a summit 1030 feet high; the lower terrace attains a height of only 350 feet. On the western side of the island these terraces are separated a distance of from one to one and a half miles, and in some places there is a depression between them; the eastern side is very precipitous, the terraces there being very narrow, and forming cliffs at their seaward edges.



"The upper terrace seems composed of foraminiferous limestone and reef rock, with volcanic rock (a hydrated oxide of manganese) cropping out on the western side at the edge of the terrace where the coral rock has been removed by the effects of weather. The limestone is compact, reddish-brown, and largely foraminiferous. It is both *in situ* and scattered in detached blocks over the upper terrace, and when weathered has a honeycombed appearance.

"Low coral cliffs, which in many places form the shore-line, give indications of recent elevation, being marked by two distinct lines of erosion. On the beach, on the eastern side of the island, I observed that the sea had washed up recently high above the ordinary high-water mark; trunks of cocoa-nuts were lying about rotting, and the lower part of the stems of those still standing near the shore had been washed by salt water. This was afterwards explained by the fact that a very high tide occurred on March 18 (about the time of the hurricane at Samoa), which rose 2 feet above high-water mark, and remained so for fifteen minutes. A narrow fringing reef generally borders the island. On the eastern side of the island, below the summit, at an elevation of 300 feet, volcanic stones were observed lying in the bed of a stream, and on the beach, a little further south, are dykes of volcanic rock; one of diorite shows through the beach, another, which is close-to, is about 100 feet high, and can be traced a short distance inland. The inner part has *coral-reef rock*, *conformable*, *superimposed*. The beach for a considerable distance either side is strewn with blocks of conglomerate formed of coral and volcanic rocks cemented together.

"The higher part of the lower terrace on the western side of the island was found to be composed of coral-reef rock.

"The present volcanic nucleus must have been originally below the surface, but sufficiently near to allow coral to grow, and reef-making Foraminifera to be deposited on its summit. It was then evidently elevated about 300 feet (marked *aa'*, *bb'* in section) in a comparatively short interval, after which a long period of rest, or subsidence, followed, during which coral grew (forming the portion *bb'* to *cc'*), and a lagoon (*d*) was produced. Then another period of elevation raised the island to its present height, and exposed the volcanic foundation.

"C. F. OLDHAM."

I am indebted to Captain Wharton for sending me the specimens collected by Commander Oldham. The limestones, some of which are fairly crystalline in character, are composed of Foraminifera and water-worn fragments of calcareous Algæ ("Nullipores"). The deposit of hydrated manganese oxide cropping out from below the limestone is remarkable, but it has all the appearance of an ordinary terrestrial deposit. A much-weathered mass from the neighbourhood contains many beautiful crystals of magnetite. Although the rocks forming the nucleus of the island are of igneous origin, they are not modern volcanic materials. They consist of much altered glassy andesites (porphyrites) with epidiorites; and are suggestive of ancient volcanic masses that have been exposed at the surface by denudation. The significance of such facts as these has been pointed out by Prof. Bonney and Dr. Blanford; and it is quite incorrect to quote examples like this as lending support to the view that all oceanic islands are of volcanic origin. JOHN W. JUDD.

NOTES.

IN the list of Birthday honours the merits of many different classes of public servants are duly recognized. The services of men of science in the Science and Art Department, however, following an unbroken rule, fail to receive any acknowledgment.

AMONG those who have received the honour of C.B., we are glad to see the name of Prof. W. C. Roberts-Austen, F.R.S., Assayer to the Royal Mint.

THE date of the second *soirée* of the Royal Society, to which ladies are invited, is fixed for June 18.

THE Jubilee of the Uniform Penny Post was well and worthily celebrated at the Guildhall on Friday last, the 16th inst., by the Corporation of the City of London. The grand old hall was never applied to a better purpose. Every process connected with Post Office work, from telegraphy to sorting, was shown in actual operation. The Exhibition remained open for three days, and 25,000 people were delighted.

ON May 15 an influential deputation from the Marine Biological Association of the United Kingdom waited upon the Chancellor of the Exchequer to ask for an additional grant from the Treasury in aid of investigations in connection with food-fishes, crustacea, and mollusks, carried on by the Association. Mr. Joseph Chamberlain, M.P., introduced the deputation. Both he and Sir E. Birkbeck, M.P., called Mr. Goschen's attention to the large amount spent by the United States in the encouragement of the fishing industry; and Sir E. Birkbeck pointed out that even Scotland, with her grants to the Scotch Fishery Board, is in this respect ahead of England. After some remarks from other members of the deputation, the Chancellor of the Exchequer, in reply, said that the questions which had been put by him in the course of the speeches were not made from a critical or carping point of view, but merely to convince himself as to what were really the aims of the Association, which he regarded as excellent; but he could not say anything as to the practicability of their being carried out. He pointed out that the Treasury would very carefully consider the whole question of how to recast the Fisheries Department, but he felt that it would be an inconvenient thing to have four bodies, two in England and one each in Scotland and Ireland, whose jurisdiction might overlap. He hoped to be able in a short time to have an opportunity of consulting some of the scientific and other gentlemen present, in order to have further light thrown upon the subject before the Government took any action in the matter.

AT the Royal Institution, on Tuesday afternoon, May 27, Mr. Andrew Lang will begin a course of three lectures on "The Natural History of Society." The remaining lectures will be given on June 3 and 10.

MR. G. BERTIN is about to deliver, at the British Museum, a series of four lectures on the manners and customs of the Babylonians, from the cuneiform documents in the Museum. The lectures will be given on the following Tuesdays—June 3, 10, 17, and 24, at 3.30 p.m.

ON Monday evening Mr. T. W. Russell asked in the House of Commons whether the Committee consisting of certain members of the Royal Society appointed to inquire into the question of lighthouse illuminants had yet reported. Sir M. Hicks-Beach replied that there had been some unavoidable delay in the matter in consequence of a change made in the composition of the Committee. But he had communicated with the President of the Royal Society, and understood that the Report of the Committee might be expected in the course of the summer.

MR. F. H. SNOW, of Lawrence, Kansas, calls attention in *Science* to a remarkable fall of meteorites of unknown date in Kiowa County, Kansas. "Many of the citizens of Greensburgh, the county seat, were," he says, "aware of the existence of these strange irons, and commonly called them meteoric; but there seems to have been no suspicion of their true character and value. Indeed, until March 17, 1890, a specimen weighing 101·5 pounds, had ornamented the side-walk in front of a real estate office in the above-named town for about three years. The farmers in the vicinity of the locality where the fall had occurred had put some of the specimens to various uses." Prof. W. Cragin, of Washburn College, was the first scientific man who visited the farm upon which the meteorites had fallen. This was on March 13. He secured from one of the farmers five meteorites, aggregating in weight over a thousand pounds, the

heaviest specimen weighing 466 pounds. Mr. Snow himself shortly afterwards visited the place several times, and obtained five specimens, one of them being the meteorite which had been used as an ornament of a side-walk. The total number of masses included in the fall was at least twenty; and Mr. Snow says that the total weight of all the masses must have exceeded two thousand pounds. They fell within an oval area about one mile in length. "Some of the specimens were only partially buried in the ground; others were struck by the breaking plough at a depth of from three to four inches; others at the second ploughing, five or six inches deep; others yet, by the stirring plough at the third ploughing in a subsequent season." A specimen retained for the museum of the University of Kansas weighed 54·96 pounds. "It is," says Mr. Snow, "an irregular plum-shaped mass, much pitted, and covered with a burned and weathered crust. Its extreme length is about eleven inches, and its breadth is seven inches. This specimen, as well as the others mentioned above, so far as examined by the writer, belongs to that class of meteoric iron known as 'pallasite.' It is composed of nickeliferous iron, including many cavities throughout the entire interior. These cavities are filled with troilite and a yellowish, glassy mineral, which is probably olivine. Some of the latter is very dark and less transparent. The specific gravity, determined by Mr. E. C. Franklin, our assistant in chemistry, and obtained by weighing the whole mass, is 4·76. Two hundred and ninety-three grams have been removed from the larger end of the specimen, and a polished surface of about fifteen square inches has been obtained, which shows very well the structure. The Wiedmanstaeten figures, rather coarse in outline, were developed readily upon the polished iron surface by the application of nitric acid. The portion removed from the specimen is being used for analysis by Prof. E. H. S. Bailey and Mr. E. C. Franklin, and the results of the analysis will appear later."

M. V. FAYOD, of Nervi, near Genoa, has been appointed assistant in the bacteriological laboratory of the Faculty of Medicine in Paris.

THE post of Director of the Botanic Garden at Hamburg, vacant by the death of the late Prof. H. G. Reichenbach, will not at present be filled up: the Garden will remain under the care of the present Inspector, assisted by the botanists Sadebeck and Dingler.

Notarisia is no longer the only botanical journal in Italy devoted to the interests of algology. The first number has been issued of *La Nuova Notarisia*, a quarterly journal with a similar scope, published at Padua, under the editorship of Dr. G. B. De Toni, Director of the Botanic Garden at that University.

THE *Canadian Record of Science* for April records the opening of a botanical laboratory in connection with the McGill University, Montreal, under the control of Prof. D. P. Penhallow. The course of study to be pursued at the laboratory, which is furnished with microtomes, embedding baths, &c., embraces a thorough grounding in vegetable histology, and carries on those students who may desire it to a study of tissues and their constituent elements, and to the complete histology and life-history of plants.

THE Bulletin of the Torrey Botanical Club records that Miss Mary E. Banning has presented to the New York State Museum of Natural History a magnificent volume of illustrations in water-colour, accompanied by manuscript descriptions of about 175 species of the Fungi of Maryland, belonging mostly to the Hymenomycetes and Gasteromycetes.

THE trustees of Columbia College, New York, have adopted a report which, according to the *Nation*, completely reorganizes the College, and puts it definitively on the footing of a University, with faculties of philosophy, political science, mines, and law, each independent in its own sphere, but working under a University Council, made up of representatives of each faculty, and of some selections made by the President. The University will give the Master's and Doctor's degrees, and the Council will "advise the President as to all matters affecting these degrees, the correlation of courses, the extension of University work in new and in old fields, and generally as to such matters as the President may bring before it." The *Nation* attributes much importance to this change, which, it thinks, "must have the effect of stimulating the love of culture among the undergraduates, of making the University, more than ever, what all our colleges ought to be, but what only a few really are, a seat of learning."

GREAT efforts are being made in the United States to secure that American industrial products shall be well represented at the forthcoming Jamaica International Exhibition. A Committee has been appointed to make all necessary arrangements; and one of the advantages already obtained for exhibitors is that low freight rates will be charged for exhibits.

THE Smithsonian Institution has issued the tenth of the Toner Lectures, which have been established at Washington by Dr Joseph M. Toner, of that city, for the promotion of medical science. The new lecture is by Dr. Harrison Allen, and is entitled "A Clinical Study of the Skull." It is described by the author as "a contribution to the morphological study of diseased action." He expresses a hope that the results he has expounded may excite increasing interest in the proposition that "medicine for the most part is a science based on biology." "The study of biology," he says, "should not be the preparatory work of the tiro only, but should be the subject of increasing assiduity in every phase of medical work. The study of anatomical variation in the human frame is a phase of biology, and it is held in this connection to be a subject as important as any other which may claim the attention of the student of etiology of disease."

THE United States Hydrographic Office has called attention to the fact that the Bordeaux Chamber of Commerce has offered a series of prizes in order to induce masters and officers of vessels to test thoroughly the use of oil at sea. There are three sets of prizes, each set consisting of a first prize of 200 francs and a second prize of 100 francs. These prizes will be awarded for the best reports received by January 31, 1891, based upon actual experience.

ON Monday evening last, at the Surveyors' Institution, London, Mr. R. F. Grantham, M.Inst.C.E., read a paper entitled "The Encroachment of the Sea on some parts of the English Coast, and the best means of arresting it." After bringing forward evidence to show the rate of erosion on various parts of the coast, the author referred to several works for defending the coast-line from encroachment, best adapted for various situations, and described a system of groyning which had been successful for the past twelve years at Shoreham, Sussex, in protecting some land lying below the level of high-water of the tides, and in driving high-water mark further seawards. He suggested that in some instances where shingle travelled along the coast, inasmuch as groynes were necessary to protect sea-walls, the sea-walls might be omitted, and thus a substantial saving in the first cost of protection might be effected.

THE Pilot Chart of the North Atlantic Ocean shows the tracks of nine cyclones during the month of April; only five of these were of noteworthy severity: one, moving between Scotland and Iceland on the 1st and 2nd was the same great storm that gave birth to the tornado which wrecked Louisville on March 27. Another noteworthy cyclone originated north of Bermuda on the 1st, moved north-easterly at the high velocity of about 1080 miles a day, causing terrific gales along the transatlantic routes, and disappeared near Iceland on the 4th. A new feature during the month was the very unusual easterly

movement of the ice, so that, in addition to the large number of bergs south of the Banks, ice was constantly reported almost as far east as the 35th meridian, in latitude 46° and 47° N.

Dr. Max Buchner, who has spent a year and 9 months in Australia, Japan, China, and Manilla, has returned to Munich. He has brought back a valuable scientific collection for the Ethnographical Museum, of which he is the director.

Engineer and *Engineering* for May 16 print excellent leading articles on the disastrous accident to the West Coast Scotch express at Carlisle, on March 4; their principal reason being that the Board of Trade Report has just been issued, and that it is in many respects a remarkable document. The accident, as our readers will remember, was due to the driver losing control of the train on entering Carlisle Station, where it ran into a Caledonian engine waiting at the other end of the station. The Report issued by the Board of Trade contains all the available evidence, and the Inspector's opinions as to the cause. The question to be settled was, Why or how did the driver lose control of the train? The Inspector held that the driver was in fault, and this in the face of much evidence that did not support his theory. This evidence he got rid of by the simple expedient of rejecting it as untrue. Our contemporaries clearly demonstrate the real cause of the brake failure, and point out that the Board of Trade Inspector, even after the inquiry, did not understand the construction and working of the North-Western automatic vacuum brake, and that, therefore, his opinion is not worth the paper it is written on. For instance, in his Report he is evidently under the impression that it is possible for the driver to alter the working of the train-brake from automatic to non-automatic working from the foot-plate—an impossibility. The accident was caused by the train-pipe between the engine and train becoming blocked by ice, and thus causing the train-brake to become gradually useless, owing to the connection with the engine being closed. The engine-driver had no means of knowing this state of affairs except by applying the brake, which he did on approaching Carlisle, and found it of no use. The Board of Trade Inspector has thrown the blame of the accident on the driver—a man who, according to the evidence, displayed exceptional presence of mind in what he did. Had the Board of Trade Inspector been a trained railway engineer, he would certainly have come out of this inquiry more satisfactorily. The inquiry, or rather the result of it, distinctly points to the anomaly of officers, however eminent, adjudicating on matters concerning which they have not been thoroughly instructed.

In the year 1886, when Mr. John Gardiner was scientific adviser to the Board of Agriculture of the Bahamas, he was asked by Governor H. A. Blake to prepare a list of the flora of the colony. At the same time a list of the plants of New Providence, prepared some years before by Mr. L. J. K. Brace, was placed at his disposal. With this as a base, Mr. Gardiner set to work, and in due time his task was accomplished. The list, with notes and additions by Prof. Charles S. Dolley, has now been printed in the Proceedings of the Academy of Natural Sciences of Philadelphia. It is called provisional, as Mr. Gardiner explains in an introductory note, mainly because it is not backed throughout by herbarium specimens.

Mr. George W. Perry, of Rutland, Vt., writes to *Science* that European furze grows in one spot in the island of Nantucket, where it has maintained itself for fifty years. It was introduced by an Irishman, "who was homesick because it did not grow about his cabin, as in the old country." Mr. Perry believes it has not spread to any great extent. "It may be interesting to some," he adds, "that the Scotch heath also is found in one spot in the island, where it has continued for a long time." Mr. George M. Dawson, of the Geological Survey of Canada, also writes to our American contemporary about gorse or furze in the New World. He says it has for many years been fully naturalized in the southern part of Vancouver Island, where, along road-sides and in waste places near Victoria, it is very common. The broom is also abundant in similar situations in the same locality, and "both plants appear to be as much at home as in their native soil."

The new number of the Journal of the Anthropological Institute of Great Britain and Ireland contains, among other papers, an interesting address by the President, Dr. John Beddoe, in the course of which he refers to the vexed question as to the original seat of the Aryan race. Speaking of the fact that the Lithuanian language is regarded by some philologists as "the most primitive in form of the whole Aryan family," he points out that we have little definite knowledge as to the physical type of the Lithuanians. "Here, then," he says, "is a fine opportunity, well within reach, for a partisan of the European-origin theory. Let him go to Kovno or Vilna, and bring us back, thoroughly established, the true Lithuanian type."

A detailed description of the useful minerals and mineral waters of the Caucasus, by Prof. V. Möller, has appeared at Tiflis. The author is at the head of the Mining Administration of the Caucasus, and has availed himself of all accessible information on the subject. The work is illustrated by a map. It appears as the third volume of the second series of "Materials for the Geology of the Caucasus."

The U.S. Department of Agriculture has issued Parts I., II., and III., of a valuable "Bibliography of American Economic Entomology." These parts relate to the more important writings of B. D. Walsh and C. V. Riley, and have been prepared by Samuel Henshaw.

Messrs. Crosby Lockwood and Son will publish immediately a new "Pocket Book" for electrical engineers, which has been written by Mr. H. R. Kempe, of the Postal Telegraphs Department. They have also nearly ready a new work on "Electric Light Fitting," a practical hand-book for working electrical engineers, by Mr. John W. Urquhart, whose book on "Electric Light" is well known.

The same publishers have in the press a new elementary treatise on "Light," for the use of architectural students, by Mr. E. W. Tarn, forming a new volume of "Weale's Rudimentary Series"; also a revised and enlarged edition of Prof. Merivale's "Notes and Formulæ for Mining Students"; and a new edition of Mr. G. W. Usill's "Practical Surveying."

We understand that Mr. Caleb Pamely, of Pontypridd, has in the press a comprehensive treatise for the use of mining engineers, dealing with the whole subject of colliery working and management. It will be published by Messrs. Crosby Lockwood and Son.

At a recent meeting at Shanghai of the China Branch of the Royal Asiatic Society, Dr. Macgowan, the veteran scholar, presented a paper on the political domination of women ("gynæcocracy" or "gynarchy") in Eastern Asia. In the opening of the paper reference was made to the condition of the aboriginal peoples whom the Chinese found on the Yellow River on their arrival from Akkad. The Chinese then possessed the rudiments of civilization, of which the aboriginals were destitute. That this irruption of the Chinese was anterior to the invention of cuneiform writing in Akkad was probable, because of their use of quipos or knotted cords in keeping records. These quipos, the author said, and not mere tradition, were the base of Chinese archaic annals, and from them the earliest form of Chinese written characters was evolved. Anterior to these quipos, judging from certain neighbouring tribes, notched sticks were employed. With regard to the tribes which the Chinese

found existing on reaching their future home, Dr. Macgowan remarked that the philosopher of Universal Love, Motzu, proto-altruist and arch-heresiarch—whose sun was rising when the sun of Confucius was setting—enunciated views on the evolution of the state and family which are in accord with those of modern anthropologists. Men at first were in the lowest state of savagery: there was no Golden Age as depicted by sages and political philosophers until men felt the necessity of a remedy for the anarchy that prevailed. Practices of self-deformation were some of them remarkably curious, such as those of drinking through the nostrils, extracting front teeth and substituting dogs' teeth, head-flattening, &c.; the most striking was the attempt to raise a polydactylous race, by destroying all children who came into the world with the usual number of fingers and toes, and thus a tribe had a dozen fingers and as many toes. The writer then described a number of instances of rule by Amazons, and observed that it is chiefly among the aboriginal inhabitants that the chieftaincy of women obtains to this day. There is seldom an age in which one tribe or another does not afford examples; the more primitive the condition of these tribes the slighter is sexual differentiation as regards public governmental affairs, both civil and military. It was owing to rumours respecting tribes of this kind that fables and myths in Greece arose regarding Indo-Scythian Amazons. The paper, which is full of valuable ethnological matter, will be published in the Journal of the Society.

A PAPER upon the spontaneously inflammable liquid hydride of phosphorus, P_2H_4, is communicated by Drs. Gattermann and Haussknecht, of Heidelberg, to the new number of the *Berichte* (p. 1174). Owing to the disagreeable and highly dangerous properties of this substance, its chemical history has never been completed; very little, indeed, has been hitherto added to our knowledge concerning it since its discovery by Thénard in 1845. The Heidelberg chemists have devised a much better mode of preparing the liquid from phosphide of calcium, by means of which it is obtained in a state of almost perfect purity. A Woulfe's bottle with three necks and of about two litres capacity is three parts filled with water. The central tubulus serves to introduce a wide tube of 15 mm. diameter expanded into a funnel at the top and passing down to about three centimetres beneath the water. One of the side necks is fitted with a cork and a bent tube just dipping beneath the surface of the water, through which a current of hydrogen gas can be driven. The third tubulus carries the delivery tube which permits of the escape of first the hydrogen, and afterwards the products of the reaction between the calcium phosphide and the water, into a special form of condensing arrangement. The Woulfe's bottle is placed in a capacious water-bath, which is heated to 60° C. as soon as all the air is expelled by the current of hydrogen. The calcium phosphide is then introduced through the central wide tube in pieces about two grams in weight, until, in about 15–20 minutes' time upwards of 50 grams have been added. The escaping gases pass first through an empty wide test-tube in which most of the admixed water-vapour is condensed, then into an upright tube, narrowed in its lower half, and closed at the bottom, which forms a suitable receptacle for the liquid hydride. By means of an exit-tube the remaining gases are permitted to escape; owing to a little admixed and uncondensed vapour of the liquid, they burn spontaneously at the mouth of the tube. The condenser is surrounded with iced water instead of a freezing mixture, so that the condensation may be observed. In about five minutes after commencing the operation clear colourless highly refractive drops of the liquid form and run down into the narrower portion of the condenser, about 2 c.c. being obtained from 50 grams calcium phosphide. The experiment must not be performed in sunlight, otherwise the liquid rapidly decomposes, in the manner described by Thénard, into gaseous PH_3 and solid P_4H_2. By a slight addition to the above arrangement, all three hydrides of phosphorus may be simultaneously prepared. The escaping gases are allowed to pass through a large flask containing hydrochloric acid, which decomposes the vapour of the remaining liquid hydride, and large quantities of the yellow solid P_4H_2 separate out. The escaping gas, which may be collected over water, is non-spontaneously inflammable, and consists of practically pure PH_3. Liquid P_2H_4 boils constantly and without decomposition when not suddenly heated at 58° under a pressure of 753 mm. Its specific gravity at 12° is 1·007, nearly the same as that of water. Exposed to sunlight it becomes yellow in half an hour, due to the formation of solid P_4H_2, which remains at first dissolved; after 2–3 hours' exposure, the yellow solid begins to separate out, and in $1\frac{1}{2}$ days 0·2 gram is totally decomposed, in accordance with the equation $5P_2H_4 = 6PH_3 + P_4H_2$. Consequently, sealed tubes containing this substance exposed in daylight are very dangerous articles. Owing to the accumulation of PH_3 gas, they are apt to explode with deafening concussion and production of a wide-spreading and very brilliant flame, especially if the drawn-out end becomes accidentally broken off.

THE additions to the Zoological Society's Gardens during the past week include a Wanderoo Monkey (*Macacus silenus* ♀) from the Malabar Coast of India, presented by Miss Eileen Martin; two Leopards (*Felis pardus*) from India, presented by Mr. — Egerton; two Yellow-winged Blue Creepers (*Cœreba cyanea*) from South America, presented by Mr. H. E. Blandford; two Mandarin Ducks (*Æx galericulata* ♂ ♀) from China, presented by Mr. C. J. Kingzett; two —— Touracous (*Corythaix* sp. inc.) from South Africa, presented by Mr. C. W. Burnett; two Undulated Grass Parrakeets (*Melopsittacus undulatus*) from Australia, presented by Mr. A. Golden; two Common Vipers (*Vipera berus*), British, presented respectively by Mr. W. H. B. Pain and Mrs. Mowett; an Australia Peewit (*Lobivanellus lobatus*) from Australia, presented by Capt. Shepherd; a Himalayan Bear (*Ursus tibetanus* ♂), two Bengal Foxes (*Canis bengalensis*), two —— Hares (*Lepus macrotis*) from India, a Ruffed Lemur (*Lemur varius*) from Madagascar, deposited; two Bar-tailed Pheasants (*Phasianus reevesi* ♀ ♀), an Amherst Pheasant (*Thaumalea amherstiæ* ♂) from China, a Variegated Sheldrake (*Tadorna variegata* ♀) from New Zealand, two Black-headed Conures (*Conurus nanday*) from Paraguay, purchased; a Crested Porcupine (*Hystrix cristata*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on May 22 = 14h. 1m. 54s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 3770	—	White.	13 59 17	+52 54
(2) 69 Virginis	5·5	Yellowish-white.	13 21 35	−15 24
(3) B.A.C. 4699	5	Yellowish-red.	14 3 32	+44 23
(4) 20 Boötis	5	Yellow.	14 14 36	+16 49
(5) η Boötis	3	Yellowish-white.	13 49 30	+18 57
(6) U Cygni	Var.	Very red.	20 16 11	+47 33

Remarks.

(1) This very large nebula (101 M Boötis) has not yet been spectroscopically examined. According to the Parsonstown observations, it is at least 14′ across, and exhibits a spiral structure with arms and knots. It is everywhere faint, except in the middle. In the General Catalogue it is described as: "Pretty bright; irregularly round; at first gradually, then very suddenly much brighter in the middle to a small bright nucleus." The

spectrum of such a diffused mass is likely to possess great interest, and the nebula is so large that it will probably not be difficult to differentiate the spectra of different regions.

(2) In his catalogue of stellar spectra, published in 1887, Konkoly records an observation of this star, in which bright lines were strongly suspected. Notwithstanding the recent additions to this group by Prof. Pickering and Mr. Espin, the number is still very small, and it is important that suspected cases should be fully investigated. The lines suspected by Konkoly in 69 Virginis were C, D_3, and F—the three commonly observed in β Lyræ and γ Cassiopeiæ. It is quite possible that the appearance of the lines is periodic, and observations should therefore be continued for some time. If the lines are of any considerable brightness, the observations ought not to be difficult, as the bright lines in the 8th magnitude stars in Cygnus are easily seen and measured with 10 inches aperture. Any irregularities in the continuous spectrum, especially in the green and blue, should be noted; and, if possible, comparisons should be made with the carbon flutings. Prof. Lockyer has pointed out that a line near λ 447 is associated with D_3 in the Orion nebula, and also in the solar chromosphere, and it is important to observe whether this also applies to the bright-line stars. He has demonstrated by photographs that the line in the nebula (447) is coincident with one of the bright lines photographed by Prof. Pickering in P Cygni.

(3) Dunér describes the spectrum of this star as a magnificent one of Group II., particularly in the red end. The bands 1–9 are all strongly marked. The star is thus probably a little more advanced in condensation than the mean species of the group, and it will be interesting to know what line-absorptions appear at this stage, and also what is the extent of carbon radiation.

(4 and 5) These are given in Vogel's catalogue as stars of the solar type and of Group IV. respectively. The usual observations are required in each case.

(6) This is one of the very few variables with spectra of Group VI. So far, we have no records of any changes in spectra which may accompany the variations in magnitude, and the cause of the variability is, consequently, very imperfectly understood. Dunér says that the spectrum consists of three zones rather feebly developed, band 6 (near λ 564) being weak, but he does not state the magnitude of the star at the time of his observation. The next maximum will occur about May 28. The star ranges from about magnitude 7·5 to < 11 in a period of 461 days. Changes of colour should also be noted.

A. FOWLER.

SPICA.—At the Berlin Academy of Sciences on April 24, Prof. Vogel announced that Spica consists of two close stars revolving round their common centre of gravity. The star's spectrum is that of Class IV., and twice in April 1889 the F line appeared to be shifted towards the violet end of the spectrum as compared with the H β line given by a vacuum tube, whilst once in the following month the shift appeared to be towards the red end. These observations and others made this year of the star's motion in line of sight are given in the following table, approach to the sun being indicated by (−) and recession from the sun by (+), both being expressed in German miles per second:—

Potsdam Mean Time.		h.	m.	Observed Motion.	Relative to the Sun.	✳−☉
1889 April	21	9	15	−11·6	−0·7	−12·3
,,	29	11	10	−12	−1·2	−13·2
May	1	10	58	+7·5	−1·3	+6·2
1890 April	4	11	30	−3·4	+0·5	−2·9
,,	9	10	30	−14·2	+0·2	−14·0
,,	10	11	30	−0·3	+0·1	−0·2
,,	11	10	50	+7·6	0·0	+7·6
,,	13	10	50	−14·7	−0·1	−14·8
,,	15	10	0	+11·3	−0·3	+11·0

The observations have been reduced to the epoch 1890, April 2, 10h. Potsdam mean time, and the period of revolution of the system determined as 4 days 0·3 hours. The greatest motion in line of sight due to the orbital velocity is about 12 miles a second, and the system as a whole is moving towards the earth with a velocity of about 3 miles a second.

From this it is found that the distance between the components of the system is 660,000 miles, and their total mass = 1·2 that of the sun.

It will be remembered that Algol is a spectroscopic double of the same character as the above described.

THE METEORIC THEORY OF COMETS.—In the *Sidereal Messenger* for May, Mr. W. H. S. Monck discusses the evidence that has been brought forward in support of this theory, and in connection with the meteoritic origin of the universe. Only four comets are definitely known to be connected with meteor-showers; and conversely, only four meteor-showers have been connected with comets; these comets are all periodic, the longest period being 415 years. From this fact it is argued that there is not sufficient evidence to allow the assertion that all comets are connected with meteor-swarms and that the ejection theory advocated by the late Mr. Proctor is supported. It is asserted that, since an ejection from a rapidly cooling body may be partly solid, partly liquid, and partly gaseous, the gaseous matter might form the comet and the solid (or solidified) matter form the attendant meteors; but for this origin to be true the assumption must be made that two planets exist beyond Neptune. Mr. Monck argues that because Arcturus was seen through 90,000 miles of Donati's comet, whereas Saturn's rings (except perhaps the inner crape ring) are not transparent, the rings must be more than 1000 times as dense as the comet at the point where it crossed between us and Arcturus, hence meteoritic collisions should be more frequent and the effect of the increased temperature should be made clearly manifest in the spectrum. The meteoritic hypothesis is not, however, objected to as a working hypothesis, but is said to be on an equality with the older nebular hypothesis; and the writer does not think the spectroscope will ever afford a crucial test between the two, for the reason that it cannot distinguish between a large solid, surrounded by a gaseous envelope, and a number of small bodies with interspaces filled with gas.

MASS OF SHOOTING-STARS.—Mr. C. C. Hutchins, in the *American Journal of Science* for May, gives the result of an investigation undertaken with the object of finding data for determining the mass of shooting-stars. Having determined the radiant energy of the standard candle, it was found that on the supposition that the rays of a meteor have the same ratio of visible to total energy as those of the candle, the mass of a meteor at a distance of 50 miles, having a magnitude equal to Vega and a velocity of 25 miles per second, would be 0·2936 gram if it continued two seconds. If the meteor in burning produce, for a given expenditure of energy more light than the candle, then a less mass would serve to produce the light given by it. A lump of the Emmett Co. (Iowa) iron meteorite was placed upon the lower carbon of an arc lamp and vaporized by the passage of the current, and it was found that for a given expenditure of energy the arc of meteoritic vapour gave ten times the light of the candle, hence the mass of a meteor giving the light of a first magnitude star moving with parabolic velocity, and lasting for two seconds, is 0·029 gram.

PHOTOGRAPHS OF THE MOON.—Admiral Mouchez, at the meeting of the Paris Academy of Sciences of May 12, presented a note on some new photographs of the moon obtained by the Brothers Henry at Paris Observatory. The instrument used was the equatorial 0·32 metres aperture, destined for the map of the heavens. The photographs are said to be far superior to those obtained in England and the United States with larger apertures, the superiority of the results being ascribed not only to the perfection of MM. Henry's objectives, but also to the method of direct enlargement adopted.

THE ROYAL SOCIETY CONVERSAZIONE.

THE *conversazione* held by the Royal Society on May 14 was in every way most successful. The attendance was large, and everyone was pleased and interested by the programme. We note some of the objects exhibited:—

The Director-General of the Geological Survey exhibited:—(1) A series of specimens illustrating deep borings in the south of England. In this case was arranged a series of cores and specimens from all the deep bores which during the last thirty years have been made in the south of England in search of water. They included the borings at Richmond, Crossness, Kentish Town, Meux's Brewery, Streatham, Turnford, Ware, Chatham, Gayton and Orton in Northamptonshire, Harwich, and Swindon. The positions of these bores were shown on the large index

map suspended in the same room.—(2) Series of specimens illustrating the dynamical metamorphism of rocks. This case contained an important collection of specimens from Switzerland, Norway, and Scotland, illustrating some of the more remarkable effects of the mechanical deformation and recrystallization of rocks. The first series was one of specimens of Triassic and Jurassic dolomites and limestones from Canton Glarus, showing the extraordinary manner in which these rocks have been squeezed and puckered. Attention was particularly directed to the evidence afforded by the fossils (*Belemnites*) of the extent to which the strata have been stretched in some parts. The second series, from the south of Bergen, showed the presence of recognizable Silurian corals and trilobites in rocks which have been so much metamorphosed as to have acquired the characters of finely crystalline phyllite or micaceous schist. The third series, from the north-west of Scotland, illustrated how a massive quartzite, full of annelide-tubes, has been crushed and recrystallized until it has assumed the structure of a quartz-schist, and all trace of the fossils has been obliterated. The effects of mechanical movements even among the comparatively young and soft rocks of the south of England were illustrated by two specimens placed in this case from the under-surface of a "thrust-plane" in the vertical chalk of the Dorsetshire coast. They showed how the chalk has been indurated, smoothed, and polished by the movement of the overlying mass. A view and section of this thrust-plane were placed beside the specimens.

Specimens of minerals brought from Ceylon by C. Barrington Brown, exhibited by Prof. J. W. Judd, F.R.S. Large perfectly crystallized and clear beryl, 2650 grammes in weight. The specimen, though water-worn, exhibits the crystalline form. The colour is intermediate between that of emeralds and aquamines. The specific gravity is 2·703. Fine crystal of yellow corundum (oriental topaz). Well developed crystals of corundum (sapphires, &c.). Crystal of chrysoberyl from the same district.

Maps to illustrate magnetic surveys of special districts in the United Kingdom, exhibited by Profs. Rücker and Thorpe, FF.R.S. The arrows represent the horizontal disturbing forces in magnitude and direction. The figures give the vertical disturbing force in terms of 0·00001 C.G.S. units, taken as positive when it acts downwards. In some maps, regions of great (downward) vertical force are indicated by deeper tints. Map 1. Indications of an attracting centre at sea, to the south of the Hebrides. Map 2. Horizontal disturbing forces at stations near the boundaries of a district in Yorkshire and Lincolnshire, within which there is a locus of attraction. Map 3. Regions of high vertical force within the above district. The highest observed values are at Market Weighton and Harrogate. Map 4. Ridge line or locus of attraction drawn (continuous line) by connecting stations of maximum vertical force, and (dotted line) by connecting points midway between the stations at which the direction of the horizontal force disturbance changes. Map 5. Ridge line, 150 miles long, probably correct to within five miles for the greater part of its length.

Mr. C. V. Boys, F.R.S., exhibited:—(1) Oscillating spark experiment. This is a modification of the method employed by Dr. Lodge to show the oscillatory nature of a spark formed under proper conditions. Six lenses are mounted on a disk, and are made to rotate. Each forms upon a screen an image of the spark, which is drawn out by the movement of the lens into a broken band of light. The lenses are not exactly the same distance from the axis, so that the band formed by one is not overlapped by the band formed by the next. Thus the whole duration of the spark from the first to the last oscillation may be observed or photographed. —(2) Photographs showing the formation of drops. Water drops, half an inch or more in diameter, were allowed to slowly form and break away in a liquid of slightly lower specific gravity—namely, a mixture of paraffin and bisulphide of carbon. Photographs of these were taken as follows: they were illuminated by an electric arc and large condensing lenses, a camera was placed in front, and the view was rendered intermittent by a card disk with one hole near the edge made to rotate at from fourteen to twenty turns a second. The exposure was about one eight-hundredth of a second. Forty inches of photographic plate were arranged in a long slide which could be drawn past by hand. Three of these multiple photographs are exhibited. The thaumatrope was made by sticking the separate parts of the last series round a card disk, and afterwards painting the surface black and white, following the outlines of the photographs exactly. The thaumatrope clearly shows the gradual formation of the drop and the spherule, the oscillation of the pendant drop immediately afterwards, the rebound of the spherule from the pendant drop, the oscillation of the large drop as it falls, and its rebound from the water below into which it fell. Other photographs are shadows of water jets cast upon a photographic plate by the action of a small distant spark, a method invented by Mr. Chichester Bell. The remainder are photographic shadows cast by a water jet upon a rapidly moving plate by the intermittent light of an oscillating spark. These clearly show the movement of the separate water drops.

Sugar-cane (*Saccharum officinarum*) seed and seedlings, exhibited by Mr. D. Morris. There appears to be no authentic record of any really wild station for the sugar-cane, and the fruit has not hitherto been figured or described. At Barbados, several times during the last twenty years, and more recently by Prof. Harrison and Mr. Bovell, self-sown seedlings of the sugar-cane have been observed. The subject was taken up systematically in 1888, and about sixty of the seedlings raised to mature canes. Many of these exhibited well-marked characteristics differing from the varieties growing near them. Careful inquiry has shown that canes known as the "purple transparent" and "white transparent," and possibly also the "Bourbon" cane, produced seeds in very moderate quantities. Spikelets received at Kew have been examined and the seed found *in situ*. It is anticipated that, by cross-fertilization and a careful selection of seedlings, it will now be possible to raise new and improved varieties of sugar-cane, and renew the constitutional vigour of plants that have become deteriorated through continuous cultivation by cuttings or slips. Great importance is attached to the subject in sugar-producing countries, as it opens up an entirely new field of investigation in regard to sugar-cane cultivation.

Prof. H. Marshall Ward, F.R.S., exhibited a selection of transparent photographs, showing (1) the habits, &c., of various trees from different parts of the world; (2) the comparative structure and anatomy of several European timbers; and (3) some of the more prominent features of diseases of wood, &c., and fungi causing them.

The electrification of a steam jet, exhibited by Mr. Shelford Bidwell, F.R.S. The shadow of a small jet of steam cast upon a white wall is, under ordinary conditions, of feeble intensity and of a neutral tint. But if the steam is electrified, the density of the shadow is at once greatly increased, and it assumes a peculiar orange-brown hue. The electrical discharge appears to promote coalescence of the exceedingly minute particles of water contained in the jet, thus forming drops large enough to obstruct the more refrangible rays of light. It is suggested that this experiment may help to explain the intense darkness, often tempered by a lurid yellow glow, which is characteristic of thunder-clouds. See *Phil. Mag.*, Feb. 1890, p. 158.

Mr. Killingworth Hedges exhibited:—(1) Gramme dynamo worked as a motor, fitted with bearings of a new carbon composition, which does not require oil for lubrication.—(2) Vortex speed indicator, driven by the above, fitted with oilless bearings.

Lord Rayleigh, Sec.R.S., exhibited:—(1) An instrument for testing colour vision.—(2) Polarization of light by chlorate of potash crystals.

Photographs of eggs of the Great Auk, exhibited by Mr. Edward Bidwell. There are 67 recorded eggs of this extinct bird, of which 45 are in Great Britain. The collection of photographs exhibited consists of two views each of 53 of these eggs, photographed to scale.

Specimens of Simony's Lizard (*Lacerta simonyi*), from the lonely rock of Zalmo, near the Island of Ferro, Canaries, exhibited by the Zoological Society of London. A rare lizard, only known from this spot, and said to feed on crabs. These lizards were obtained by Canon Tristram, F.R.S., during his recent visit to the Canaries, and presented to the Zoological Society by Lord Lilford.

Electric-radiation meter, for obtaining quantitative measurements of the intensity of the radiations emitted by an electric oscillator, exhibited by Mr. Walter G. Gregory. Its action is based on measuring the increase of length of a stretched wire, or strip of metal, when heated by the currents induced in it by the rapidly varying field of force. In the instrument exhibited, the elongation of a fine platinum wire is shown by attaching to one end of it a fine helical spring made by winding a thin metallic ribbon round a cylinder. As the wire extends the spring rotates, and the motion is further magnified by a small mirror which reflects the image of a wire on a scale. The oscillator is

of the usual type, and is worked by an induction coil and four accumulators, the latter kindly lent by the Electric Construction Corporation.

Breath figures, showing that polished surfaces placed near to bodies in low relief often take an impression of the detail, which is made visible by breathing upon the surface (the period of exposure varying in different circumstances), exhibited by Mr. W. B. Croft. (1) A coin is lightly pressed on a freshly split surface of mica for 30 seconds; the mica takes a breath figure of the detail of the coin. (2) Paper printed upon one side has lain for 10 hours between two plates of glass; the print appears in white letters on both. Part of this phenomenon, although not with print, was noticed by Moser in 1840. (3) Sometimes the print appears in black letters; the same impression may change from white to black. (4) Coins are put on the two sides of a piece of glass and electrified for two minutes; each side has a perfect impression of that side of the coin which faced it. An electrotype plate may be reproduced in a similar way. These effects were partly indicated by Karstens in 1840. (5) An electric spark is sent across glass. Five superposed bands appear, black and white, of decreasing breadths, as well as three permanent scars. Riess, 1840. (6) The microscope shows water particles over the whole surface, larger or smaller as the effect is black or white.

Prof. Silvanus P. Thompson exhibited:—(1) Optical rotator. This apparatus is for rotating the plane of polarization of light, and is intended to be used in conjunction with polarizing reflectors (black-glass mirrors, &c.), which do not admit of being bodily rotated around the axis of the beam of light. The principle of the new rotator consists in the employment of two quarter-wave plates of mica, one of which is fixed at 45° across the plane-polarized beam of light, which it thus converts into circularly-polarized light. The second quarter-wave plate, which can be rotated by a simple gear, reconverts the circularly-polarized beam into plane-polarized light, vibrating in any desired azimuth. (Constructed by Messrs. Newton and Co.)—(2) Natural diffraction-grating of quartz. This specimen of iridescent quartz exhibits diffraction-spectra corresponding to those of a grating ruled to 12,000 lines to the inch. A microphotograph taken by Mr. C. L. Curteis, with a Reichert's apochromatic (3 mm.) lens, shows the nature of the minute structures of the specimen. For the sake of comparison, a diffraction-grating of 6000 lines to the inch, photographed on glass, is exhibited beside the piece of quartz.—(3) New straight-vision prisms, consisting each of a single prism of Jena glass, of very wide angle, immersed in cinnamic ether. The materials having identical mean refractive index, rays of mean refrangibility pass straight through. (Constructed by Messrs. R. and J. Beck.)—(4) Colour experiments. Two liquids, incapable of mixing, are placed over one another in a flat bottle. They are chosen so that each absorbs all the rays that the other one can transmit. Though each is transparent, they are jointly absolutely opaque. They are also opaque when shaken up together.

Experimental illustration of the recent investigations of M. Osmond on molecular changes which take place during the cooling of iron and steel, exhibited by Prof. W. C. Roberts-Austen, F.R.S.. In the case of mild steel, containing 0·5 per cent. of carbon, as it cools down from a temperature of 1100° C., two points may be observed at which heat is evolved. The first of these occurs at 750° C., and marks the change of β (or hard) iron to α (or soft) iron. The second evolution of heat is observed at 660°, and is due to a change in the relation of the carbon and iron. M. Osmond, in continuing an investigation made by Roberts-Austen, has shown that the presence in iron of elements with small atomic volumes retards the change of β to α iron, and, conversely, elements having large atomic volumes hasten the change.

Specimen of phosphorous oxide, and apparatus for preparing same, exhibited by Prof. Thorpe, F.R.S., and Mr. Tutton. This substance has been shown by the exhibitors to be represented by the formula P_4O_6. It crystallizes in monoclinic prisms melting at 25°·5, and boils in an atmosphere of nitrogen or carbon dioxide at 173°. Cold water dissolves it with extreme slowness, forming phosphorous acid. With hot water, strong caustic alkalies, chlorine, bromine, and alcohol it reacts with great energy, generally with inflammation. Oxygen slowly converts it, at ordinary temperatures, into phosphoric oxide, and under diminished pressure the combination is attended with a faint luminous glow similar to that observed in case of phosphorus. No ozone, however, is formed. At slightly higher temperatures the oxidation is brought about instantly with production of flame. Phosphorous oxide possesses the smell usually attributed to phosphorus, and which is identical with that noticed in match manufactories. It is highly probable, as Schönbein surmised, that the element phosphorus is without smell, and that the smell ordinarily perceived is due to a mixture of ozone and phosphorous oxide. Phosphorous oxide is highly poisonous, and it is not improbable that phosphorus necrosis is caused by this substance.

Photographs of the spectrum of the nebula in Orion, exhibited by Prof. J. Norman Lockyer, F.R.S. These photographs were taken in February with the 30-inch reflector at Westgate-on-Sea, the exposures varying from 2 to 3 hours. The one taken with a 3 hours' exposure (February 10) shows about 50 lines between λ 500 and λ 373, but many of them are only visible with difficulty, especially in artificial light. The Henry Draper Memorial photograph of the spectrum of P Cygni was shown for comparison, and it was seen that all the bright lines were amongst the brightest in the nebula. This argues in favour of the view that stars with bright-line spectra are of a nebulous character.

Photograph of the two clusters (33 and 34 H VI.) in the sword-handle of Perseus, showing remarkable coronal and festoon-like groupings amongst the stars on several parts of the photograph, exhibited by Mr. Isaac Roberts. These clusters are quite free from nebulosity, and in this respect they differ from other clusters which Mr. Roberts has photographed; for those clusters are involved in faint but distinct nebulosity.

The larvæ of Amphioxus, exhibited by Prof. E. Ray Lankester, F.R.S.

A selection from the butterflies collected in the great equatorial forest of Africa by Mr. William Bonny, one of Mr. Stanley's staff, exhibited by Mr. Henley Grose-Smith. Little was known of the Lepidoptera of this part of Africa; few of the species collected by Mr. Bonny have been previously recorded from that region, and nine are new to science. The collection includes, amongst others, the great *Papilio antimachus*, also *Papilio zalmoxis*, and many West African species.

Collection of iridescent crystals of chlorate of potash to illustrate the production of colour and its intensification by reflection from multiple thin plates, exhibited by Dr. Alex. Hodgkinson.

Dr. Alexander Muirhead exhibited:—(1) Some patterns of Dr. Lodge's lightning protector for cables and for telegraphic work generally. In these instruments a series of air-gaps, separated by self-induction coils, are offered to the lightning, or other high-tension currents, which have got into the line. The greater part of the flash jumps the first air-gap, most of the residue jump the next, and so on, until after four or five dilutions nothing is left which can break down the thinnest insulation, or appreciably affect even a delicate galvanometer connected to the protected terminals.—(2) Muirhead's portable form of the Clark standard cell, in cases, with thermometer.—(3) Standard condenser, ⅓ microfarad (with Dr. Muirhead's certificate).—(4) Set of Thomson and Varley slides, small.—(5) Saunders's capacity key, suitable for Dr. Muirhead's capacity test.—(6) Saunders's reversing key.

Specimens of aluminium and alloys manufactured by the Aluminium Company, Limited, exhibited by Sir Henry E. Roscoe, F.R.S. Pigs of aluminium, 99 per cent. pure. Castings in aluminium, rough and finished. Specimens of aluminium, soldered. Aluminium wire, sheet and drawn rod. Aluminium medals, plain and gilt. Cast aluminium bronze and brass, showing (*a*) tensile strength and elastic limit; (*b*) twisting stress; (*c*) thrusting stress, long specimens; (*d*) thrusting stress, short specimens. Stampings in aluminium bronze, rough. Ten per cent. aluminium bronze, twisted cold. Five per cent. aluminium bronze, worked hot and cold. Aluminium brass, worked hot and cold. Aluminium bronze and brass sheet.

Specimens illustrating ancient copper and bronze from Egypt and Assyria, exhibited by Dr. Gladstone, F.R.S. The collection consists of borings from tools found by Mr. Flinders Petrie, at Kahun, in Egypt, and which belong to the XII. Dynasty—about B.C. 2500; also from other tools found at Gorub, which belong to the XVIII. Dynasty—about B.C. 1450. There are also fragments of Egyptian bronze figures from Bubastis, and of Assyrian bronze from the gates of the Palace of Shalmanezer II., at Balawat—about B.C. 840; as well as two pieces of slag from the old copper mines of the Sinaitic Peninsula, which were worked by the Egyptians in very early times, and discontinued after the XVIII. Dynasty. The principal point illustrated is the fact that the earliest metal implements were of copper, containing a very little arsenic and tin, probably as accidental im-

purities, and that afterwards tin was added to the copper in increasing proportions with the object of producing a hard alloy.

Mr. Percy Newberry, exhibited by permission of Mr. W. M. Flinders Petrie :—(1) Three pages of an ancient Egyptian book on medicine written on papyrus, by a scribe named Usertesen Sen, in the twenty-sixth or twenty-fifth century before Christ. This papyrus, together with a number of others of the same date referring to miscellaneous subjects (letters, legal documents, accounts, a fragmentary treatise on mathematics, &c.), was recently discovered by Mr. W. M. Flinders Petrie, during excavations in a ruined town of the XII. Dynasty, at Kahun, in Central Egypt. It contains directions for the use of midwives, written in black and red ink, in hieratic characters (a cursive or written form of hieroglyphics). The black ink is used in the body of the work for the symptoms, diagnoses, and prescriptions, and the red ink is used at the heads of the sections. The following translation of the last two and a half lines of the first page will serve to show the kind of directions given in this ancient work :—"*Treatment of a woman*[1] who is pained in her legs and in all her limbs, as one who is beaten. *Say with regard to her,*[1] it is the growth of the *at* (vulva). *Do thou with regard to her*[1] thus : let her eat grease until she is cured."—(2) Facsimile of an unpublished papyrus preserved in the British Museum containing medical prescriptions written in the Egyptian hieratic writing of the XIX. Dynasty (B.C. 1400–1200). This papyrus is chiefly interesting from the fact that it contains prescriptions copied from an earlier work, now lost, which is said (by the ancient copyist) to have dated from the IV. Dynasty (*circa* B.C. 4000). Facsimiles of these two papyri, together with translations, notes, &c., will shortly be published, under the editorship of Mr. F. Ll. Griffith and Mr. Newberry.

Egyptian spear-head of bronze, bearing the name and titles of Kames, a king at the end of the XVII. Dynasty, *circa* 1750 B.C., exhibited by Dr. John Evans, Treas. R.S. The blade is cast, and the socket is made of hammered bronze, and these two pieces that form the weapon seem to have been "burnt" together.

MM. Richard Frères, Paris, exhibited :—(1) Continuously recording hair hygrometer. This is the latest form of the Saussure hair hygrometer, so much used on the Continent, owing to its working satisfactorily when most other hygrometers fail, viz. near 32° F. In some of Saussure's instruments more than one hair was used, but in none did the apparatus give a continuous record. In the present hygrometer, the expansion and contraction of a bundle of hairs raise and lower a pen, which leaves on a paper-covered cylinder a continuous record of the humidity of any position, garden, or sick-room in which it may be placed.—(2) Curves produced by the anemometers on the summit of the Eiffel Tower, and on that of the Central Meteorological Office at Paris. These show (1) that the average velocity of the wind on the top of the tower (994 feet) is about $3\frac{1}{2}$ times that at 66 feet, and (2) that the hour of greatest average velocity on the summit was 11 p.m., whereas at 66 feet (as at most observatories), it was 1 p.m. ; so that the times of maximum and minimum are almost precisely reversed.—(3) Isochronous regulator for electric contacts. An instrument for making and breaking electrical contact at equal intervals of time.

Chætopodæ, Medusæ, Ascidians, Nudibranchs, and other Invertebratæ, prepared as lantern slides, showing not only the general form, but also much of their anatomy, exhibited by Mr. H. C. Sorby, F.R.S. The success of the method depends on the fact that when soft-bodied animals are dried on glass the extreme edge dries first, and adheres firmly, so that on further drying the animal does not contract irregularly, but becomes thin and flat, and shows like a drawing projected on the plane of the glass. In many cases the natural colour is well seen, but in other cases artificial staining is used, which brings out the anatomical structure to great advantage. In some cases the specimens are best seen by reflected light, and it is then well to use a photographic slide, taken under such conditions. Some details may also be brought out to greater advantage by means of a properly developed photograph.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Among the distinguished persons on whom honorary degrees will be conferred at the *Comitia Maxima*, on June 10, are the following :—Mr. Henry M. Stanley, Sir Andrew Clark, F.R.S., President of the Royal College of Physicians, Jonathan Hutchinson, F.R.S., President of the Royal College of Surgeons, George Richmond, R.A. (retired), Prof. J. J. Sylvester, F.R.S., Dr. John Evans, Treasurer R.S., and Alexander J. Ellis, F.R.S.

A discussion by the Senate took place on May 17, on the proposal, recently referred to in NATURE, that the experimental work in chemistry carried on by candidates previous to the Natural Sciences Tripos Examination (Part II.) should be allowed to count in determining the places in the Class List. The opinions expressed by members of the Senate were, in general, unfavourable to the proposal, as tending to diminish the confidence felt in the independence of the Examiners. It was stated that it would be impossible to make commensurable in practice the testimonials to the work of candidates given by different teachers ; and further, that it would tend to make the superintendence of that work more formal, and so diminish its freshness and originality.

The Council of the Senate report that in January 1887, the late Mrs. Clerk Maxwell bequeathed the residue of her estate to the University for the purpose of founding a Scholarship in the Cavendish Laboratory at Cambridge, to be called the "Clerk Maxwell Scholarship." The estate of the testatrix has now been realized, and the residuary account furnished by the executors shows a balance of £5963 14*s.* 10*d.* with accruing interest on the sum of £5000 deposited with the National Bank of Scotland. After consulting the Lucasian and Cavendish Professors and Mrs. Clerk Maxwell's executors, the Council have framed regulations for the Scholarship, of which the following are the most important :—

A Scholarship to be called the Clerk Maxwell Scholarship shall be instituted in the University in connection with the Cavendish Laboratory, for the advancement by original research of experimental physics, and especially of electricity, magnetism, and heat.

The person elected to the Scholarship shall be called the Clerk Maxwell Student in Experimental Physics.

Any member of the University who has been a student for one term or more in the Cavendish Laboratory shall be eligible for the Scholarship.

The Electors to the Scholarship shall be the Cavendish Professor of Experimental Physics and the Lucasian Professor of Mathematics, and in case of any difference of opinion between them the final decision shall rest with the Master of Trinity College or with someone specially appointed by him for this purpose.

The Electors, in electing the student, shall be guided by the promise shown by the candidate of capacity for original research in experimental physics, and shall take such steps as they may think desirable to enable them to form a judgment of such promise.

The student so elected shall devote himself, under the direction of the Cavendish Professor, to original research in experimental physics within the University ; he may, however, carry on his researches elsewhere if he has first obtained the written permission of the Cavendish Professor to do so.

The Scholarship shall be tenable for three years, and a student who has once held the Scholarship shall not be capable of re-election.

SCIENTIFIC SERIALS.

THE most important paper of original research in the numbers of the *Journal of Botany* for March, April, and May, is the conclusion of Mr. G. Massee's "Monograph of the Genus *Podaxis*," in which he gives his views of the systematic position of this genus of Fungi consequent on some recent discoveries as to its structure, together with descriptions of the seven known species, one of them new.—Messrs. H. and J. Groves describe and figure an interesting addition to the British flora in the minute *Nitella Nordstedtiana*.—Dr. W. O. Focke gives a description of no less than fifty-two species or forms of British *Rubi*.—Mr. R. A. Rolfe contributes a monograph of a small and interesting genus of Orchids, *Scaphosepalum*.—Mr. E. M. Holmes enumerates the marine Algæ of Devon.—Messrs. Britten and Boulger's "Biographical Index of British and Irish Botanists," has now advanced as far as the letter Q.

[1] In red ink.

WITH the exception of an enumeration of the flora of the little island of Giannutri, off the coast of Tuscany, the number of the *Nuovo Giornale Botanico Italiano* for April is chiefly occupied with a report of the meetings of the Italian Botanical Society, held sometimes at Florence, sometimes at Rome. The papers here reported are mostly of interest to Italian botanists: those of more general importance being chiefly by Prof. Arcangeli, who continues his researches on the interesting points of structure in the anatomy of *Euryale*, and other members of the water-lily family.

Memoirs of the St. Petersburg Society of Naturalists, Zoology and Physiology, vol. xix. No. 2.—The article on the fauna of the White Sea, by V. M. Shimkevitch, contains two separate monographs. One of them deals with the *Balanoglossus*, and is a detailed anatomical research into its structure, thus making a most valuable addition to the work of Kovalevsky, Agassiz, Metchnikoff, Spengel, Balfour, and Bateson. The author's conclusions are, that the *Balanoglossus* occupies an intermediate position between the worms and the *Chordata*. It has originated from a trochozoon which acquired some features in common with worms, as well as some features distinctive of the *Chordata*. The other monograph deals with the *Enteropsis dubius*, a new parasitic species closely akin to Aurivillius's *Enteropsis sphinx* (*Krustaceer hos arktiska Tunicater*). The morphology and embryology of this new Copepod are dealt with, and they are followed by general remarks about the history of development of parasitic *Copepods*. Both papers are well illustrated with plates.—A paper by N. N. Polejaeff deals rather too shortly with the following topics: on the filaments which are found in the *Hircinidæ* sponges; on spermatogenesis of the *Porifera*; on the anatomy of *Chalinidæ*; on the *Luffaria*; and on the new genus of *Porifera*, the *Korotnewia desiderata* (with plates).—The appendix to the same volume contains a detailed experimental inquiry, by A. Gendre, into the causes of death in animals when the excretory functions of the skin are artificially stopped.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 8.—"On the Heating Effects of Electric Currents. No. IV." By William Henry Preece, F.R.S.

The following table gives the fusing constants and fusing temperatures for all the metals in general use when bare and exposed in still air:—

	Fusing constant. (*a*)	Fusing temperature. ° C.
Copper	2530 ...	1054
Silver[1]	1900 ...	954
Aluminium	1873 ...	650
German Silver	1292 ...	1200
Platinum	1277 ...	1775
Platinoid	1173 ...	1300
Iron	774·4 ...	1600
Tin	405·5 ..	226
Lead	340·6 ...	335
Alloy (lead 2 parts, tin 1 part)	325·5 ...	180

The table means that if we take, for example, a uniform copper wire of 1 cm. diameter, a current of 2530 amperes will raise it to 1054° C., and therefore fuse it; and if we take any conductor of copper of similar form, but of different diameter (d), the fusing current (C) is

$$C = 2530d^{\frac{3}{2}}.$$

The fusing current of any other material is obtained from the equation

$$C = ad^{\frac{3}{2}}.$$

It seemed natural that these constants, marking such a distinct and well-defined fiducial point, should also enable us to obtain the currents that would raise the wire to any other temperature.

It is shown that if we determine C for any temperature, then, since $\theta = \kappa C^2$, θ being the rise of temperature, the current C′ producing any other temperature θ' is obtained by means of the equation

$$C' = C\sqrt{\frac{\theta'}{\theta}}.$$

[1] G. Roux, *L'Électricien*, December 14, 1889.

Now, for copper of 1 cm. diameter C = 2530 for 1054°. Thus, if we want to find the current that will raise such a conductor to 13°, we have

$$C' = 2530\sqrt{\frac{13}{1054}} = 281 \text{ amperes,}$$

if the surface be equivalent to the normal surface at white heat.

The coefficient which converts this normal surface emissivity to that of the surface of ordinary wires at low temperatures by taking the normal surface as unity, is as follows:—

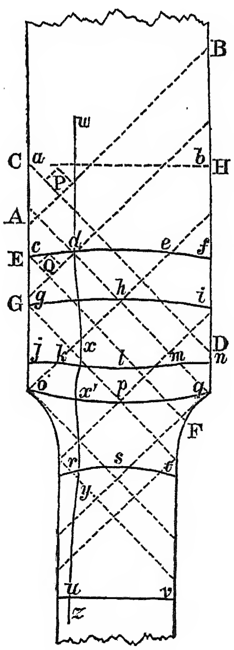
	Surface coefficient.
Bright and polished copper	0·5
Copper, dirty, oxidized, or blacked with shellac varnish	0·6
Copper, well coated with lamp-black	1·0

Thus, the current (C) producing 1054° (θ) in a bare bright copper cylinder of 1 cm. diameter is 0·5 × 2530 amperes, and that which would produce a temperature of 10° C. is

$$0·5 \times 2530\sqrt{\frac{10}{1054}} = 123·2 \text{ amperes.}$$

Thus, if we know the current producing any fixed temperature in any cylindrical conductor, we can readily calculate the current required for any other temperature.

Physical Society, May 2.—Prof. W. E. Ayrton, President, in the chair.—Mr. C. A. Carus-Wilson read a paper on the distribution of flow in a strained elastic solid. The author pointed out that when a bar is subjected to tensile stress the elements of the bar are distorted by the resulting shearing stresses which attain maxima in planes at 45° to the axis. If the bar be supposed to be divided into elements such as P or Q (see Fig.), then if the shearing strains are equal in the two directions



parallel to the sides of the elements, the bottom points of the strained elements will be directly below their top corners, whereas if the strains be different in the two directions there will be a displacement to one side or the other depending on which side the greater strain occurs. Since each inclined section is subject to the same total shearing force, the shearing strain along any section such as EF may be taken as inversely proportional to the length of the line EF, the bar being supposed of uniform thickness. From these considerations it may be seen that an element P will be subject to equal strains, for AB = CD, hence the lower point of P will remain vertically below its upper point. In this region, therefore, a horizontal straight line drawn

on the unstrained bar will remain horizontal and straight on the strained bar. An element at Q, however, will be subjected to unequal strains, for EF is $<$ GH, hence the lower points of the elements will be displaced towards the axis. This displacement will increase as the distance beyond d and e from the axis increases, and an originally horizontal line will become curved at the ends cd and ef, whilst de will remain straight. In a similar way it was shown that horizontal lines should assume the shapes indicated at *ghi*, *jklmn*, *opq*, *rst*, and *uv*, in their respective positions, whilst vertical lines should become pinched inwards above and below the shoulder as shown by the curve *wxx'yz*. To test whether the reasoning, by which the above conclusions were arrived at, was satisfactory, a copper bar was carefully prepared, ruled, and subjected to permanent strain. The curvatures of the various lines clearly show the characteristics predicted by theory. Prof. Perry inquired whether it was correct to assume the stress uniform over the plane sections inclined at 45° to the axis. He also said that the general character of the flow somewhat resembled that of a viscous fluid passing from a wide to a narrower channel. Prof. Herschel thought Mr. Carus-Wilson justified in assuming the stress uniform over the diagonal sections; the latter said he only made the assumption as a provisional hypothesis, but the results of his experiment agreed so closely with his theoretical deductions that he thought the hypothesis correct.—Mr. C. V. Boys made two communications, (1) on photographs of rapidly moving objects, and (2) on the oscillating electric spark. A collection of apparatus by which he had been able to photograph drops of water in their various stages of formation was exhibited. It consisted of a lantern and lenses by which a trough in which the drops were formed could be strongly illuminated, combined with a camera and revolving disk with one perforation. By this means exposures of about 1/600 of a second could be made about 20 times a second. The slide of the camera was about 3 feet long, and could be moved across the field by hand so as to take the consecutive impressions on different parts of the plate. The resulting photographs show with remarkable clearness the formation, breaking away, the oscillations of the drops, and their rebounding in the liquid into which they fall. By cutting the photographs into strips, each strip representing a single exposure, and mounting them on a disk, Mr. Boys had arranged a kind of thaumatrope which represented the phenomena in a very realistic manner. He also exhibited photographs of small water fountains broken up into drops by musical sounds, which he had taken by the electric spark without the aid of lenses. The shadows of the drops were sharply defined even when magnified considerably, and the various stages of transition from the liquid column to the detached particles were well shown. Finding it possible to obtain such good results from a simple spark, it occurred to him that he might get a succession of photographs from the intermittent light of an oscillating spark, and in this he was fairly successful. An apparatus devised to show the oscillatory character of a discharge was next exhibited in operation. It consisted of a disk carrying six lenses arranged in two sets of three. The members of each set were at different distances from the axis so that the images of the spark on the screen do not coincide. The disk can be revolved at a high speed, and the successive sparks are seen as bright patches on the screen. By this apparatus a single discharge can be examined, whereas with Dr. Lodge's apparatus it is desirable to have a fairly rapid succession of sparks. Photographs of an oscillatory discharge taken with the apparatus were exhibited, and these show that the duration of the illumination is a considerable fraction of a complete period. Lord Rayleigh said he was greatly interested by Mr. Boys's apparatus. He (Lord Rayleigh) had photographed water fountains both when broken up, and when made to coalesce under electrical influence, but it had never occurred to him that it would be possible to get enough light or sufficient sharpness from a single spark. Mr. Boys's success he believed to be owing to the fact of his using no lenses, which would absorb the ultra-violet rays. He also thought the method might be developed so as to give shaded pictures instead of mere representations in black and white. Mr. Gregory asked Mr. Boys if he had tried to get greater potentials for his oscillatory discharges by using Dr. Lodge's "impulsive rush" arrangement. Mr. Trotter inquired whether the single sparks used to photograph the water fountains were as large as those required to show oscillations. Mr. Boys said he had not tried Dr. Lodge's "impulsive rush" arrangement because of the enormous capacity of the condensers required. The sparks used to photograph the broken up fountain were very small, being only about ¼ inch long, and from a few jars. Prof. Perry asked Lord Rayleigh whether it would be possible to compare the shapes of the water drops shown in the photographs with the shapes of the liquid surfaces of revolution given by Sir William Thomson at the Royal Institution some years ago, or whether the changes of shape were too rapid to permit of the surface tension being all important. Mr. Boys thought the motions of the drops would be too rapid, and that inertia would play an important part. Lord Rayleigh pointed out that by forming a drop slow enough the effect of inertia might be made negligible until such time as the unstable state was reached; after that, however, inertia must have considerable influence on the shape.

Geological Society, April 30.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On certain physical peculiarities exhibited by the so-called "raised beaches" of Hope's Nose and the Thatcher Rock, Devon, by D. Pidgeon.—The Devonian rocks of South Devon, by W. A. E. Ussher, of H.M. Geological Survey. This paper is the result of work done in continuation of the labours of the late Mr. Champernowne, and refers particularly to the area north of the Dart and east of Dartmoor. Owing to the complicated stratigraphy of the region, we have to fall back upon such information as can be procured of the general types of Upper, Middle, and Lower Devonian faunas; for though the lithological constituents of these three divisions are broadly distinguishable, there are no definite lithological boundaries between them. The Lower Devonian is mainly distinguished by the occurrence of sandstone and grit, but the upper beds are shales passing into the Middle Devonian slates. The Middle Devonian consists of limestones, and shaly limestones upon slates, the latter representing the Calceolen-Schiefer, and containing *Spinifer speciosus*. *Stringocephalus* is found here and there in the middle Devonian Limestones. The upper part of the middle Devonian Limestones (with Lummaton fauna) passes into the *Cuboides* beds of the Upper Devonian. The Upper Devonian contains thin-bedded limestones, often concretionary, with chocolate-red and pale greenish slates and mudstones. These beds correspond to the Goniatiten-Schichten, Kramenzelstein and Knollenkalk of Germany, and to the Cypridinen-Schiefer. In the Upper and Middle Devonian rocks we find a local prevalence of schalstein and tuffs, breaking up the limestones. The slate and sandstone type of Upper Devonian in North Devon appears to give place southward to a purely slate type, possibly accompanied by overlap of the Culm measures. The author groups the South Devon rocks under the following heads:—

Upper.	1. Cypridinen-Schiefer.
	2. Goniatite-limestones and slates.
	3. Massive Limestones.
Middle.	4. Ashprington Volcanic Series.
	4. Middle Devonian Limestones.
	5. Eifelian slates and shaly limestone.
Lower.	6. Slates and sandstones, generally red.
	7. Slates with hard grits.

After discussing the relationship of the Lincombe and Warberry beds and the New Cut Homalonotus beds, the author notes the discovery of *Pleurodictyum* by Mr. Whidborne in the railway cutting at Saltern Cove. He proves the Lower Devonian age of the Cockington beds and their correlation with the Torquay Lower Devonian by the discovery of fossils. He considers it probable, though not certain, that the main mass of Meadfoot beds is below the Lincombe, Warberry, and Cockington sandstones. The distribution of the Middle Devonian Limestones is described. *Stringocephalus* is found in limestones containing *Rhynchonella cuboides*. The upper parts of the limestone-masses (East Ogwell, Kingskerswell, Barton, Ilsham, &c.) may be Upper Devonian. The massive limestones may terminate abruptly or pass laterally into shales, and the whole mass of the limestones seems to be replaced by slates between the Yealmpton and Totnes areas. The commencement of the phase of volcanic activity which caused the accumulation of the Ashprington series is shown to coincide with the latest stage of Eifelian deposition, and the Ashprington series may represent continuous or intermittent vulcanicity up to a late stage in the Upper Devonian. North of Stoke Gabriel a mass of limestone seems to have been formed contemporaneously with the volcanic material on the immediate borders of which it occurs. Elsewhere the limestones are interrupted by local influxes of volcanic

material. The occurrence of other local developments of Middle and Upper Devonian volcanic rocks is described. The relationship of the Middle and Upper Devonian deposits varies. In some cases Upper Devonian shales may have been deposited against Middle Devonian limestones; in others there is a continuous development of limestone, the Middle Devonian limestones being succeeded by *Cuboides* beds, *Goniatite* limestones, and Knollenkalk. The local variations of these are described, and fossil lists are given. The Knollenkalk is shown to pass under *Entomis*-bearing beds ("Cypridinen-Schiefer"), which are described, though a detailed account of their relationship to the Culm-measures is reserved for a future occasion. After the reading of this paper, some remarks were offered by the President, Prof. T. Rupert Jones, Prof. Hughes, and the author.

Paris.

Academy of Sciences, May 12.—M. Hermite in the chair.—New lunar photographs by the Brothers Henry, of Paris Observatory, by M. Mouchez (see Our Astronomical Column).—On volume iii. of the "Annales de l'Observatoire de Nice," by M. Faye.—Experiments on the deformations undergone by the solid envelope of a fluid spheroid submitted to the effects of contraction; possible applications to dislocations of the terrestrial globe, by M. Daubrée. In order to obtain the necessary oblateness, spherical balloons of vulcanized caoutchouc, having disks of the same material affixed at the extremities of a diameter, were used. The disks gradually increased in diameter, so that the thickness gradually decreased from the poles in each hemisphere, and unequal pressure was exercised on the liquid contained in the balloon. The oblateness has been determined of various liquid spheroids, and the conditions of production of ridges and fissures similar to those exhibited in the earth's crust.—On the retardation of foliation in Provence during the spring of 1890, by M. G. de Saporta. The low temperature and the abnormal humidity having exercised a very sensible influence during the spring of this year on vegetation in the middle of France, the author has investigated the state of foliation at the beginning of May in a locality situated at Saint-Zacharie (Var), in the high valley of Huveaune, at an altitude of about 200 metres.—On an hydraulic instrument with a new model of turbine for the continued utilization of the power of rivers, by M. Paul Decœur.—The difference between the surface of the earth taken as fluid and that of an ellipsoid of revolution having the same axis, by M. O. Callandreau. It is shown that in the case of a supposed fluid earth the maximum depression for latitude 45° is 9·1 metres, which agrees with the value given by M. Helmert in his "Géodésie supérieure."—On surfaces possessing a train of geodetic conjugates, by M. C. Guichard.—On some particular cases of visibility of interference fringes, by MM. J. Macé de Lépinay and Ch. Fabry.—On undulatory transverse magnetization, by M. C. Decharme It appears from some experiments given that a continuous electric current traversing the length of a tempered cylinder of steel may become undulatory on account of the resistance which the molecular actions of the magnetic medium oppose to it.—A note by M. A. Witz describes a method of exploration of magnetic fields by tubes of rarefied gases.—On the double chlorides of iridium and phosphorus, by M. G. Geisenheimer. By heating in a sealed tube at 300° C. 1 gram of iridium hydrate with 10 grams of PCl_3 and 15 grams of PCl_5, and reheating the yellow crystalline mass obtained with $POCl_3$ to 250°, a body possessing the empirical formula $Ir_2P_3Cl_{15}$ is formed in fine clear yellow crystals. By appropriate treatment several other double chlorides are obtained therefrom. An acid corresponding to the body $Ir_2P_3Cl_{15}$ is the product obtained on evaporating an aqueous solution of the latter as far as possible. The analyses of the salts of this acid indicate that the formula for the double chloride above should be written $2Ir_2Cl_3, 3PCl_3, 3PCl_5$.—A note on a characteristic reaction of hydrogen dioxide, by M. G. Denigès. A 10 per cent. solution of ammonium molybdate in water added to its own volume of concentrated sulphuric acid gives with a few drops of hydrogen dioxide an intense yellow coloration.—On the existence of microlithic peridote in the andesites and labradorites of the ridge of the Puys, by M. A. Michel Lévy.—On the contact phenomena of elæolithic syenite at Pouzac (Hautes-Pyrénées), and on the transformation into *dipyre* of the felspar of the ophitic rock in the same bed, by M. A. Lacroix.—On the metamorphic rocks of Pouzac, by M. Ch. L. Frossard. These rocks, occupying a space of 1250 m. by 300 m., extending from the railway near Monloo, appear to have been principally modified by the syenite. The ophite of Palassou has hardly acted upon the surrounding rocks. The rocks of which the modifications are attributed by the author to the action of the syenite are in the state of a fragmentary breccia and are without trace of fossils or indications of stratification. They may be classed as—siliceous, hard compact argillaceous, amphibole, talcose, chloritic, limestone, and dolomite rocks.—On the organisms of nitrification, by M. S. Winogradsky. The author has succeeded in isolating the nitrifying microbe, and has found that neither its rate of multiplication nor its vigour of action is diminished by cultivation in a mineral solution quite devoid of organic carbon. The colourless microbe of nitrification is thus capable of a complete synthesis of its substance from carbonic acid and ammonia. This fact is in direct contradiction with the fundamental doctrine of physiology that a complete synthesis of organic matter only occurs in chlorophyll-bearing plants, under the action of luminous rays.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Elementary Statics, new edition: Rev. J. B. Lock (Macmillan).—Dynamics for Beginners, 3rd edition: Rev. J. B. Lock (Macmillan).—Capital and Interest: E. V. Böhm-Bawerk: translated by W. Smart (Macmillan).—First Lessons in Political Economy: F. A. Walker (Macmillan).—Sketches of British Sporting Fishes: J. Watson (Chapman and Hall).—Yachting Guide and Tide Tables, 1890: A. Thomson (Pall Mall).—A Guide to the Literature of Sugar: H. Ling Roth (K. Paul).—Nautical Surveying: Vice-Admiral Shortland (Macmillan).—La Géographie Zoologique: Dr. E. L. Troussart (Paris, J. B. Baillière).—American Economic Entomology, Part 1, The More Important Writings of Bishop — Walsh (Washington).—Picturesque Wales: G. Turner (Adams)—Practical Chemistry for Medical Students: S. Rideal (Lewis).—Catalogue of the Birds in the Provincial Museum, North-West Provinces, and Oudh, Lucknow (Allahabad).—Masken von Neu Guinea und dem Bismarck Archipel: Dr. A. B. Meyer (Dresden, Stengel and Markert).—Anoa Depressicornis (H. Smith): Dr. K. M. Heller (Berlin, Friedländer).—Abhandlungen und Berichte des Königlichen Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden, 1888-89: Dr. A. B. Meyer (Berlin, Friedländer).—Harpur Euclid, Books 5, 6, 11: E. M. Langley and W. S. Phillips (Rivingtons).—On Aphasia, or Loss of Speech, 2nd edition: Dr. F. Bateman (Churchill).

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THURSDAY, MAY 29, 1890.

THE LABORATORY OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

Reports from the Laboratory of the Royal College of Physicians, Edinburgh. Vol. II. (Edinburgh and London: Young J. Pentland, 1890.)

THE liberal spirit in which the laboratory of the Royal College of Physicians of Edinburgh is thrown open to workers in every department of biology that bears, however remotely, upon medicine is worthy of the highest praise. That the opportunity for research thus afforded has been appreciated is well shown by this record of the work done in the laboratory during the second year of its existence. Sixteen papers are included in the volume, many of them anatomical and gynæcological, some pathological, one morphological (on the stomach of the Narwhal), and others (while including the results of studies in the laboratory) in the main clinical. This very diversity renders criticism difficult. Taking a high critical standpoint and employing as a standard the volumes which emanate from laboratories devoted to one subject—the Reports of the Physiological Laboratory of University College, London, or the studies from the Biological Laboratories of Cambridge or of Owens College, for example—it would be easy to find fault, to indicate papers that ought scarcely to be included, and to discover the absence of any series of allied researches of high scientific value, such as might be expected to be turned out in some special field of work, were the laboratory already long established, and were it given up to one branch of science, rather than intended from the first to be of use for investigations in all branches of biology. Yet to judge the volume from such a standpoint would be unfair both to the promoters of the laboratory and to those working within it. Taking medicine alone—that is to say, as apart from surgery and gynæcology—its extent is so considerable, and the topics dealt with so varied, that all original investigations, even if of equally high practical value, cannot be of equal scientific import: when surgery and gynæcology are also included, it is yet more obvious that much of the work that is rightly performed in the laboratory, while capable of almost immediate application to clinical practice, will be of a nature that does not necessarily call for great powers of original research. Clinical importance equally with scientific value must determine the inclusion of articles in such a volume as this. Herein, indeed, lies the only valid criticism that can be directed against these reports: if they be published purely as evidence of the activity of the laboratory, they well fulfil their purpose; but it is a little difficult to see what other use they possess. From the very diversity of the investigations, the reports cannot be expected to rank as useful additions to the library of the specialist in any of the subjects treated; there is too much extraneous matter. The gynæcologist will reap little benefit from the latter half of the volume, the pathologist will fail to appreciate the niceties of frozen sections of the lower portion of the body, cut in different planes. If such reports are to be of value to other workers, rather than, as I have said, as evidence of activity, they must be issued in separate parts, and, what is of still greater importance, they must assuredly not be issued at regular intervals. Successful as the laboratory has been up to the present, it is impossible to manufacture always a definite quantity of original work per annum and to order, and if it is intended to publish so many hundred pages at the expiration of every year, then it is only to be expected that many of those pages will either be work not of the highest quality, or will be upon subjects incompletely matured.

Having said thus much, it is a pleasure to draw attention to the many excellent articles that appear in these reports. The investigation by Mr. Irvine and Mr. Woodhead (the late Director) upon the secretion of carbonate of lime, a continuation of that described in the last volume, is of great importance to morphologists as well as to pathologists. In their last paper these observers pointed out that birds can assimilate and secrete carbonate from other salts of lime, as, for instance, the sulphate, and they advanced the statement that coral animals have in all probability the same power. In this communication is described the process of shell formation in the crab. The crab can produce its shell if, in the artificial sea-water with which it is supplied, the chloride be the sole calcium salt present; and the carbonate which forms the basis of the shell is deposited, it would appear, by a process of dialysis within the chitinous upper part of the epithelial cells. In this process it is suggested that phosphoric acid acts as the carrier of the lime to parts where carbonic acid is being given off; that carbonate of lime is formed in such regions; and that the phosphoric acid re-enters the circulation. It is thus rendered easy to comprehend why it is that wherever dead or vitally inactive tissue exists in the body, in bone matrix, chitin, and foci of caseous or fatty degeneration, there lime is deposited.

It has been known since 1875 that glycosuria may be only apparent, and that the agent reducing oxide of copper in the presence of an alkali, after the administration of chloral hydrate, for example, is not a sugar. Schmiedeberg and Meyer, in 1879, showed that this substance is glycuronic acid. Dr. Ashdown contributes an excellent paper upon the differentiation of this substance from glucose. From his experiments he leans to the view that there is a distinct chemical process presided over by the renal epithelium, which has as its result the formation of glycuronic acid—morphia, chloroform, curare, or one of a number of other drugs, being present in the blood.

Mr. H. A. Thomson, in his paper upon "Tuberculosis of the Bones and Joints," gives what is perhaps the most complete *résumé* of the varieties of tubercular affection in these regions that has yet appeared in our language; following König, he emphasizes the bone-factor in joint tubercle, as opposed to the synovial membrane. Dr. Cartwright Wood's paper, upon "Enzyme Action in the Lower Organisms," deals in a most suggestive manner with certain points in the biology of the Bacteria. The action of the soluble ferments produced during the growth of micro-organisms, not only in directing and controlling the growth in various media, but, as each month at the present time is yielding further indications, in producing the symptoms of disease, is a subject which before all

others deserves the attention of the medical profession, and Dr. Wood's paper is of the greatest interest in this connection.

Of the gynæcological articles, undoubtedly the most important is that by Drs. Barbour and Webster, upon the "Anatomy of Advanced Pregnancy and of Labour." The opportunities afforded to these observers have fallen to no others either abroad or in this country, and they have employed them to the full. The illustrations to their paper, as indeed throughout the Reports, are excellent.

J. G. ADAMI.

ABSTRACT MECHANICS.

Leçons Synthétiques de Mécanique générale, servant d'Introduction au Cours de Mécanique Physique de la Faculté des Sciences de Paris. Par M. J. Boussinesq, Membre de l'Institut. Publiées par les soins de MM. Legay et Vigneron, Élèves de la Faculté. (Paris: Gauthier-Villars, 1889.)

THE following *Table des Matières* will serve to show the scope of this treatise:—

1e Leçon. But de la Mécanique physique. Notions cinématiques indispensables.

2e Leçon. Les deux principes fondamentaux de la Mécanique.

3e Leçon. Forme des équations du mouvement; ce qu'on entend en Mécanique par force, forces motrices, actions mutuelles, &c. Pesanteur.

4e Leçon. Énergie potentielle interne. Action moléculaire.

5e Leçon. Principes de la conservation des quantités de mouvement et de leurs moments, pour un système matériel indépendant ou sans relations extérieures.

6e Leçon. Principes des quantités de mouvement et des moments pour un système partiel; de leur application à la formation des équations de mouvement des corps.

7e Leçon. Idées générales sur les pressions.

8e Leçon. Raisons physiologiques et psychologiques des dénominations de forces, actions, tensions, &c., employées en Mécanique. Forces d'inertie et centrifuges.

9e Leçon. Principe des forces vives pour un système partiel. Travail des forces. Énergie interne.

10e Leçon. Suite de l'étude des forces vives et du travail; flux de chaleur; loi fondamentale de la Thermodynamique.

11e Leçon. Application du principe des forces vives aux mouvements visibles ou moyens locaux; rôles qu'y prennent le travail de déformation des pressions exercées sur les particules matérielles et l'énergie potentielle de pesanteur; &c.

Such is the interesting syllabus of the subjects lectured upon by the author; and it is melancholy to think what we have lost in the treatment and illustration of such a programme at the hands of Maxwell, as a sequel and amplification of his inimitable little "Matter and Motion."

But when we open these pages we find a great contrast before us, and a great disappointment. Hardly anything more is to be found here than the elementary *banalités* of pure mathematics, in the shape of the explanation of co-ordinates and their differential coefficients as employed in representing the motion of a particle, and thence of a rigid body considered as an aggregation of particles. There is no interesting illustration or application or even diagram; merely a sequence of simple formulas of pure mathematics, interspersed with some metaphysical speculation; it is the purest of mathematics even by the side of Lagrange's "Mécanique Analytique"; we are given plenty of Mathematics, but very little Mechanics. The words of the preface to the "Lectures in Natural Philosophy in the University of Oxford," A.D. 1700, by John Keill, Savilian Professor of Astronomy, appear to be applicable even at the present day: "Although nowadays the mechanical Philosophy is in great Repute, and in this Age has met with many who cultivate it, yet in most of the Writings of the Philosophers, there is scarce anything mechanical to be found besides the Name. Instead whereof, the Philosophers substitute the Figures, Ways, Pores, and Interstices of Corpuscules, which they never saw;" &c.

These "Leçons" are the first of a course of Physical Mechanics to be delivered at the Sorbonne, with the intention of solving the Universe; but so far the author does not appear in touch with the physical questions, and he derives his mechanical notions from words in preference to facts. No doubt this is an excellent discipline for some minds, but to the applied mathematician it is devoid of all flavour.

The note to p. 34 we have found the most interesting passage in the book, pointing out that g varies with the position of the sun and moon, but that the variation would be imperceptible but for the tides.

The French have the advantage of possessing the two words *poids* and *pesanteur*; much of our own dynamical confusion would be cleared up if we had a separate word equivalent to *pesanteur*, something like *gravity*, or *gravitation*, as proposed by Thomson.

The word *force vive* for mv^2 is still allowed to appear in these pages, in spite of all the recent efforts of Thomson and Tait, Maxwell and recent writers to banish it to oblivion; this is carrying reverence for Lagrange too far for modern progress.

Maxwell's "Matter and Motion" practically covers the same ground as these "Leçons," and the two books compared would offer the best idea of the difference between the teaching of abstract Mechanics in this country and in France.

A. G. G.

OUR BOOK SHELF.

A Manual of Anatomy for Senior Students. By Edmund Owen, M.B., F.R.C.S. (London: Longmans, Green, and Co., 1890.)

THIS manual has been written from a point of view different from that usually adopted in anatomical text-books. Instead of giving a detailed systematic or topographical description of the whole of the organs or parts of the body, the author has selected those regions or structures which have a special reference to medical and surgical practice, and he has described them as fully as is necessary to bring out the points which have to be considered and attended to by the practitioner. In making his selection, he has not limited himself to a description of those parts or arrangements which are characteristic of adult structure, but he has incorporated in his book an account of such developmental anomalies as are sometimes

observed in infancy and childhood, and regarding which anxious parents require the advice and assistance of the surgeon. But, although written by a surgeon, the manual is not confined to what is commonly called "surgical anatomy." The needs of the physician have been consulted, and the position and boundaries of the heart, the lungs, and the great viscera of the abdomen have been described and illustrated by appropriate diagrams. In many instances the author seeks to give an explanation of the symptoms produced by disease of the viscera by a reference to the anatomical relations and connections of the parts.

The marvellous progress which operative surgery has made of late years is illustrated by several chapters in this manual. The antiseptic system of treatment, devised by Sir Joseph Lister, has rendered possible the performance of many operations which would not have been thought of fifteen or twenty years ago. The brain, the spinal marrow, the great serous cavities of the body, and the larger joints, are now with safety made the subjects of operative interference. No treatise on applied anatomy therefore would now be considered complete unless it embraced an account of these parts in their surgical relations, and Mr. Owen has furnished his readers with the necessary information.

The mode of treatment necessitates on the part of the reader some preliminary knowledge, so that the book is not intended for the beginner, but for the senior student and the practitioner.

Advanced Physiography. By John Thornton, M.A. (London: Longmans, Green, and Co., 1890).

THIS is a continuation of the same author's "Elementary Physiography," and, to quote the preface, "It carries the student into the wider realms of Nature, and treats of advanced physiography as defined by the Science and Art Department. Whether physiography be regarded as a separate science or not, it cannot be denied that, as thus set forth, it includes a fairly well-defined and well-ordered series of facts connected with the study of the universe." This is, perhaps, the first really serious attempt which has yet been made to give anything like a full account of the whole subject, and we have no doubt that teachers will find it convenient to have all the parts thus brought together. The author has very wisely quoted the best authorities, a proceeding which is far preferable to mere paraphrase. The book is thus largely a compilation, but it is only fair to say that full acknowledgment is made in nearly every case.

Most of the important astronomical instruments are described in considerable detail, and the fundamental notions of astronomy are clearly explained. The chapters on the so-called "new astronomy" are exceptionally good for a work of this class, and it is quite evident that the author has carefully followed the latest researches. Vogel's work on the orbit of Algol and Schiaparelli's new rotation period for Mercury are included, though only recently published. There is also an excellent summary of the work which has been done in celestial photography. The chapter on the sun is very detailed, and considers all the important facts and theories. No attempt is made to discuss any disputed points—a commendable feature in a school text-book.

There are apparently few mistakes, but one is of sufficient importance to be referred to. On p. 249 it is stated that the dark bands in stars like α Herculis are probably due to carbon absorption; this ought to read metallic fluting absorption, the *bright* flutings being probably due to carbon.

The book is profusely illustrated, but most of the diagrams have already seen service. The drawing of the Orion nebula is perhaps the least satisfactory. The large coloured plate is instructive, but there is a curious mistake. This has probably arisen from the fact that the plate is compiled from those which have appeared in the last two editions of a well-known text-book of astronomy, one of which was on a scale of wave-lengths, and the other on a prismatic scale. The flutings of carbon have evidently been transferred from one to the other without the necessary corrections, the result being that they are quite out of place relatively to the other spectra.

We can confidently recommend the book to all interested in the subject, whether for examination purposes or for the purpose of acquiring fairly accurate information as to the present state of our knowledge of the Earth's place in Nature.

An International Idiom: a Manual of the Oregon Trade Language, or "Chinook Jargon." By Horatio Hale, M.A., F.R.S.C. (London: Whittaker and Co., 1890.)

IN the district formerly called Oregon, which is of much wider extent than the State of Oregon, a sort of international language, known as the Chinook jargon, is current among the native tribes and white traders. It grew up about the beginning of the present century, and has been of great service not only in facilitating commerce, but in stimulating friendly intercourse between tribes who, if this strange speech had not existed, would have had no means of communicating with one another. Many of the words are of Chinook origin, but contributions have also been drawn from French and English, and various words have been formed by onomatopœia. In 1841, when connected with the United States Exploring Expedition which surveyed a part of the western coast of North America, Mr. Hale had occasion to study the Chinook jargon; and he has since taken pains to make himself acquainted with information brought to light by later investigators. In the present little volume he gives a full account of the subject, describing the origin and history of the "idiom," and presenting a grammar and dictionary, with specimens of colloquial and narrative phrases, songs, hymns, and a sermon. The facts he has brought together are of considerable scientific interest, and the book ought to be useful to travellers and settlers in the North American Pacific States and Provinces.

A Class-book of Geography, Physical, Political, and Commercial, for Intermediate and Senior Pupils. By W. B. Irvine, B.A. (London: Relfe Brothers, 1890.)

THE compilation of this volume must have cost the author a good deal of hard work, but we cannot say that the result seems to us satisfactory. The subject is treated in an extremely uninteresting way, and the appearance of the pages, with their short, jerky paragraphs and masses of disconnected facts, might alone suffice to deter many boys and girls from the study of geography. In the teaching of this subject almost everything depends on the intelligence and skill of the teacher; so that even the present work, in good hands, might be made the basis of instructive and useful lessons. But the book would increase rather than diminish the difficulties in the way of teachers who have no exceptional degree of ability or knowledge.

LETTERS TO THE EDITOR.

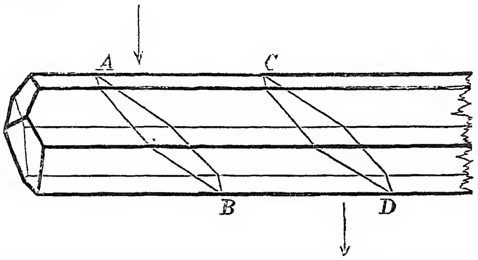
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Idiocyclophanous Crystals of Calcite.

IT seems to have escaped notice that one at least of the many crystal-forms of calcite can be induced to show its ring-system—can be made, in fact, into a Bertrand prism—simply by proper cleavage, without any artificially-worked planes at all.

Hexagonal prisms of the mineral are of frequent occurrence; good, clear, regular-shaped specimens, 1 cm. (or more) in diameter, coming especially from Cumberland. The sides of these prisms are, of course, parallel to the optic axis; and hence a pair of opposite sides, if smooth and well developed, serve for two of the plane surfaces required for the Bertrand prism (see NATURE, May 15, p. 52) without any alteration. Moreover the crystal has a very strongly marked cleavage along the planes of the fundamental rhombohedron; and since these planes make angles of almost exactly 45° with the optic axis, a pair of them will supply the two remaining surfaces of the prism.

If, then, we select a good hexagonal prism of calcite, as shown below—



and carefully cleave it in two places, AB and CD, and allow a beam of ordinary light (preferably from an opal lamp shade) to enter the side of the prism near A, it will be affected in its passage through the crystal in the manner explained in my former letter (*ante*, p. 53), and the usual pair of ring-systems will be visible to an eye receiving the light emergent near D.

If there are any slight imperfections in the natural plane surfaces, we can easily improve upon Nature by cementing thin plates of glass upon them with Canada balsam.

Queen's College, Oxford. H. G. MADAN.

Testing for Colour-blindness.

MAY I ask, in connection with the lecture of Mr. Brudenell Carter, why those interested in the testing of colour-vision do not avail themselves of a scientific instrument like Lord Rayleigh's colour-box, wherein a given yellow has to be matched by proportions of red and green adjusted by turning a handle over a dial graduated on the back; instead of contenting themselves with crude methods, such as selection of coloured wools, which cannot give results definite enough to be of much interest, even if they were quite efficient in detecting the grossly colour-blind?

OLIVER J. LODGE.

IN Mr. Brudenell Carter's interesting paper read at the Royal Institution on Friday, May 9, a physiological explanation is suggested of an easily-verified fact of colour-perception.

An eye with the pupil dilated is proportionally more perceptive of red rays than one with the pupil contracted. If we stand at right angles to a window, or other light, shading the further eye with the hand, and look at a piece of white paper with the two eyes alternately, we shall find that, to the eye in the light, the tint of the paper seems distinctly colder than to the shaded eye.

Dr. Waelchli's observations of the retinal red zone in birds, surrounding the central green region, makes the cause of this phenomenon comparatively clear; while the phenomenon itself tends to prove that the distribution of colour-zones in the human eye resembles that of birds. E. H.

May 16.

I DO not know whether the following will be considered too trivial to be admitted as an illustration of Dr. Brudenell Carter's lecture.

About 30 years ago, being then an assistant master at a school, I one day asked an older colleague (since dead), "Who's that boy in the red cap?" Several were standing in a line, most of them with black caps. "Red cap?" said my friend; "I don't see any red cap" (it was scarlet flannel); "I can see the red head"—meaning another boy with so-called "red" hair.

The same gentleman could see no difference in colour between the flower and the leaf of the *Pirus japonica*; but it will be observed that he distinguished "red" hair without difficulty.

Otham Parsonage, Maidstone. F. M. MILLARD.

Red Spot on Jupiter.

ON May 22 last, at 15h., I observed Jupiter through a 10-inch reflector, power 252, and saw the red spot between the east limb and centre of the planet. It was rather faint, and not nearly so conspicuous as some of the belts in its vicinity. According to careful estimation, the spot reached the central meridian of Jupiter at 15h. 35m. This is 15·5 minutes before the time given in Mr. Marth's valuable "Ephemeris for Physical Observations of Jupiter," published in the *Monthly Notices* for March 1890, p. 344. The difference proves that the motion of the spot continues to accelerate in a slight degree relatively to the mean rate of rotation of 9h. 55m. 40·63s., adopted by Mr. Marth in his recent ephemerides. Observers, therefore, who wish to see the spot at mid-passage across the disk of Jupiter must turn their telescopes upon the planet at least a quarter of an hour before the predicted times based on the daily rate 870°·27.

During my observation on May 22 I saw a very dark, if not black, spot of circular form threading its way across the disk, and projected upon the northern half of the great north equatorial belt. I ascertained by reference afterwards to the *Nautical Almanac* that this object was the fourth satellite in transit.

Bristol, May 25. W. F. DENNING.

Coral Reefs, Fossil and Recent.

I PUBLISHED a review of the third edition of Darwin's "Coral Reefs," with appendix by Prof. Bonney, both in the *Biologisches Centralblatt* and in the *Naturwissenschaftliche Rundschau* of last year. This will, I think, considerably modify the meaning of Prof. Bonney's statements, published in NATURE of the 15th inst. (p. 53), according to which I was ignorant of this work of his when I wrote my letter on "Coral Reefs, Fossil and Recent." R. VON LENDENFELD.

Swallows at Sea.

THE following notes from my journal as to meeting swallows at sea during the autumn migration may be of use to anyone interested in that subject.

On board ss. *Port Victor* from Plymouth to Sydney, 1889.—"October 31, lat. 16° N., long. 19° W. A quantity of swallows flying about the ship evidently tired and very tame, perching freely within a few feet of anyone. Look thin. A solitary swallow or two were seen yesterday and day before in lats. 20° and 24° N. respectively. 9 p.m., passed Cape Verde at a distance of forty miles.

"*November* 1.—The swallows roosted last night on board, and left during the forenoon, with the exception of one or two who remained on board all day. Saw a curlew and a wagtail at noon, lat. 12° N., long. 18° W.

"*November* 2.—One swallow still on board, roosted last night on the poop. Calm. Saw a wagtail.

"*November* 3.—Five swallows and two martins about the ship, apparently in very fair condition, so tame that they would perch on one's hand; had three or four sitting on my hand at once sometimes. Noon, lat. 5° N., long. 14° W., about 150 miles off land.

"*November* 4.—Swallows left, could not see in what direction they went. Picked up south-east trade, lat. 2° N."

HERBERT E. PUREY-CUST.

H.M.S. *Egeria*, Auckland, April 6.

The Corolla in Flower-Fertilization.

I HAVE noticed a curious fact in reference to the blue gentian of the garden here that will interest you. This flower (like the daisy) closes at night and opens in the morning, and is exquisitely sensitive to the time of sun rising and setting (it is a lasting, and with its bronzed throat an exquisite flower). The fact observed is this, that, when visited by the large handsome bee that fertilizes it, the beautiful widespread pentamerous flower closes gently on the bee, if the insect effectually enters and fertilizes it, on its passage to the honey of the five cups at the base of the corolla; and after the insect's exit, does not again unfold, if the fertilization is complete, but remains a folded flower—a protection (shorn of its beauty) for the precious seed-vessel and its maturing contents within. JOHN HARKER.

Hazel Grove, near Carnforth, May 13.

Popocatapetl.

IN vol. xli. of NATURE, (p. 592) you state: "Despatches from Mexico state that observations show that the height of the active volcano Popocatapetl has decreased by 3000 feet since the last measurement." This despatch, which was forwarded from Prof. Heilprin's party now in Mexico, would seem to indicate that there has recently been an actual *loss* of height in Popocatapetl; whereas Prof. Heilprin's object was to indicate that the observations hitherto accepted are inaccurate.

EDMUND J. DE VALOIS.

295 Adelphi Street, Brooklyn, N.Y., May 16.

CHEMICAL CHANGES IN ROCKS UNDER MECHANICAL STRESSES.[1]

AFTER pointing out that his object was to inquire how far the experimental researches of chemists and physicists are capable of affording a satisfactory explanation of the phenomena observed when the rocks of the earth's crust are studied microscopically in thin sections, the lecturer proceeded to give a *résumé* of the experimental investigations of Daubrée, Bunsen, Sorby, Thorpe, Spring, Guthrie, Fouqué, Michel-Lévy, and other chemists, who have devoted their attention to the action of pressure in influencing chemical affinity. The evidence that the deeper-seated rock-masses of the globe, and those constituting mountain-chains, have been subject to enormous pressures was then indicated; and the difference between the statical pressures arising from a great weight of superincumbent rocks, and the dynamical pressures resulting in actual movements within the earth's crust, was insisted upon. The chemical and physical principles which have been established by direct experiment, and which, at the same time, appear to be illustrated by the observations that have been made during recent years upon the minute structure of rocks, and of the minerals composing them, were stated in the following series of propositions:—

I. *In all those cases in which crystallization is accompanied by contraction, the tendency of pressure is to promote the change from an amorphous to a crystalline condition.*

Spring has shown that under a pressure of 6000 atmospheres plastic or amorphous sulphur, having a density of 1·95, passes into rhombic, crystallized sulphur, having a density of 2·05.

The mixtures of silicates which constitute the igneous rocks of the earth's crust all undergo *contraction* in passing from the amorphous (vitreous) to the crystalline condition. This is easily proved by comparing the specific gravities of more or less crystalline rock-masses with that of the glasses formed by their artificial fusion. The experiments of Delesse, Deville, Cossa, and others have shown that mixtures of the silicates of alumina and the alkalies with over 70 per cent. of silica, must undergo a contraction to the extent of $\frac{1}{11}$ of their bulk in passing from a glassy to a highly crystalline state (granite). Mixtures of the silicates of alumina, magnesia, iron, lime, and the alkalies with less than 50 per cent. of silica, in passing from a vitreous state to a perfectly crystalline one (gabbro), must undergo a reduction in bulk equal to $\frac{1}{6}$.

It may fairly be anticipated, therefore, that great pressure would tend to promote the crystallization of the mixtures of silicates composing most of the rocks of our globe, or to prevent their assuming the glassy state; and a great body of geological facts tends to support this conclusion. It must not, of course, be lost sight of that slow consolidation is also favourable to the process of crystallization, and rocks being extremely bad conductors, the process of cooling in great rock-masses is excessively slow. It is often difficult therefore to discriminate between the effects that must be referred to slowness of cooling, and those which may be safely considered to result from pressure.

As long ago as 1846, Charles Darwin showed that the andesitic lavas of the Cordillera of South America are associated with perfectly crystalline rock, true granites, made up of precisely the same minerals. The identity of the minerals in the plutonic rocks and the lavas respectively was demonstrated by the careful studies of Darwin himself, and of Prof. W. H. Miller, of Cambridge, long before the method of studying rocks in thin sections had been invented. Quite recently Prof. A. Stelzner, employing the modern methods of research, has been able to completely confirm the interesting results arrived at by Darwin and Miller, and to show that a perfect gradation can be traced between the highly crystalline "Andengranites," and the more or less glassy lavas (andesites) which are so closely associated with them.

In 1874 I was able to show that in the Western Isles of Scotland there occurred masses of perfectly crystalline (granitic) rock, identified by Zirkel as true gabbros and granites, which can be traced passing by the most insensible gradations into natural glasses ("tachylytes" and "obsidians") (Quart. Journ. Geol. Soc., xxx., 1874, 233–48), and the truth of these conclusions has been fully established by the more recent researches of Dr. A. Geikie (Trans. Roy. Soc. Edinb., 1888, 122–24, 145–50). In 1876 I further showed that the diorites and quartz-diorites of Hungary and Transylvania pass insensibly into the ordinary lavas of the district, which have the same ultimate chemical composition, and the same mineralogical constitution (Quart. Journ. Geol. Soc., xxxii., 1876, 292). In 1885, Messrs. Arnold, Hague, and J. P. Iddings, of the United States Geological Survey, established precisely similar conclusions by the study of rocks in the Nevada district (Bull. U.S. Geol. Surv., No. 17, 1885); and Signor B. Lotti, of the Italian Geological Survey, in the following year proved the same to be true in the case of the rocks of Elba.

In all these cases it is seen that the masses which have been most deeply seated, and thus subjected to the greatest statical pressures, are those which have undergone the most perfect crystallization. It must of course be remembered that in these cases the other cause tending to the development of crystalline structure comes into play—namely, slowness of cooling. The ordinary materials of igneous rocks are such bad conductors of heat, that enormous periods of time must elapse before the deeply seated portions of igneous rock-masses can become solidified.

The potent influence of this extreme slowness of cooling in bringing about the crystalline structure in molten masses of silicates has been well illustrated by the splendid researches on rock-synthesis by MM. Fouqué and A. Michel-Lévy. They have shown that the secret of making a particular mineral crystallize out of such a mass consists in finding out the temperature of fusion of the mineral, and in maintaining the molten mass for a long period just below this temperature. In the excessively slow cooling of deeply seated rock-masses, the materials must be kept successively and for long periods at temperatures a little below the fusion-points of each of their mineral constituents.

But while the influence of slow cooling in producing the crystalline structure in rocks is unquestionably very great, the effect of pressure in promoting crystallization can scarcely be doubted. We have no proof, indeed, that the holocrystalline or perfectly granitic structure of rocks can ever be produced except under these conditions of extreme pressure.

II. *Crystallized minerals, developed in a magma under pressure, may lose their stability and be dissolved by the same magma when the pressure is removed.*

The very remarkable researches of Fouqué and Michel-

[1] "The Evidence afforded by Petrographical Research of the Occurrence of Chemical Change under Great Pressure." A Lecture delivered before the Chemical Society, March 20, 1890, by Prof. J. W. Judd, F.R.S.

Lévy upon the synthesis of rocks is not less instructive, whether we consider the successes or the failures of their experiments. While able to reproduce by fusion and slow cooling—either from the powdered rocks themselves, or from duly admixed proportions of silica, alumina, iron oxide, and the alkaline earths and alkalies—various kinds of basalts and other basic rocks, all attempts to form certain other rocks, especially those containing quartz, hornblende, and muscovite, failed. The conclusion at which the experimenters arrive—and the correctness of this conclusion it is scarcely possible to doubt—is that, for the formation of such minerals and of the rocks containing them, water and other volatile substances, held within the solid mass by intense pressure, is absolutely indispensable.

Now in the porphyritic constituents (*Einsprenlinge* or phenocrysts) of many lavas, we find examples of minerals which have been formed at great depths in the earth's crust and then brought up to the surface and exposed to totally different conditions, especially as regards pressure. Very clearly do these phenocrysts tell the tale of their origin, and of the influence exerted upon them by their subsequent environments.

Crystals of quartz and felspar, which have grown to large proportions in the deeper portions of the earth's crust, are found when brought up in lavas to the earth's surface, and thus relieved from the action of pressure, to be attacked by the magma in which they were originally formed. The proof of this is seen in the corroded condition of the crystals, the glassy matter surrounding them having attacked their angles, their edges, and in a less degree their whole surface, penetrating irregularly into their interior, and reducing them sometimes to mere skeletons.

Crystals of hornblende and mica betray in an even more striking manner the effects of a change of environment. When brought up from great depths in masses of molten lava, crystals of these minerals are constantly found to be surrounded by "resorption halos." The outside of the hornblende or mica crystals, where in contact with the molten glass, is found to be attacked by it, and crystals of pyroxene and magnetite have resulted from the reaction. The action may in some cases continue till the whole of the hornblende has been converted into a pseudomorph.

In some instances there may be reason to believe that the phenocrysts have become enveloped in a magma of different chemical composition to that in which they were originally formed. But in many cases there is no room for doubt that the minerals which were formed and maintained their stability under certain conditions of pressure, lost that stability upon the diminution of pressure.

That, conversely, the increase of pressure leads to the production of a condition of instability in minerals formed at or near the earth's surface there cannot be any doubt. The study of the formation of crystalline schists from various aqueous and igneous rocks supplies us with numerous and very interesting illustrations of changes of this kind: hornblendes, chlorites, micas, and talc are produced under conditions of pressure in which pyroxenes, epidotes, felspars, and olivines lose their stability.

III. *In all those cases where solution is attended by contraction, the solvent action of water and other liquids is increased by pressure.*

That this is the case at elevated temperatures is proved by the researches of Daubrée to which we have already referred. Pure water was made to attack various silicates quite insoluble at ordinary temperatures and pressures. Even if we admit with Bunsen that there are temperatures at which this influence of pressure is no longer operative, or at which the effects are wholly inappreciable, the admission would not in any way affect the theoretical views of the geologist, seeing that the increase of temperature within the earth's crust is so rapid, that even at moderate depths the temperature at which solvent action is increased by pressure must certainly exist.

The *effects* of this solvent action under pressure are everywhere manifest when we come to the study of the rocks building up our earth's crust. At more or less considerable depths, water containing carbon dioxide has attacked the silicates composing the rock-forming minerals; so that it is impossible to find rocks which have been deep-seated, at any period of their history, in which the minerals are in a perfectly unchanged condition.

Great masses composed originally of calcic carbonate, are found to have been changed into dolomite (the magneso-calcic carbonate), or into chalybite (the ferrous carbonate); while in other cases the whole mass of a bed of calcic carbonate has been dissolved away, and silica substituted as a "pseudomorph."

We must proceed to study the details of such processes especially as they are affected by pressure and by the crystalline structure of the minerals affected.

IV. *Under great statical pressures, the whole substance of solid bodies may be permeated by fluids, and chemical reactions between them are thus greatly facilitated.*

It is not necessary to point out that the molecules of the densest solids cannot be in actual contact; this is proved by the circumstance that such solids undergo contraction by lowering of temperature, and that gases may be occluded in them. Physicists and mathematicians, as recently pointed out to this Society by Prof. Rücker, have even been able to arrive at positive conclusions concerning, not only the actual order of magnitude of molecules, but the *distances* that separate them from one another in solids.

The effect of pressure in causing the molecules of one body to pass between those of another, has been expressed by Van der Waals in the dictum, "All bodies can mix with one another, when the pressure exceeds a certain value." A similar conclusion was expressed by the late Dr. Guthrie, as the result of his experiments on potassic nitrate, when he asserted that "fused nitre and fused ice are miscible in all proportions."

Now, nothing is more certain, from petrographical researches, than that the whole substance of the minerals in the deep-seated rock-masses of the globe may be permeated by fluids. This is shown by the condition of the minerals forming these deep-seated masses.

The felspars, in their normal condition, are colourless and transparent minerals with a vitreous lustre, and this is their character when they are found in lavas and in blocks ejected from volcanoes. In granites and other deep-seated rocks, however, these same felspars exhibit grey, green, pink, or red tints, with more or less opacity, and a remarkably pearly lustre. The cause of this change of aspect is found in the fact that the unstable alkaline silicates which enter into their composition have been attacked by the fluids that have penetrated through the whole substance of the crystal, leading to the formation of the hydrated silicates of alumina, and, in some cases, the peroxidation of any traces of iron compounds that may have been present in them.

Similar changes can be shown to have affected most, if not all, the minerals which, at any period of their history have formed portions of deep-seated rock-masses.

V. *By the intimate intermixture, under great statical pressures, of solids and fluids, the properties of the former undergo great modifications.*

Bunsen, in common with all chemists who have studied the great problem of geology, has insisted that fused silicates, in spite of the high temperatures at which they assume the fluid state, obey the same laws as those governing ordinary solutions. Guthrie has shown that the principles which determine the formation of "cryo-

hydrates" and of "eutectic compounds" are equally operative in the case of the separation of minerals from a mixture of fused silicates; and the same idea has been elaborated by Lagorio. Guthrie has further shown that, as water is added to a salt, the fusion-point of the mixture is progressively lowered, and from this fact he concludes that "the phenomenon of fusion is nothing more than an extreme case of liquefaction by solution."

That silicates, when they are mixed with water, fuse at a lower temperature, was long ago recognized by geologists—long, indeed, before any physical explanation had been offered of the fact. Poulett-Scrope, Scheerer, Elie de Beaumont, Daubrée, and many others who might be mentioned, have insisted on the important part played by water in promoting the fusion of lavas and other igneous masses.

In the case of the volcanic glass known as *marekanite*, I have shown that at a comparatively low temperature the mass will, when heated, swell up and intumesce, the escaping steam causing the molten glass to froth up and assume the character of a true pumice (*Geol. Mag.*, Dec. 3, iii. 243). The brown glass ejected from Krakatão, during the great eruption of 1883, if heated, increases to many times its original bulk, and passes into a substance which, macroscopically and microscopically, is indistinguishable from the pumice thrown out in such vast quantities during that great eruption (*Geol. Mag.*, Dec. 3, v. 6).

Many volcanic glasses contain an appreciable quantity of water, amounting in some cases, indeed, to as much as 10 per cent. of their mass. The glasses which contain water fuse at a lower temperature than those which are anhydrous. There is reason to believe that most lavas are not masses in a state of simple fusion, but consist of crystals floating in a mass of mixed silicates and water, the magma being at a temperature above the fusion-point of the mixture but below that of the crystals.

VI. *Mechanical stresses, which tend to overcome the attraction between the particles of a solid, promote chemical action at those parts of its mass which are in a condition of intense strain.*

That a direct relation exists between mechanical and chemical forces is shown by the fact that capillary action is capable of overcoming weak chemical affinities. Violent mechanical shocks will sometimes completely overmaster chemical affinity, as was shown by Berthelot in the case of acetylene, cyanogen, &c., and more recently by Prof. Thorpe in the case of carbon disulphide.

Carnelley and Schlerchmann endeavoured to show that the solution of a copper wire by acid was promoted when the wire was put into a condition of strain. These experiments, it is true, yielded negative results, a circumstance which is, perhaps, hardly to be wondered at, when we remember how feeble were the mechanical forces employed.

In the case of the curiously impressed limestone pebbles of the Swiss Nagelflüe, however, Sorby has shown that there are grounds for believing that solution is promoted in masses which are subjected to intense mechanical stresses, and he has confirmed this conclusion by an ingenious experiment with rock-salt (Yorksh. Proc. Geol. Soc., iv. 458-61).

Similarly impressed and faulted pebbles from the Old Red Sandstone of Stonehaven, in Scotland, have afforded what I think is indisputable evidence of the action of strain in promoting solution. The sand-grains, of which these pebbles are composed, are seen under the microscope to be traversed by bands of liquid enclosures that are clearly of *secondary* origin. Now, these bands of enclosures are parallel to the actual faults that have been produced in the pebbles, and the careful study of all the facts renders inevitable the conclusion that when the whole mass, under great statical pressures, was permeated by fluids, solvent action was determined in parts of the mass subjected to violent strain (*Mineralogical Magazine*, vii. 83).

Similar bands of secondary liquid inclusions, which have clearly been produced in the same way, abound in the crystals of many rock-masses that have been subjected to strain and movement.

VII. *Pressure may supply the conditions required for the renewal of the growth of crystals when their development has been arrested for an indefinite period, and even after they have suffered mechanical injuries.*

In 1856, Louis Pasteur published the results of his interesting investigations upon the property exhibited by bimalate of ammonia and other salts, the crystals of which are able to repair injuries produced by fracture; and this experiment has been repeated and confirmed by Scharff and other observers.

This principle of the growth and repair of injured crystals is one of great importance and wide application in geological investigation. Sorby has shown that rounded and water-worn sand-grains that have originally constituted a portion of granite or other igneous rock may, in the presence of solutions of silica and under pressure, renew their growth, and, in the end, acquire the faces and angles characteristic of quartz-crystals The observations of Becke, R. Irving, Van Hise, Bonney, and other microscopists have shown that, not only fragments of quartz, but portions of the crystals of felspar, augite, hornblende, biotite, and other minerals, may undergo enlargement in a similar way. It has further been shown that this repairing and growth of crystals is continually taking place in rocks under pressure; that the composition of the outer parts of a crystal may vary as growth goes on; and that the action can take place in solid rock-masses (Quart. Journ. Geol. Soc., xlv. 175-86).

I have found it possible to illustrate experimentally some of the phenomena exhibited by zoned crystals in rocks. An octahedral crystal of chrome-alum of a dark-purple colour was mutilated by having two opposite solid angles broken off from it and then placed in a solution of common ammonia-alum (see Fig. 1). By more rapid

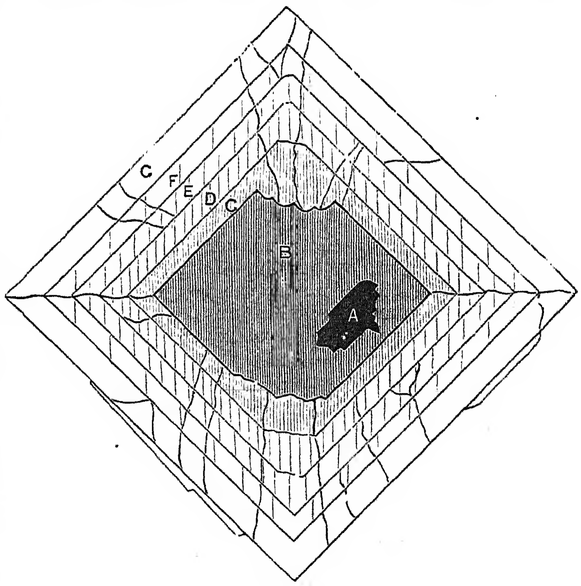


FIG. 1

growth in the injured portions, the crystal tended to repair itself, but the regularity of this process was interfered with by subjecting the crystal and solution to a somewhat wide range of temperature. As the coefficient of expansion of chrome-alum appears to be different from that of ammonia-alum, the shell of the latter material

was from time to time cracked by the unequal expansion. The solvent, finding its way into the interior, partially dissolved away the original crystal of chrome-alum. The final result of these changes was that while the form of the alum octahedron was almost completely reproduced, only a small portion (A) of the original chrome-alum crystal remained. Much of the chrome-alum was dissolved out and replaced by a mixture of the two alums (B), while the other layers of the crystal (C, D, E, F, G) were formed by still paler-coloured zones, also consisting of mixtures of a like kind. Zoned crystals exhibiting similar abnormal appearances to this alum crystal are by no means rare in some igneous rocks.

VIII. *When solution under pressure is going on in a crystalline body, the action is controlled and modified by its molecular structure. This molecular structure may have been produced either in the process of crystallization, or as the result of mechanical or other forces acting upon the crystal subsequently to its formation.*

Daniell's earliest contributions to science, in the year 1816, dealt with the remarkable and unequal action of solvents upon crystals. The curious and complicated patterns produced on the faces and the cleavage or cut surfaces of crystals (etching-figures) have subsequently been studied by Leydolt, Klocke, Baumhauer, Becke, and other investigators. The results obtained have been shown to vary with the nature and strength of the solvent, the temperature, the pressure, and the time during which the action is allowed to take place.

In 1884-85, Von Ebner, as the result of an exhaustive study of the etching-figures of calcite and aragonite, showed that crystals possess *planes of chemical weakness*, to which he gave the name of "solution-planes," these being analogous to the well-known planes of least cohesion or cleavage-planes. Quite independently, I, about the same time, arrived at the same conclusion by studying the crystals in deep-seated rocks (Quart. Journ. Geol. Soc., xli. 383, &c.). In these deep-seated rocks the crystals (their whole substance being permeated by the solvent) yield to chemical action along their solution-planes, along which hollow spaces in the form of negative crystals are produced.

When twinning-planes are developed in crystals by pressure or other mechanical agencies, these planes (gliding-planes) become planes of chemical weakness (*Mineralog. Mag.*, vii. 87). The experiments of Reusch, Baumhauer, Mügge, Foerstner, and others have shown how frequently this secondary twinning is developed in the crystals of rock-forming minerals.

When the negative crystals formed along the solution-planes of a mineral are filled with various secondary products, the whole character and aspect of the substance may be transformed. When the infilled negative crystals are of appreciable dimensions, the avanturine and "schiller" phenomena result from the action. When the action is on an ultra-microscopical scale, the phenomena of opalescence and of iridescence may be produced.

By the introduction of various substances in solution into a crystal, its composition may be altered and the way prepared for the recrystallization of the substance as a distinct mineral. It has been shown that, by the introduction of sodic chloride into a plagioclase-felspar, the way has been prepared for the conversion of that mineral into scapolite (*Mineralog. Mag.*, viii. 186).

IX. *Under great pressures, paramorphic changes take place in crystalline bodies without any alteration in their chemical composition.*

It is a well-known fact that, under the slight pressure which can be exerted by the hand, the orthorhombic, yellow variety of mercuric iodide passes into the tetragonal, red variety. Spring has shown that, under a pressure of 5000 atmospheres, monoclinic sulphur passes, at ordinary temperatures, into the orthorhombic form. Van 't Hoff and Reicher have shown that the temperature at which this latter change takes place is progressively diminished as the pressure is increased.

That slight forces acting through a considerable period of time are competent to produce such paramorphic changes has long been known. Thus the mercuric iodide and sulphur undergo their paramorphic changes slowly when subjected only to the ordinary vicissitudes of atmospheric temperature.

Many interesting examples of similar heteromorphous forms of the same compound are familiar to geologists, such as calcite and aragonite among the carbonates, and pyroxenes and amphiboles among the silicates. Heteromorphism, indeed, appears to be the rule rather than the exception in the mineral kingdom.

The slow paramorphic changes between heteromorphous forms of the same compound was long ago studied by Gustav Rose; and in more recent years the dependence of these changes on great pressures, or on small forces acting through long periods of time, has engaged the attention of J. A. Phillips, Allport, Hawes, R. D. Irving, J. Lehmann, G. H. Williams, Teall, and other observers.

In considering these paramorphic changes, it must be remembered that the transition under pressure is not always, as in the case of sulphur, from a less dense to a more dense form. On the contrary, as in the change of both aragonite to calcite and of augite to hornblende, we find the denser but less stable form passing into the less dense but more stable one. Stability, however, is only a relative term: while one form of a compound may be most stable at one temperature or under a certain pressure, other conditions may exist under which it becomes an unstable form.

X. *Both solution and the formation of new crystalline compounds may result from pressure, and these two operations may take place together; in this way more or less complete interchange of ingredients may take place between the crystalline bodies, and pseudomorphs be formed.*

That most of the pseudomorphic changes, so common in the mineral kingdom, take place at considerable depths from the surface there seems no room to doubt; and in all these cases it may be inferred that pressure is one of the determining conditions of the action.

The effects of these pseudomorphic changes in transforming vast rock-masses into others of totally different composition—such as limestone into dolomite, chalybite, or silica—has long been familiar to geologists; and modern microscopical methods have enabled us to trace the progress of these changes from their earliest beginnings to their complete consummation.

Without entering further into this very wide question, I may mention that Mr. G. F. Becker has lately published the full details of his studies of the Coast-Ranges of California, and that these tend to prove that, in comparatively recent geological times, vast masses of rock in that district have had their substance replaced in some cases by silica, and in others by serpentine; the changes sometimes taking place over considerable areas. These conclusions, arrived at by the officers of the U.S. Geological Survey, if fully established—and there appears to be no room for doubt as to their general accuracy—are not less interesting and suggestive than they are novel and startling.

XI. *When, as the result of dynamical pressures, the crystalline constituents of rocks are brought into close contact, chemical affinity comes into play between them, and new mineral species result from the reactions that take place. This operation is facilitated, when, as a consequence of internal strains, differential movements are set up within the rock-mass, and rubbing or sliding contacts between its particles are brought about.*

Chemists are acquainted with many examples of che-

mical action following from the simple bringing into close contact of molecules. In the union of gases, when they are condensed by platinum-black, and even in the light rubbing of a safety-match on the match-box, we have illustrations of such phenomena.

Spring has shown that, when powdered metals are mixed together and subjected to great pressures, union takes place between them, and alloys are formed. When dry anhydrous salts are similarly treated, double decomposition takes place, and new compounds are formed.

Prof. Thorpe has shown that dry anhydrous salts may be made to react with one another by being simply rubbed together in a mortar; and both Mr. Hallock and Prof. Spring are agreed as to the intensification of action which occurs when rubbing or sliding movements—attended with necessary multiplication of points of contact in compressed bodies—takes place.

Lastly, it may be pointed out that Spring has recently shown *time* to be a very important factor in such changes, by allowing slow diffusion to take place at the surfaces of contact.

That the rocks known as "crystalline schists and gneisses" have had their peculiar characters produced by "internal differential movements," resulting from "enormous irregular pressures," was clearly recognized by Poulett-Scrope, Darwin, Naumann, and Sharpe long before the researches of physicists and chemists had supplied us with the explanation of the phenomena. Modern petrography has confirmed and illustrated these conclusions, enabling us to study the actual stages of the processes of change by which, through the reaction of the constituent minerals of a rock under pressure, the whole mass resolves itself into a completely different mineral-aggregate. The labours of Lossen in the Hartz, of J. Lehmann in Saxony, and of H. Reusch in Norway, have been of especial value in establishing these important conclusions.

As an illustration of this kind of action, we cannot, perhaps, do better than take the case of a rock (gabbro) consisting of three somewhat unstable constituents (see Fig. 2), labradorite (A), pyroxene (B), and olivine (C). In

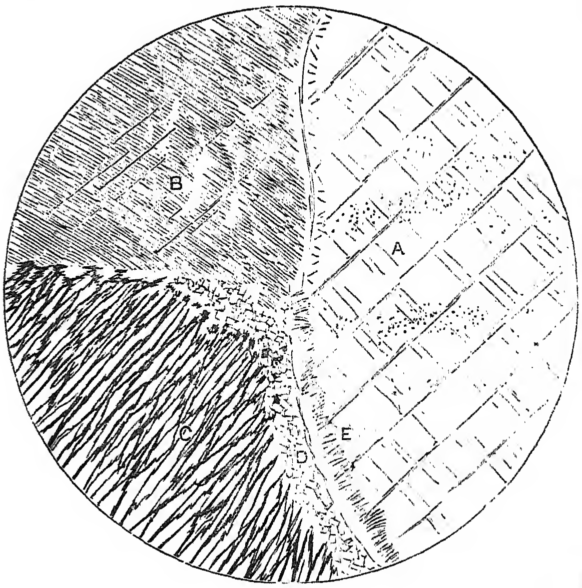


FIG. 2.

the rock from which the figure has been taken, there are clear evidences of its having been subjected to deforming stresses. Where the olivine, the least stable of the three minerals, is in contact with the labradorite, the silicates of which they are composed have reacted upon one another. The result is seen in the formation of a zone between them, consisting of entirely new minerals—a pyroxene (D), and a hornblende (E). Similar changes, but not so strongly marked, are seen to be in progress between the olivine and the pyroxene, and between the pyroxene and the labradorite.

By carefully selecting and studying a series of specimens from the same rock-mass, every step in the metamorphosis of a rock may be followed, from incipient changes like those in the case above illustrated, to the final disappearance of every vestige of the original mineral constituents of the rock, and the substitution of new mineral species.

XII. *When internal strains and differential movements affect a mass, which is at the same time undergoing re-crystallization, the forms and relations of the crystalline particles that build up the new rock may be greatly modified by the action of the mechanical forces.*

That perfect rest is a condition upon which well-developed crystallization depends, is a fact too well known to need dwelling upon here. That very small mechanical causes, such as the presence of foreign bodies, or the existence of rough surfaces, may determine the size and position of crystals in a solidifying mass, is also a fact familiar to every chemist. By stirring or similar movements carried on within a crystallizing mass, granulation, or the formation of a number of small imperfect crystals, rather than of large and well-developed ones, is brought about; as in the well-known Pattison's process for desilverizing lead.

The evidence of perfectly similar actions having taken place in crystallizing rock-masses is everywhere conspicuous; and the results are the same, whether the crystallization occurred in a mass passing from a fluid to a solid state, or in a mass which remained solid during the whole process of recrystallization.

There are two structures which are especially exhibited by rocks that have been subjected to dynamo-metamorphism which seem clearly to have been produced by such causes; these are the structures known as the *granulitic* and the *foliated*.

The *granulitic* structure, which is so well exhibited by the rocks called "granulites," is characterized by the crystals assuming the form of granules, having more or less rounded outlines, and lying in every position; so that, under the polariscope, the mass resembles a fine mosaic. I have shown that well crystalline rock-masses (gabbros), when forced in a molten state through great fissures, assume on their edges, where much friction must have occurred, this granulitic habit, which is sometimes exhibited in a very striking manner (Quart. Journ. Geol. Soc., xlii. 76, &c.).

The foliated structure, so characteristic of schists and gneisses, consists in the separation along nearly parallel planes of leaf-like patches (folia) of the several mineral constituents of the crystallizing mass. Poulett-Scrope, from his study of the viscous lavas of Ponza, Lipari, and Hungary, and Darwin, from his study of the similar lavas of Ascension, were able to show that these rocks, under similar conditions, often assume a *foliated* structure. Perfectly granitic rock-masses, like the syenite of the Plauenschen Grund and the granitite of Aberdeen, sometimes exhibit on their margins a distinctly foliated structure.

It is worthy of notice that both the granulated and the foliated structures are produced in recrystallizing masses that are subjected to internal strains and differential movements. They are equally produced when the mass has been a liquid which has slowly passed into the solid state by the process of crystallization; and when by the processes we have already considered the mass, *retaining its solidity*, has undergone internal molecular rearrangement and recrystallization.

A rock-mass behaves as a viscous body, under slight pressures, when heat and the presence of water have overcome the cohesion of its particles. But the researches of Tresca and Daubrée have shown that, when subjected to sufficiently powerful stresses, the most perfectly solid bodies we know of behave like viscous bodies, and can be made to *flow*.

In the foregoing remarks, my main object has been to show how far the physical and chemical principles, which have been established by actual experiment, are capable of explaining the phenomena observed by the geologist in studying the earth's crust. I have especially avoided invoking any causes which must be regarded as hypothetical.

Some of the actions relied upon as explaining the origin of the great features of the rock-masses which compose the earth's crust may seem at first sight small and even insignificant. But the great lesson taught by modern geological science is that such small forces, operating upon enormous masses of matter during vast periods of time, are capable of effecting the most stupendous results.

In speaking of *statical* pressure, I have not treated it as an agent of change, like heat or electricity, but simply as a condition under which these agents operate—one which may profoundly modify or control their action. Such pressure, too, may produce great effects by causing a closer contact and consequent chemical action between the molecules of a fluid made to penetrate a solid, or between the molecules of two solids forced into more perfect contact. Statical pressure may, further, prevent the escape of volatile materials even under extreme temperatures, and these substances, as in the case of the "mineralizers" of the French chemists, may exercise important influences on the solids or liquids within which they are retained.

Dynamical pressure, especially when it results in differential movements in a mass, can certainly do all that is effected by statical pressure, and perhaps something more. That such motion is converted into heat there can be no doubt; and some geologists, like the late R. Mallet and Prof. Prestwich, have argued that the heat so produced must have played an important part in the work of metamorphism. But considering the slowness with which the earth-movements have probably taken place, and the opportunities for the dissipation of this thermal energy, it may be regarded as at least doubtful if at a particular point in the rock-mass the temperature could ever have been raised to such an extent, that any very important part of the work of metamorphism ought to be ascribed to it. In the same way, we may, perhaps, regard the suggestion of Mr. Sorby that, during great earth-movements, mechanical energy is directly converted into chemical energy, as one in favour of which no convincing evidence has as yet been adduced.

It is at least conceivable that the realm of excessively *high* pressures is one in which phenomena may be displayed which are as anomalous as those exhibited under extremely *low* pressures—the high *vacua* of Mr. Crookes. But until such effects have been demonstrated by actual experiment, it is unwise to invoke their aid in geological hypothesis. My great object, in the remarks I have ventured to offer you this evening, has been to show that, on well-established physical and chemical principles, the phenomena, which are exhibited by rock-masses that have been subjected to great pressures, are capable of satisfactory explanation.

THE UNIFORM PENNY POST.

Of all the jubilees that are now being celebrated, there is none which has had a more beneficial influence on the age than that celebrated last week at the Guildhall, with such success and good management. There are some who deplore the decay of letter-writing, and even a few who regard the penny post as an unmitigated evil, but no one can fail to perceive that the conduct of the great commercial business of this country would have been impossible without cheap postage.

We are not celebrating the penny post. This was proposed in 1659, by one John Hill, an attorney of York—curiously enough a namesake of Sir Rowland's—who showed its practicability and advocated free trade in letter-carrying. He proposed a rate of 1*d.* in England, 2*d.* in Scotland, and 4*d.* in Ireland, as well as 3*d.* per ounce for small parcels.

Nor are we celebrating the invention of adhesive stamps, but the introduction, in 1840, of that great measure which swept away a sliding scale of postage of single letters written on single sheets of paper which varied between 4*d.* and 1*s.*, and a system of franking that had grown, even in the reign of our present Queen, to a most shameful abuse. Envelopes or covers and enclosures involved double postage. If the letter weighed an ounce the rate was quadrupled. A single letter, London to Brighton, cost 8*d.*, to Edinburgh 1*s.* 1½*d.*, to Cork 1*s.* 5*d.*; or if it weighed 1¾ ounces, to Edinburgh 7*s.* 7½*d.*, to Cork 9*s.* 11*d.* The number of letters passing through the post in 1839 was 76,000,000. In 1889, it amounted to 1,600,000,000, and this excludes newspapers, post-cards, books, and parcels. The grand total for 1889 was 2,362,000,000.

It is not too much to say that the transport of this enormous mass of material would have been impossible but for the advent of steam. Railways and steam-boats have led to the possibility of the uniform post. Telegraphs have made its administration practical and simple. Without these practical applications of science its success would have been impossible. Pack-horses, mail-coaches, and sailing-vessels, would have failed to transport mails with the celerity, trustworthiness, and regularity, that are the essentials of a true postal service.

The Stuarts made the Post Office a monopoly of the Crown; and the Commons, who in Charles's day denounced the establishment of the monopoly, promptly proceeded by Cromwell's soldiers to put down John Hill and his free trade in letters. It has remained the monopoly of the State, and its work is well done, but this is due to the fact that it is so well supervised by the public itself, every member of which is interested in its well-working. If the breakfast table is not garnished with the expected letters, if any abuse, want of accommodation, or delay occurs, the press or the House of Commons soon wants to know the reason why, and the remedy is at once applied. In fact, the public is the master and the postal service knows it. In no service in the world can there be found more zeal, energy, and attention. The rewards are not quite so evident. Although this is the jubilee year, Post Office names were conspicuous by their absence from the Queen's Birthday honours, and even such ardent reformers as Mr. Henniker Heaton, with an ignorance that is surprising, speak in contemptuous terms of the unimpressionable mind of the red-taped official.

The most scientific branch of the Post Office is unquestionably the telegraphic. Many fears were entertained that its efficiency would deteriorate under the supposed chilling influence of Government monopoly, but these fears have not been realized. Facilities have been increased, business has been developed, improvements have been introduced, new processes have not only been adopted but originated, and our Postal Telegraph Department unquestionably holds the most prominent position in the world at the present moment. The number of messages, which in 1869, the year before the transfer, amounted to 6,000,000, now reaches over 60,000,000. Duplex, quadruplex, and sexuplex methods have been made practical. The automatic system of Wheatstone,

which its ardent inventor hoped would work well at 120 words, now works equally well at 600 words a minute.

The telephone, owing to mismanagement and the operation of our Patent Laws, has not received much development in England yet; but with the expiry of the patents at the end of this year it is hoped that every post-office will become an exchange, and the business of telephony will flourish as well in England as it has in Sweden and Norway and some of the smaller States in Europe. Competition and free trade will certainly tend to bring this marvellous and beautiful apparatus within the sphere of every domestic circle.

It is marvellous how science is rapidly becoming a household god. The electric light, bells, and telephones must prompt all to some knowledge of electricity. Ventilation, sanitation, pure water, warming apparatus, lead to a knowledge of other scientific principles. The laws of Nature are rapidly but surely becoming as familiar in our mouths as household words.

PENDULUM ELECTROMETER.

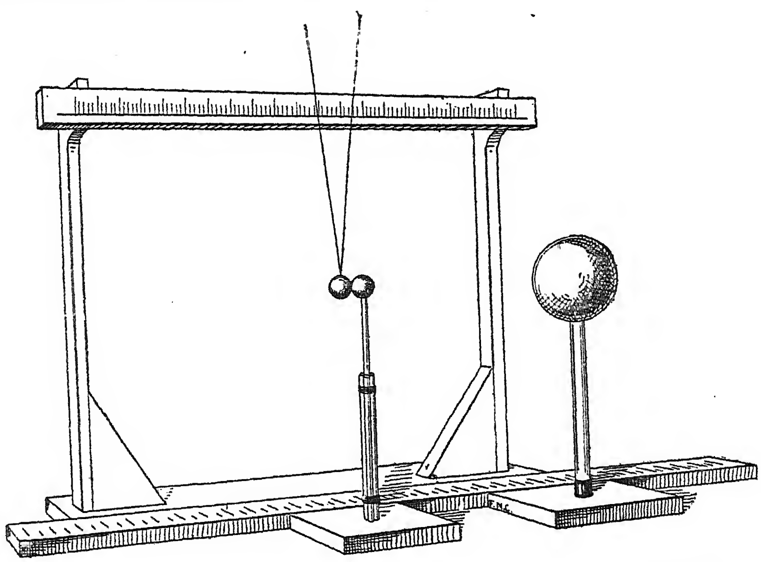
IN order to obtain an inexpensive apparatus by which the nature of electrostatic measurements could be clearly presented to students, and the measurements carried out before a class with ease and despatch in absolute units, Prof. Mayer, of the Stevens Institute of Technology, of New Jersey, has arranged the apparatus shown in the accompanying figure.

It consists of a gilt pith ball of 1 cm. radius, made of pieces of pith cemented together, and suspended at a distance of 364 cm. from the ceiling by a very fine silk fibre passed through a small staple driven into the ball. The ends of the fibres are attached to the ceiling at a distance of 52 cm. apart, and arranged so that the suspended ball can be raised or lowered, until it is at the same height as a brass ball, also of 1 cm. radius, supported on a glass rod, coated while hot with paraffin wax. A force of 1 dyne acting on the suspended ball deflects it through 13·3 mm., and, as 2° deflection was the maximum employed, the scale was inclined to the horizontal so as to coincide with the chord of an angle of 2°.

If a charge be given to the two small balls when in contact, and when therefore it will divide equally between them, the charge on either in *absolute electrostatic units* equals

$$D\sqrt{\frac{d}{1 \cdot 33}},$$

where d is the deflection in centimetres of the pendulum from the vertical, and D the distance in centimetres between the centres of the two balls.



To test the sensitiveness of the apparatus, the ball on the stand was placed at various distances from the suspended one, and the force between them observed. The law of the inverse squares was found to be verified with an error of less than 1 per cent. when D was over $5\frac{1}{4}$ cm. Next the gradual diminution of the deflection when the brass ball on the stand was in a fixed position was used to measure the rate of loss of charge, and it was found that the measured leakage to earth was proportional to the measured charges with a considerable degree of accuracy in several experiments, and with a maximum error of 20 per cent. in the most discordant experiments. Then the pendulum electrometer was used to measure the electric distribution over the surface of a cylinder, a proof plane being employed to convey the charge from different parts of the surface of the cylinder to the pendulum electrometer, and results were obtained closely agreeing with those obtained by Coulomb. Lastly, the potential of the large sphere was experimentally determined in absolute electrostatic units for different charges given to it.

NOTES.

WE are glad to learn that the President of the French Republic has conferred on Prof. Sylvester and Prof. Cayley the "Decoration d'Officier de la Légion d'Honneur." This honour has been granted in consequence of a request addressed to the French Minister of Foreign Affairs by the President and other members of the Academy of Sciences.

LORD RAYLEIGH has been elected a corresponding member of the Imperial Academy of Sciences in Vienna.

THE French Association for the Advancement of Science will hold its nineteenth meeting at Limoges from August 7 to 14. Various English men of science have been invited by the Bureau of the Association to attend the meeting, and they are asked to let their decision be known before July 1. Those of them who accept the invitation will be the guests of the Municipality of Limoges.

THE Queen has been pleased to approve of the grant of Civil List pensions to Miss Charlotte, Ruth, Margaret, and Rose,

daughters of the late Rev. M. J. Berkeley, F.R.S. A Civil List pension has also been granted to Mrs. Wood, widow of the Rev. J. G. Wood, the well-known popular writer on natural history.

DR. JAMES CLARK, M.A. (Edin.), Ph.D. (Tübingen), Royal Exhibitioner and Associate in Botany, Prizeman in Geology (Edinburgh University), has been appointed Professor of Natural History in the College of Agriculture, Downton, Salisbury. Dr. Clark has recently been employed on important work in the Natural History Department of the British Museum, and is the author of several papers on geology and biology.

A CHAIR of Mechanical Engineering is about to be established in connection with the University College of South Wales and Monmouthshire, Cardiff; and the authorities of the institution are already looking about for a suitable professor. A lectureship in mining engineering will shortly be founded at the same College, and electrical engineering is also to receive attention.

THE Annual Congress of the British Archæological Association will be held at Oxford in the second week of July.

AN important International Photographic Exhibition will be held in Vienna in April next year. The *Photographic News* says it is intended that only comparatively recent work, and that of the best kind, shall be shown. A jury of artists and photographers will decide as to the admission of pictures. The Exhibition will be held in the Imperial Austrian Museum of Art and Industry.

MR. THOMAS LAYTON, F.S.A., of Kew, who has for many years been engaged in forming a museum of the prehistoric antiquities of his district, has lent to the British Museum a fine series of bronze swords, spears, and axes, all found in the River Thames between Richmond and Battersea. The loan is shown in a case by itself.

A SCIENTIFIC and Industrial Exhibition was to be opened at Kazan on May 27. It will contain exhibits from Eastern Russia, Siberia, Central Asia, and the Caucasus, and promises to be of great interest to ethnographers.

DR. ADOLF STRUBELL, of Frankfort, who started on a journey of zoological research in India in March 1889, is now in Java making scientific collections, which he intends to present to the Senckenberg Museum of Frankfort.

THE celebration of the six hundredth anniversary of the foundation of the University of Montpellier has been most successful. All the great technical schools of Paris and the French provinces were represented, and deputations from many foreign Universities were present. The proceedings began on May 22, when there was a great reception in the University hall. M. Chancel, the Rector, welcomed the guests, and Prof. Tedenat sketched the history of the University and its most celebrated professors. On the following day M. Carnot arrived. The delegates of foreign Universities, followed by those of the great French schools, marched from the University to the Prefecture to be presented to the President of the Republic; and, if we may judge from a description by a correspondent of the *Times*, the procession must have been a remarkably interesting spectacle, the French and foreign professors being in robes of the most varied colours. The pavement and balconies along the route were crowded by men, women, and children. After the ceremony at the Prefecture the company proceeded to a park overlooking the town, commanding a view of the Cevennes on one side and the Mediterranean on the other. Several speeches were delivered under an awning. The Rector of the University thanked the President for having honoured the celebration by his presence. M. Croset gave a history of the University, and dwelt on the great trade of Montpellier in the Middle Ages, and its relations with the Arabs and Jews. Its most flourishing period, he said, was from the twelfth to the fourteenth century, and Petrarch spoke of it as a kind of ideal University. It made special progress in studies based on the observation of nature. The delegate of Bologna, the most ancient University represented, thanked M. Carnot for his reception of the foreign delegates. M. Bourgeois, Minister of Education, in a much-applauded speech, said the Government recognized the justice of the desire expressed by Montpellier and the other great schools to resume the name of University and the privileges associated therewith, and the question would shortly be discussed in the Chamber. We may specially note that the later proceedings included the presentation of an address by French men of science to Prof. Helmholtz, who represented the University of Berlin.

THE Königliche Physikalisch-Oekonomische Gesellschaft of Königsberg, one of the oldest societies of its kind, recently celebrated its centenary. It met first in Mohrungen, but in 1792 was amalgamated with the Economical Reading Institute of Königsberg, and thereafter bore its present name. In its earlier years it dealt chiefly with rural economy and agriculture. Later on, questions of natural science came more to the front, partly under the influence of Karl Ernst von Baer, the most illustrious name in the Society's annals. Still later, the Society had an anatomist of note among its members, Heinrich Rathke, who did good work in the same field as von Baer.

DR. T. A. HIRST, a former President of the Association for the Improvement of Geometrical Teaching, has presented to the Association, for its library, a valuable gift of forty volumes on geometry. The Association has also acquired by purchase an interesting collection of about twenty-five older text-books, including the "Treatise on Algebra" by Saunderson, the blind Lucasian Professor, and Stirling's "Methodus Differentialis."

ON Wednesday, May 21, a public meeting was held at the Mansion House "to promote the national work undertaken by the committee for testing smoke-preventing appliances." The Lord Mayor presided. Lord Derby proposed a resolution approving the objects of the committee. He thought that the diminution of smoke, and its necessary accompaniment dirt, was a matter which concerned everyone, except those who were fortunate enough to live away from great towns. Indifference was the real difficulty which they had to encounter, but in England anything which came to be recognized as a want was eventually supplied. The expenditure of fuel in creating dirt—for that was what it came to—was a waste of fuel itself, and the injury caused to property was not inconsiderable. He believed that more than three-fourths—he would say something like nine-tenths—of the smoke from collieries and factories was absolutely preventable, though some trouble and outlay would be required. Possibly more stringent legislation would be needed, but let them first try the experiment of enforcing the laws which they already had. Lord Howard of Glossop seconded and Prof. Chandler Roberts-Austen supported the resolution, which was carried unanimously. On the motion of Sir Henry Roscoe, M.P., seconded by Earl Fitzwilliam, and supported by Alderman Bowes (Salford), a resolution was passed in favour of the raising of a fund to meet the expenses of the work.

SOME interesting explorations have just been made in connection with the famous Adelsberg Cave. The Vienna Correspondent of the *Daily News* says that various citizens of Adelsberg, wishing to ascertain whether the Ottoker Cave, discovered a year ago at some distance from Adelsberg, was in any way connected with the great cave, followed the course of the subterranean river Poik. It was known that forty years ago a party of explorers had their progress barred by a large lake, and the present adventurers therefore carried with them a boat.

Having successfully crossed the body of water mentioned, they came to lofty galleries through which the river flowed. It was possible to walk on the banks of the stream, but at intervals it expanded into small lakes, and the boat had to be used. At last the gallery branched into two corridors, one of which the stream rendered impassable, while the other was high and quite dry. The boat was dragged up, and the party proceeded. After crossing a fourth lake, the largest they had met, they found that the Ottoker Cave had been reached. The journey through the galleries lasted six hours. The explorers saw that they had by no means penetrated to the remotest parts of the grotto, and there is evidently still a wide field for discovery.

ACCORDING to a telegram sent through Reuter's Agency from New York, a slight shock of earthquake was felt at Utica and at other points in the northern portion of New York State on May 25. The disturbance was felt more severely in Montgomery County. At Little Falls the shock was sufficiently strong to cause dishes and other similar articles to rattle, and subterranean rumblings were heard, while at Fort Hunter the buildings were so shaken that beds were moved and their occupants awakened. No damage was done.

ANOTHER telegram, sent through Reuter's Agency from Constantinople on May 26, tells of the destruction of an Armenian village by an earthquake. The village was Kayi, in the district of Refahie. Mineral springs spouted from the crevices made in the ground, and flooded the fields. There was no loss of life, as two days previously subterranean rumblings were heard, and cracks appeared in the ground, in consequence of which the Caimakan of the district ordered the inhabitants to leave the village.

ON Sunday last an influential meeting was held in Madrid, at the official residence of the Prime Minister, to prepare the way for the celebration, in 1892, of the four hundredth anniversary of the discovery of America by Columbus. The meeting selected a Grand Committee, which will act in concert with the Spanish Government and the Royal Commission appointed some time ago, and presided over by the Duke of Veragua, a lineal descendant of Columbus, and the present Minister of Public Works. The Madrid Correspondent of the *Daily News* says that the most eminent among Spanish statesmen, as well as artists, writers, men of science, and military men, will assist on the organizing committees. It is proposed that the centenary shall be celebrated, if possible, by an Exhibition at Madrid. Vigorous preparations are also being made at Genoa for the suitable commemoration of the same great event.

THE Danish Admiralty has ordered systematic hydrographical observations to be made all round the Danish coast. They began on May 1, and are to be continued regularly once a month on all lightships and on five movable stations. The object of these observations is to obtain detailed data concerning the ichthyological and meteorological conditions of the Danish seas. Special apparatus has been constructed by Captain Rung for the measurement of the percentage of salt in the sea-water.

WE learn from *Science* that, at a recent meeting of the American Meteorological Society in Washington, resolutions were adopted "favouring the recognition of the eminent services of American electricians by perpetuating their names in the nomenclature of electrical units." At the Electrical Conference to be held in America in 1892, it will be proposed that the name of Joseph Henry—or some modification of it—shall be given to the unit of self-induction, "he having been the first to investigate that phenomenon, and his investigations having been more complete than those of other electricians before or since."

THE temperature of snow at different depths has been investigated by Signor Chistoni. He finds that the variations in temperature of the lowest layer, next the ground, are extremely small, whilst the uppermost layer has often considerably higher temperature (as much as 10° C. at times). The temperature minimum of the air-layer next the snow was always lower than that of the uppermost snow-layer, while an air-layer about 20 inches above the snow had a higher temperature than the layer 1·2 inches above the snow.

DR. J. HANN communicated to the Academy of Sciences at Vienna, on April 17, a memoir on the high air-pressure of November 12–24, 1889, in Central Europe, together with remarks upon high-pressure areas generally. As this anticyclone lay nearly the whole time over the Alpine district, observations could be made at various stations up to a height of above 10,000 feet. Dr. Hann found (1) that the high pressure extended to more than three kilometres above sea-level; (2) that at this altitude the relative warmth was as great as at a height of one kilometre, while the usual depression of temperature of winter anticyclones was limited to a few hundred feet above the earth; (3) that great dryness prevailed in the higher strata of the air. The author finds in these results a cogent reason for concluding that in barometrical maxima the air has a descending motion, and that the conditions of pressure are not explained by conditions of temperature, but are a consequence of the movement of the air. The temperature conditions are dependent upon the movements of the anticyclones, in the same way as the dryness of the air, and the clearness of the sky. In another section of the paper he investigates the vertical distribution of temperature in a barometrical minimum. During one instance, on October 9–10, 1889, he found that the temperature on the summit of the Sonnblick was lower than during the barometrical maximum above quoted. Until the establishment of mountain stations, the temperature was assumed to be one of the chief causes of the form of motion of cyclones and anticyclones, but future inquiries must take into account that up to at least four or five kilometres the temperature of the centre of an anticyclone may be, and probably always is, higher than in the centre of a cyclone.

IN a recent number of the *Zeitschrift für Schul-Geographie*, Mr. H. Habenicht has written an article on the causes of the cyclones of the North Atlantic. The author points out that, if the globe were covered with water, the general circulation of the air would be very regular, without local depressions and steep barometric gradients, and he refers to the contrast of the systems prevailing, *e.g.* between the South Pacific and the North Atlantic. He finds the explanation primarily in the obstruction offered to the regular courses of the winds by the great continents to the east and west of the Atlantic; and, secondly, in the constant barometrical maxima over the continent in winter and in the neighbourhood of the Arctic regions.

THE Massachusetts Institute of Technology, Boston, has issued the twenty-fifth annual catalogue of its officers and students, with a statement of its courses of instruction and a list of its alumni. The courses of study include the physical, chemical, and natural sciences and their applications; pure and applied mathematics; drawing; the English, French, German, and other modern languages; history; political science; and international and business law. It is claimed that these studies and exercises are so arranged as to afford a liberal and practical education in preparation for active pursuits, and a thorough training for most of the scientific professions.

IN the entomological part of the forty-first Annual Report of the trustees of the New York State Museum of Natural History, lately published, reference is made to the statements which have been advanced as to the long imprisonment of beetles within furniture. The writer suggests that when such cases occur the condi-

tions may bring about a lethargic state, in which respiration and accompanying phenomena are almost or entirely suspended through the complete exclusion of air (a hermetic sealing) by the rubbing, oiling, varnishing, or other polishing which the furniture has undergone. As an instance of prolonged vitality, he quotes an extract from the third Report on the insects of New York, by Dr. Fitch. In this passage Dr. Fitch says:—"In 1786, a son of General Israel Putnam, residing in Williamstown, Mass., had a table made from one of his app'e-trees. Many years afterwards the gnawing of an insect was heard in one of the leaves of this table, which noise continued for a year or two, when a large, long-horned beetle made its exit therefrom. Subsequently, the same noise was heard again, and another insect, and afterwards a third, all of the same kind, issued from this table-leaf—the first one coming out twenty, and the last one twenty-eight, years after the tree was cut down." The evidence before Dr. Fitch convinced him that the insect was the longicorn beetle *Ceras phorus balteatus*, now known as *Chion cinctus* (Drury).

The *American Naturalist* quotes from the *Salem Register* an extract from which it seems that the museum of the Peabody Academy of Science of Salem, Mass., has lately been enriched by a fine collection of objects illustrating the art and ethnology of Japan. This has been formed by Prof. Edward S. Morse, who some time ago spent several months in Japan. The catalogue of Japanese accessions enumerates 691 specimens, the most conspicuous objects being life-sized figures, representing different classes of the community. These models were all made for the museum, and are the best that have ever been brought to America. The collection also includes many fine old swords, sets of tools, and pictures illustrating various trades and professions.

Some curious electrical phenomena were lately observed (according to a writer in the *Chemische Zeitung*) in a stearin and ceresin manufactory in Italy. One evening four vats of white ceresin (which is a paraffin got from ozokerit), containing about 500 kg. each, were being stirred to cool. When the point of solidification was nearly reached, the electric light of the place accidentally went out; and, to the surprise and alarm of the rather ignorant workmen, the mass of ceresin was observed to give pale sparks on the slightest motion. If the hand was brought near, loud sparks nearly two inches long were obtained. The phenomenon lasted over half an hour.

A valuable collection of Tibetan medical works and drugs has been brought by M. Ptitsyn from Transbaikalia. He has also collected most interesting information as to the courses of study at the Buddhist *lamas'* University at the Gusinoie Ozero Monastery in Transbaikalia. The curriculum lasts ten years, the first four of which are devoted to the study of the Tibetan and Mongol languages, to religious service, and to practice in drawing and various handicrafts. The next three years are given to medicine. During the first of these three years the pupils learn by heart the five volumes of the chief Tibetan hand-books of medicine, and the names of the drugs. The next two years are given to the study of therapeutics and surgery. The students also visit the Urga High School to follow the courses of the more renowned Tibet *lamas*, who come to Urga on purpose. The eighth year is given to astronomy and astrology, and the last two to philosophy and theology. Medicine is studied only by those who wish to devote themselves to the medical profession, and the courses of astronomy, astrology, philosophy, and theology are followed only by the best pupils. The chief (printed) medical work of Tibet is the "Rodijaçhava," or "The Tale of the *Curkhan* Otochi (god of Medicine) about what formerly was," a copy of which was secured by M. Ptitsyn. The Tibet medical authorities recognize 101 fundamental diseases, and M. Ptitsyn gives the names of 429 elements of drugs used by the Buddhist physicians. He notices that of the 101 diseases only two (paralysis and a kind of influence of the planets) are attributed to a mythical origin, and that of the 429 drugs only three have a similar origin (the bones of a dragon, the horns and the skin of the unicorn). The remainder are chiefly herbs, seeds, fruits, roots, and flowers, and partly mineral matters. They are all bought in Chinese drug-shops, except quinine, which is bought in Russia. M. Ptitsyn was allowed to visit one of the drug-shops, and found all drugs kept in order in separate drawers. He has brought to St. Petersburg samples of 202 different drugs, which will be analyzed at the Medical Academy.

Some sea-urchins are known to live in cavities in rock. And the diameter of the cavity is often wider than that of the entrance, so that the animal could not leave its home or be taken out without injury. On the French coast of Croisic (Lower Loire) may be seen thousands of urchins thus ensconced in the granite rock, which is rich in felspar and quartz. The animals, it is not doubted, make and widen the holes for themselves; but the question how has not been satisfactorily answered. Chemical solution of the rock seems excluded, considering both the nature of the latter, and also that no acid which could be thus used has been proved to exist in the urchin. The matter has been studied lately by M. John, and in an inaugural dissertation (*Arch. f. Naturges.*) he explains the effects by mechanical action. With the so-called "lantern of Aristotle" the animal probably bites the rock; the sucker feet are also attached, and a rotatory motion is imparted to the body, the prickle points, with the lantern, gradually wearing down the surface. These cavities afford a shelter to the urchins against the action of the waves. An attempt is made to conceal them by means of mussel and other shells. The rocks in which the cavities occur are in general thickly covered with calcareous Algæ. It has been thought that possibly these decompose the rock, and so facilitate the work of the urchins. M. John, however, finds no such chemical relation, but atmospheric agencies, he considers, may help the work of boring. A number of other animals are known to penetrate rock, and it is supposed that they do it also in a mechanical way. Recently, M. Forel described to the Vaudois Society of Natural Sciences how in the hard limestone of Constantine, Algiers, *Helix aspera* was found in holes 4 to 5 inches in depth.

It has been hitherto impossible, by the most careful and subtle methods, to produce absolutely pure water. Such water, it is thought, would have no conductivity for the galvanic current; but, as a matter of fact, there is always a measurable conductivity, which, in glass-vessels *e.g.*, gradually grows from day to day, through glass being dissolved. It has been lately observed by Herr Pfeiffer (*Wied. Ann.*) that water purified as much as possible, and standing only a short time in contact with the air, showed next day a continuous decrease of conductivity, which gradually disappeared, giving place to the normal unavoidable increase. After testing various explanations of this by experiment, he came to the conclusion that the true explanation is micro-organisms coming into the water, and absorbing the conducting substances present. On this assumption such organisms would appear to have an almost absolute power of absorption, something like that of sulphuric acid for water-vapour.

Mr. L. Upcott Gill has issued the first part of a volume entitled "British Cage Birds," by Mr. R. L. Wallace. The work will be completed in 15 parts, and will contain directions for breeding, rearing, and managing the various British birds that can be kept in confinement. Mr. Gill has published also the first part of "The Canary Book," by the same author. Both works are illustrated with coloured plates and wood-engravings.

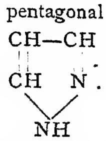
IN the new number of the *Internationales Archiv für Ethnographie*, Hermann Strebel continues his paper on a peculiar kind of stone object, found in Mexico and Central America, which is generally supposed to have been used in connection with the sacrifice of human victims. It is fashioned in the shape of a yoke or bow, and enriched with sculpture. Herr Strebel shows that it was worn as a mark of honour by persons of high rank. The sculpture was, he believes, of a symbolical character. Dr. L. Lewin, of Berlin, contributes an interesting paper on betel-chewing, adding fresh information to that which he brought together in a previous article. M. de Clercq gives some notes (in Dutch) relating to New Guinea, and Herr H. Vos deals with the area of anthropophagy on the Asiatic continent.

THE Leicester Literary and Philosophical Society has issued the third part of the second volume of its Transactions (new quarterly series). Among the contents are the abstract of an address, by Prof. Flower, on pygmies; a paper on spiders, by the Rev. W. Agar; and a contribution to the pterylography of birds' wings, by W. P. Pycraft.

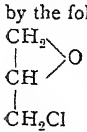
WE have received Part 20 of Cassell's "New Popular Educator." It is carefully illustrated, and contains maps of Eastern Australia and New Zealand.

MR. JOHN WHELDON has issued a catalogue of zoological works, and papers, transactions, and journals relating chiefly to anatomy and physiology. A catalogue of works on astronomy, magnetism, and meteorology has been issued by Messrs. Dulau and Co.

PYRAZOL, $C_3H_4N_2$, the fundamental base of a rapidly growing series of compounds, has been synthesized by Prof. Balbiano, of Messina, from epichlorhydrin and the recently isolated hydrazine hydrate (*Berichte*, No. 8, p. 1103). Pyrazol is a pentagonal closed chain compound, which may be represented



The reaction between epichlorhydrin and hydrazine hydrate is a somewhat violent one, a considerable amount of heat being generated. It appears to take place in the manner indicated by the following equation:—



$$+ 2N_2H_4 . H_2O = C_3H_4N_2 + N_2H_4 . HCl + 3H_2O + H_2.$$

Equal weights of epichlorhydrin and hydrazine hydrate, which latter is a liquid boiling at 119° C., are cautiously mixed in a flask, to which is immediately fitted a reflux condenser. The reaction completes itself in 3–4 minutes without any external application of heat. When the reaction is at an end, and the last trace of the epichlorhydrin has disappeared, the flask and contents are heated in a water-bath for 25–30 minutes. After allowing to cool, a quantity of zinc chloride equal to that of either of the reagents is added, in order to facilitate the splitting off of water. The whole is afterwards again heated for an hour over the water-bath. The yellow waxy mass so obtained is then mixed with 300–400 c.c. of water for every 10 grams of either reagent employed, and the mixture distilled in steam. Pyrazol and ammonia distil over in the steam together, and, in order to separate the pyrazol, the distillate is treated with a solution of mercuric chloride, which produces a mixed precipitate of the mercury compound of pyrazol and mercurammonium chloride. The precipitate is suspended in water, decomposed with sulphuric acid, and the solution of pyrazol hydrochloride and salammoniac evaporated to the crystallizing point on the water-bath. The residue is then decomposed by potash, and the pyrazol extracted by ether. Upon evaporation, pyrazol is obtained as a mass of hard colourless needles. The crystals of pyrazol are readily soluble in cold water, with production of a neutral solution. They possess an odour very similar to that of pyridine. They melt to a colourless liquid at 69·5°–70°, and the liquid boils at 186°–188°. The aqueous solution gives a white precipitate with mercuric chloride solution and with an ammoniacal solution of silver nitrate. In all these respects the pyrazol thus prepared from epichlorhydrin and hydrazine hydrate is identical with a substance of the formula $C_3H_4N_2$ prepared some little time ago by Buchner by heating the methyl ether of acetylene-dicarboxylic-diazoacetic acid. A concentrated hydrochloric acid solution of pyrazol gives, with platinum chloride, a precipitate of lustrous yellowish-red needles of pyrazol-platinate $(C_3H_4N_2 . HCl)_2PtCl_4 . 2H_2O$. When this salt is heated to 205°, the colour changes to straw-yellow, and remains permanent up to 250°. The yellow substance is a definite compound, insoluble in water, and possessing the composition $\begin{matrix}C_3H_3N_2\\C_3H_3N_2\end{matrix}\Big\rangle PtCl_2$. It is formed from pyrazol-platinate by loss of two molecules of water and four molecules of hydrochloric acid.

THE additions to the Zoological Society's Gardens during the past week include two Beatrix Antelopes (*Oryx beatrix* ♂ ♀) from Arabia, presented by Colonel Ross; a North African Jackal (*Canis anthus*) from North Africa, presented by Captain Hay; a Common Paradoxure (*Paradoxurus typus*) from India, presented by Mr. C. Armstrong King; a Vociferous Sea Eagle (*Haliaëtus vocifer*), a White-crested Tiger-Bittern (*Tigrisoma leucolophum*) from West Africa, presented by Mr. J. B. Elliot; a Mexican Guan (*Penelope purpurascens*) from Mexico, presented by Mr. J. W. Dawe; two Common Kingfishers (*Alcedo ispida*), British, presented by Mr. T. E. Gunn; a Tawny Owl (*Syrnium aluco*), British, presented by the Hon. C. Parker; two All-green Snakes (*Philodryas viridissimus*), two Natterer's Snakes (*Thamnodynastes nattereri*), two Merrem's Snakes (*Liophis merremi*), a Chequered Elaps (*Elaps lemniscatus*) from South America, presented by Mr. A. C. Derrett; a Barraband's Parrakeet (*Polytelis barrabandi*) from New South Wales, a Brush Turkey (*Talegalla lathami*) from Australia, deposited; an Eland (*Oreas canna* ♂), bred in France, two Diademed Jays (*Cyanocitta diademata*) from Mexico, two Temminck's Tragopans (*Ceriornis temmincki* ♂ ♀) from China, purchased; two Persian Gazelles (*Gazella subgutterosa* ♂ ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on May 29 = 14h. 29m. 9s.

Remarks.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 3846	—	—	14 14 45	+ 4 26
(2) D.M. + 15° 2758 ...	5	Reddish-yellow.	14 40 45	+15 35
(3) o Boötis	4	Whitish-yellow.	14 40 6	+17 20
(4) ζ Boötis	3	White.	14 35 54	+14 12
(5) χ Cygni	Var.	Very red.	19 46 20	+32 38

(1) There are no very bright nebulæ which come to the meridian near 10 o'clock during this week, but the one given is probably one of the brightest. The General Catalogue description is: "Bright, pretty large; round; pretty suddenly brighter in the middle; barely resolvable (mottled as if with stars); a 12th magnitude star in *nf* quadrant." The spectrum has not been recorded.

(2) According to Vogel and Dunér this star has a magnificent

spectrum of Group II. All the bands 1-9 are plainly visible; they are wide and dark throughout the whole length of the spectrum. The chief observations required of such a star as this are direct comparisons with the bright carbon flutings, as their existence in stars of the 2nd group is still not generally accepted, although Dr. Copeland demonstrated it most conclusively in the so-called "Nova" Orionis. Our ideas of the constitution of this class of stars must turn almost entirely on this point; if the bright flutings exist, the stars, like comets, must consist of discrete masses.

(3) This is a star of the solar type (Vogel). The usual observations are required.

(4) The spectrum of this star is one of Group IV. The usual additional observations are required.

(5) This highly interesting variable will reach a maximum on May 30. The magnitude at maximum varies from 4 to 6·5, whilst that at minimum is below 13. The change of luminosity is therefore enormous, and it is obvious that many of the explanations offered for different kinds of variables, such as variation of spotted area, are quite insufficient for a case like this. The period is about 406 days, but it appears to be shortening. The spectrum is a magnificent one of Group II., and near the maximum last year Mr. Espin noted the presence of bright lines, amongst which the line D_3 was very bright. In future observations, the bright carbon flutings should also be carefully examined, as it seems very probable that, if they exist, they will brighten along with the lines of hydrogen and D_3. If the principal fluting, near *b*, be sufficiently bright, the 2nd maximum of the fluting may be bright enough to be measured. The collision theory of variability seems to explain this class of variable in every detail, even to the lengthening of the period, for the retardation which the secondary swarm would undergo at maximum must inevitably in time lengthen the period.

The Spectrum of Comet Brooks (*a* 1890).—On the evenings of May 21, 22, and 23, I made some observations of this comet which may not be without interest. On the 21st the appearance of the comet was not unlike that of the Nebula in Andromeda, except that it was almost circular instead of elliptical in shape. The colour of the comet was whitish, and the nucleus was rather ill-defined. The spectrum was to a large extent continuous, but there were also unmistakable bright flutings. These were brightest in the nucleus, but they also extended faintly throughout the whole mass. Direct comparisons with the blue base of a spirit-lamp flame showed coincidences with all the flutings, except the bright one in the violet which is characteristic of hydrocarbons. The fluting near *b* was by far the brightest, and next in order of intensities came those near λ 474 and 564. The continuous spectrum extended from about D to a little beyond λ 474. On the 22nd the nucleus was much less central than on the previous evening, probably owing to further development of a short bushy tail. The spectrum, however, showed no obvious differences, except that the brightest fluting had slightly increased in intensity. On the 23rd the form and spectrum were practically the same as on the 22nd. The observations were made with a 10-inch refractor, but the comet was easily seen in the 3-inch finder.

The comet had obviously got beyond the earlier temperature stages before I observed it, and it will be highly interesting to notice if the further changes indicated by Prof. Lockyer's discussion of cometary spectra (see p. 20) take place. These will be most obvious in the faintest band (λ 564) owing to the superposition of the bright flutings of manganese and lead. Changes of the form or wave-length of this band should therefore be particularly noted in further observations. It is, of course, desirable that the observations should be made independently by more than one observer. In my observations of May 21 I was very much struck by the resemblance of the comet to the Nebula in Andromeda, both in form and colour, and in spectrum.

On the 27th I again observed the comet, but saw no decided differences, either in its appearance or spectrum.

A. Fowler.

New Variable Star in Cygnus.—Prof. Pickering, of Harvard College, notes in *Astronomische Nachrichten*, No. 2968, that a study of the spectra of the fainter stars is now in progress with the 8-inch Draper telescope. An examination by Mrs. Fleming of photographs taken with small dispersion led to the discovery that the star D.M. + 48°·2940 whose approximate place for 1900 is in R.A. 19h. 40m. 8s., Decl. + 48° 32′ gave a spectrum resembling that of Mira Ceti and other variables of long period. A photographic chart was made, and on comparison with the photographs previously taken of this region proved conclusively that the star is a variable. The photographs compared are of three kinds: (1) trails in which the telescope was at rest and the brighter stars formed lines by their diurnal motion; (2) charts in which the stars formed circular images; (3) spectra formed by placing a prism in front of the object-glass. From the various photographs of the spectrum of this star, it appears that on September 23, 1887, the hydrogen lines *h* and G are shown bright; the star was then estimated as of the 8th magnitude. On June 16, 1888, magnitude being estimated as 9, the spectrum was faintly visible and apparently continuous, but no bright lines were seen. On September 7 of the same year, G was well seen, and *h* was barely visible, the magnitude being again estimated as 8. There is no doubt, therefore, that this is another example of that group of variable stars that exhibit bright hydrogen lines in their spectra when at a maximum.

Paris Observatory.—Admiral Mouchez, the Director of this Observatory, has issued his Report for the year 1889. An account of the resolutions adopted last September by the International Committee for the execution of the map of the heavens is given. It is also noted that the building is completed which is to receive in two or three months the *coudé* equatorial, 0·60 metre aperture and 18 metres focus. M. Lœwy has supervised the details of the installation of this instrument, which will replace, with some advantage to Paris Observatory, the instrument 0·74 metre aperture first intended for it. This latter telescope is attached to Meudon Observatory, and will find there atmospheric conditions more favourable for the use of its great optical power than at Paris.

The electric light has been installed for the lighting of the two *coudé* equatorials and the meridian circle.

For some years, almost the whole force of the Observatory has been engaged in re-observing on the meridian the stars in Lalande's catalogue. This work was commenced about twenty-five years ago, and on account of it other branches of astronomy have had to be neglected. It is, however, nearly completed, and Admiral Mouchez proposes to the Council that a regular spectroscopic service should be instituted. Up to now, spectroscopy has only existed nominally at Paris Observatory, and in appealing to the administration for the necessary funds to organize this new department it is very truly observed that no Observatory can well dispense with spectroscopic accessories, since it is the study of this branch of astronomy that enables the physical constitution of the heavenly bodies to be determined. M. Deslandres, already known for his spectroscopic works, will take charge of this new department.

A Commission has been appointed to investigate the inconveniences that would arise from the laying of the proposed railway line at a distance of about 150 metres from the Observatory. The Report of the Commission, when ready, will have some interest at other Observatories that have been similarly threatened.

MM. P. and P. Henry have continued the photographic work: 38 photographs of stars have been obtained, and 5 plates for the determination of the parallax of Victoria. The constant of photographic refraction has been determined, and lunar photographs having a diameter of 40 cm. have been obtained by direct enlargement.

Many observations have been made of comets. M. Bigourdan has made 300 complete measures of nebulæ; the major planets and the asteroids have received as much attention as the continued bad weather permitted; and, leaving out of consideration the absence of spectroscopic work, which another year may see remedied, the Report is altogether a satisfactory one.

On the Parallax of Double Stars.—At the March meeting of the Royal Astronomical Society, Mr. Arthur A. Rambant directed the attention of astronomers to a paper in which he pointed out the relation connecting the parallax and the relative velocity of the components of a double star with the period and angular elements of its orbit, and discussed the possibility of determining the distance by means of spectroscopic observations of this velocity. The photographs of stellar spectra recently obtained by Prof. Pickering in America and Prof. Vogel at Potsdam are in point of accuracy so far in advance of direct eye observations of motion in line of sight that they seem to demonstrate the possibility of applying the method described to the determination of parallax, and the author shows how the

parallax (π) of a star can be immediately deduced if its velocity (V) in miles per second in the line of sight has been determined by observation. The advantages to be expected from spectrographic observations of double stars for which πV is greater than 0·1 are shown to be:—

(1) An independent check on the parallax where this has been determined trigonometrically.

(2) A determination of the parallax where, owing to its smallness, the trigonometrical method fails.

(3) A determination of the sign of the inclination which will remove the ambiguity attaching to the situation of the orbit.

It is to be hoped that astronomers who have the requisite instruments for this kind of observation may be induced to take up what appears to be a promising field of work.

TURIN OBSERVATORY.—We have received various publications from the Observatory of the Royal University of Turin. Amongst them we find convenient ephemerides of the sun and moon for 1889 and for 1890 calculated for the horizon of Torino by Señors Porro and Aschieri respectively, and a note by the former observer on the total eclipse of the moon on January 28, 1888. The difference of longitude between the meridian circle at Turin Observatory and Milan Observatory has also been redetermined. The value found in 1823 was 5m. 58·85s., and the value now found is 5m. 58·736 ± 0·006; thus the difference between the two observations is only 0·11s., although the former was not made by telegraphy.

A CONTRIBUTION TO THE ETIOLOGY OF DIPHTHERIA.[1]

THE microbe, which was first described by Klebs (at the Wiesbaden Congress in 1883), then isolated and grown in artificial cultures by Löffler (*Mitth. aus dem K. Gesundheitsamte*, vol. ii.) from human diphtheritic membrane, was shown by this observer to act virulently on various animals. The Klebs-Löffler bacillus—by which name the diphtheria microbe is known—is the one with which also Roux and Yersin (*Annales de l'Institut Pasteur*, ii., 1888, No. 12) obtained positive results on guinea-pigs.

In the Reports of the Medical Officer of the Local Government Board for 1888-89 and 1889-90, I have shown that there occur in diphtheritic membranes two species of bacilli, very similar in morphological respects, and also in cultures on serum and on agar, but differing from one another in this, that one species, Klebs-Löffler bacillus No. 1, is not constant in diphtheritic membranes, does not grow on solid gelatine at 19°-20° C., and does not act pathogenically on animals; the other species, Klebs-Löffler bacillus No. 2, is constant in diphtheritic membranes, in fact is present even in the deeper layers of the membranes in great masses and almost in pure culture, acts very virulently on animals, and grows well on gelatine at 19°-20° C. Löffler, and after him other observers (Flügge, "Die Mikroorganismen," 1886), considered it as a character of the diphtheria bacillus that it does not grow on gelatine below 22° C., but this character, though true of the Klebs-Löffler species No. 1, does not appertain to the diphtheria bacillus species No. 2. In fact, there is no difficulty in obtaining pure cultures of this bacillus on gelatine if a particle of diphtheritic membrane be taken and well shaken in two or three successive lots of sterile salt solution, and from the last lot plate cultivations on gelatine are made. In this way I have obtained the diphtheria bacillus in great numbers of colonies and in pure culture. Zarniko (*Centralbl. f. Bakteriol. u. Parasit.*, vol. vi., 1889, p. 154) and Escherich (*ibid.*, vol. vii., 1890, p. 8) both state that the diphtheria bacillus does grow on gelatine below 20° C.

This bacillus diphtheriæ acts very virulently on guinea-pigs on subcutaneous inoculation; at the seat of the injection a tumour is produced, which in its pathology and in microscopic sections completely resembles the diphtheritic tissue of the human. In human diphtheria the diphtheria bacillus is present only in the diphtheritic membrane, but neither in the blood nor in the diseased viscera; the same holds good for the experimental guinea-pigs. In subcutaneous inoculation with artificial culture, though it causes in these animals acute disease and death—the lungs, intestine, and kidney are greatly congested—the diphtheria bacillus remains limited to the seat of inoculation. It was for these reasons that Löffler concluded that in diphtheria the diphtheritic membrane alone is the seat of the multiplication of the diphtheria bacillus, and that here a chemical poison is produced, which absorbed into the system causes the general diseased condition and eventually death. Roux and Yersin have then separated from artificial broth cultures the bacilli and the chemical products, and, by the injection of these latter alone into guinea-pigs, have produced a general effect. I have in this year's Report to the Medical Officer of the Local Government Board (1889-90) shown that in these experiments of injection of cultures into guinea-pigs, an active multiplication of the diphtheria bacilli at the seat of inoculation can be demonstrated by culture experiments; from the local diphtheritic tumour and the nearest lymph glands the diphtheria bacilli can be obtained in pure culture on gelatine.

On various occasions during the last three years information has reached me by Health Officers (Dr. Downes, Mr. Shirley Murphy, Dr. Thursfield) as to a curious relation existing between a mysterious cat disease and human diphtheria in this manner, that a cat or cats were taken ill with a pulmonary disease, and while ill were nursed by children, and then these latter sickened with well-marked diphtheria. Or children were taken ill with diphtheria, and either at the same time or afterwards the cat or cats sickened. The disease in the cat was described as an acute lung trouble; the animals were quiet, did not feed, and seemed not to be able to swallow; in some cases they recovered, in others they became emaciated, while the lung trouble increased, and ultimately they died. In one instance—in the north of London, in the spring, 1889—this cat malady, occurring in a house where diphtheria soon afterwards appeared amongst the children, was of a widespread nature; a veterinary surgeon—Mr. Daniel—informed me that at that time he had several patients amongst cats affected with the disease, consisting in an acute catarrhal affection, chiefly of the respiratory passages. He furnished me with two such animals: one that after an illness of several weeks had died, another that was sent to me in a highly emaciated state, affected with severe broncho-pneumonia; this animal was paralyzed on the hind limbs. In both instances the *post-mortem* examination showed severe lung disease, broncho-pneumonia, and large white kidneys due to fatty degeneration of the entire cortex. A similar condition is met with in the human subject in diphtheria. Further, I received from Dr. Thursfield, of Shrewsbury, the body of a cat that had died after a few days' illness from pneumonia in a house in which children were ill with diphtheria; another cat in the same house that became next ill with the same lung trouble also succumbed. The *post-mortem* examination of the animal that I received showed severe broncho-pneumonia and large white kidneys, the entire cortex being in a state of fatty degeneration.

Subcutaneous inoculations of cats were carried out with particles of fresh human diphtheritic membranes and with cultures of the diphtheria bacillus (Report of the Medical Officer of the Local Government Board, 1889-90); hereby a local diphtheritic tumour was produced at the seat of inoculation, and a general visceral disease; in the cases in which death followed after a few days the lungs were found much congested; when death followed after one or more weeks, the lungs showed broncho-pneumonia and the kidneys were enlarged and white, the cortex being in a state of fatty degeneration; if the disease in the animals lasted beyond five to seven days, both kidneys were found uniformly white in the cortex; if of shorter duration, the fatty degeneration was sometimes only in patches. Although in these experiments the bacillus diphtheriæ was recoverable by cultivation from the diphtheritic tumour at the seat of inoculation, there were no bacilli found in the lungs, heart's blood, or kidney, and the conclusion is justified that, just as in the human diphtheria and in the diphtheria produced by subcutaneous inoculation in the guinea-pig, so also in these experimental cats the visceral disease must be a result of the action of a chemical poison produced by the diphtheria bacillus at the seat of inoculation.

From this it is seen that the similarity between the artificial disease and the natural disease in the cat is very great, and the question that presents itself is, In what manner does the animal receive or give the diphtheritic contagium in the natural disease? The natural disease in the cat is in its symptoms and pathology a lung disease, and it is reasonable to suppose from analogy that the lung is the organ in which the diphtheritic process in the cat has its seat. The microscopic examination of the diseased lung,

[1] Paper read before the Royal Society by Dr. E. Klein, F.R.S., on May 22. This research was undertaken for the Medical Department of the Local Government Board, and was communicated to the Royal Society with the permission of the Medical Officer.

of cats that died from the natural disease bears this out, the membrane lining the bronchi in the diseased portions of the lobules presenting appearances which in microscopic character coincide with the appearances in the mucous membrane of the human fauces, pharynx, or larynx in diphtheria. But the correctness of the above supposition, that diphtheria has its seat in the lung of the cat naturally diseased, was proved by direct experiment. Broth culture of the bacillus diphtheriæ was introduced into the cavity of the normal trachea without injuring the mucous membrane. The animals became ill with acute pneumonia, and on *post-mortem*, two to seven days after, there was found extensive pneumonia, and fatty degeneration of the kidney. The bronchi, infundibula, and air-cells of the inflamed lobules were found occluded by, and filled with, exudation, which under the microscope bears a striking resemblance to human diphtheritic membranes, and in the muco-purulent exudation in the large bronchi and trachea the diphtheria bacilli were present in large numbers.

During the last ten or twelve years certain epidemics of diphtheria have occurred which were traced to milk, but the manner in which that milk had become contaminated with the diphtheritic virus could not be demonstrated, although the evidence as to the milk not having been directly polluted from a human diphtheria case was very strong. The epidemic of diphtheria that prevailed in the north of London in 1878, investigated by Mr. Power for the Local Government Board, then the epidemic that occurred in October 1886 at York Town and Camberley, the epidemic in Enfield at the beginning of 1888, and in Barking towards the autumn of 1888, were epidemics of this character. Mr. Power, in his Report to the Local Government Board on the York Town and Camberley outbreak, states (p. 13) that a veterinary surgeon had certified that the cows from whom the infected milk was derived were all in good health, but that two of the cows showed "chaps" on their teats, and he adds that even two or three weeks after the epidemic had come to an end —the use of milk having been in the meanwhile discontinued—he saw at the farm one cow which had suffered chapped teats. At Enfield a veterinary inspector had also certified that the cows were in good health; but at Barking the veterinary inspector found sores and crusts on the udder and teats of the cows.

I have made experiments at the Brown Institution on milch cows with the diphtheria bacillus, which appear to me to throw a good deal of light on the above outbreaks of diphtheria.

Two milch cows[1] were inoculated with a broth culture of the diphtheria bacillus derived from human diphtheria. In each case a Pravaz syringeful was injected into the subcutaneous and muscular tissue of the left shoulder. On the second and third days there was already noticed a soft but tender swelling in the muscle and the subcutaneous tissue of the left shoulder; this swelling increased from day to day, and reached its maximum about the end of the week; then it gradually became smaller but firm. The temperature of both animals was raised on the second and third day, on which days they left off feeding, but after this became apparently normal. Both animals exhibited a slight cough, beginning with the eighth to tenth day, and this gradually increased. One animal left off feeding and ruminating on the twelfth day, "fell in" considerably, and died in the night from the fourteenth to fifteenth day; the other animal on the twenty-third to twenty-fourth day left off taking food, "fell in" very much, and was very ill: it was killed on the twenty-fifth day.

In both animals, beginning with the fifth day, there appeared on the skin of the udder, less on the teats, red raised papules, which in a day changed into vesicles, surrounded by a rim of injected skin; the contents of the vesicles was a clear lymph, the skin underneath was much indurated and felt like a nodule; next day the contents of the vesicle had become purulent, *i.e.* the vesicle had changed into a pustule; in another day the pustule dried into a brownish-black crust, with a sore underneath; this crust became thicker and larger for a couple of days, then became loose, and soon fell off, a dry healing sore remaining underneath. The whole period of the eruption of papules, leading to vesicles, then to pustules, and then to black crusts, which, when falling off, left a dry healing sore behind, occupied from five to seven days. The eruption did not appear in one crop: new papules and vesicles came up on the udder of one cow almost daily between the fifth and eleventh day after inoculation, in the other cow between the sixth and tenth day; the total number of vesicles in the fomer cow amounted to about twenty-four on the udder, four on the teats; in the latter they were all on the udder, and amounted to eight in all. The size of the vesicles and pustules differed: some were not more than $\frac{1}{8}$th of an inch, others larger, up to $\frac{1}{2}$-$\frac{3}{4}$ of an inch in diameter; they had all a rounded outline, some showed a dark centre. From one of the above cows on the fifth day milk was received from a healthy teat, having previously thoroughly disinfected the outside of the teat and the milker's hand; from this milk cultivations were made, and it was found that thirty-two colonies of the diphtheria bacillus without any contamination were obtained from one cubic centimetre of the milk.

[1] The cows had been kept under observation previous to the experiment for ten days, and were in all respects perfectly normal.

Unlike in the human, in the guinea-pig and in the cat the diphtheria bacillus passed from the seat of inoculation into the system of the cow; this was proved by the demonstration of the diphtheria bacillus in the milk. But also in the eruption on the udder, the presence of the diphtheria bacillus was demonstrated by microscopic specimens and particularly by experiment. With matter taken from the eruption—vesicles and pustules—of the udder, two calves were inoculated into the skin of the groin; here the same eruption made its appearance: red papules, rapidly becoming vesicular, then pustular, and then became covered with brown-black crusts, which two or three days after became loose and left a dry healing sore behind. More than that, the calves that showed this eruption after inoculation became affected with severe broncho-pneumonia and with fatty degeneration of the cortex of the kidney. In the two cows above mentioned, on *post-mortem* examination, both lungs were found highly congested, œdematous, some lobules almost solid with broncho-pneumonia in the upper lobes and the upper portion of the middle or lower lobe respectively; the pleural lymphatics were filled with serum and blood. Hæmorrhages in the pericardium and lymph glands, and necrotic patches were present in the liver. At the seat of inoculation there was in both cases a firm tumour consisting in necrotic diphtheritic change of the muscular and subcutaneous tissue. In this diphtheritic tumour continuous masses of the diphtheria bacillus were present; their gradual growth into and destruction of the muscular fibres could be traced very clearly.

It appears then from these observations that a definite disease can be produced in the cow by the diphtheria bacillus, consisting of a diphtheritic tumour at the seat of inoculation with copious multiplication of the diphtheria bacillus, a severe pneumonia, and necrotic change in the liver; the contagious nature of the vesicular eruption on the udder and excretion of the diphtheria bacillus in the milk prove that in the cow the bacillus is absorbed as such into the system.

From the diphtheritic tumour, by cultivation, pure cultures of the diphtheria bacillus were obtained; a small part removed from the tumour with the point of a platinum wire, and rubbed over the surface of nutrient gelatine or nutrient agar, yielded innumerable colonies of the diphtheria bacillus without any contamination. In cultural characters in plate, streak, and stab cultures and in cover-glass specimens of such cultures, this cow diphtheria bacillus coincided completely with the human diphtheria bacillus, but in sections through the diphtheritic tumour of the cow a remarkable difference was noticed between it and the bacillus from the cultures; inasmuch as in the tissue of the tumour the masses of the microbe, both in the necrotic parts, as also where growing into and destroying the muscular fibres, were made up of filaments, granular threads, some of which possessed terminal oval or flask-shaped swellings. But that it was really the diphtheria bacillus was proved by culture experiments and by cover-glass specimens. In the latter, the transitional forms between typical diphtheria bacillus and long filaments with terminal knob-like swellings, with spherical or oblong granules interspersed here and there in the threads, could be easily ascertained. In the large number of cultivations that were made of the fresh tumour in both cows, the colonies obtained were all of one and the same kind, viz. those of the diphtheria bacillus; no contamination was present in any of the cultivations.

Appendix, *May* 20.—At the beginning of the month of April two cats died at the Brown Institution, after having been ill for several days, with symptoms like those of natural cat diphtheria. Between the beginning of April and the beginning of May, 14 cats became similarly affected, some more severely than others, and some died with the characteristic morbid change This epidemic, as it may be called, commenced with the illness of the

first two cats about the end of March; and the question arises as to how the disease originated in these two animals. No cats had been ill in their shed, and the two affected ones were healthy when received at the institution some weeks before. But during the latter half of March there were in the stables of the institution two milch cows ill with diphtheria induced by inoculation with the human diphtheria bacillus—in fact, the two cows already referred to. The diphtheria bacillus was found in the milk drawn from one of these animals on the fifth day after inoculation, and orders were given to the attendant that the milk of both cows was to be thrown away. This order was not obeyed, for part of the milk was given to the two cats above mentioned, and they sickened as described within a day or two afterwards. It ought to be mentioned that the man in attendance on the cows had also charge of the cats, but, in view of the fact that he was himself free from the disease, the possibility of his having conveyed it from the cows to the cats may be disregarded.

SOME NOTES FROM SOUTH AMERICA.

IN the course of a visit to the plains of South America, not far from Rosario de Santa Fe, in the Argentine Republic, lasting from September 1888 until March 1889, I was able to make some miscellaneous notes of more or less interest. From these I select the following:—

(1) *The Rhea, or South American Ostrich.*—The cock bird makes the nest, hatches the eggs, and takes care of the young birds. We had some (so-called) "tame" ostriches about the *estancia.* One day I came across the old cock in a nest that it had made in the dry weeds and grass. Its wings and feathers were loosely arranged, and looked not unlike a heap of dried grass; at any rate the bird did not attract my attention until I was close on him. The long neck was stretched out close along the ground; the crest-feathers were flattened; and an appalling hiss greeted my approach. It was a pardonable mistake if for a moment I thought I had come across a huge snake, and sprang back hastily under this impression. This *might* be cited as an instance of (unconscious) "protective mimicry."

When a troop of these birds is alarmed while yet at a distance from the enemy, they run with their wings either close to the side in the normal position, or raised above the back into a narrow wedge that offers but little resistance to the air. But when a bird is somewhat pressed, it usually droops the wings loosely, almost trailing them. And when in danger of being caught by dogs, or struck by the *bolas* of a horseman, it begins to dodge and twist in a very curious manner, the wings assuming various positions. It would seem as though the wings, thus used, may help the bird to make its sudden halts and turns; and also, when dogs are used in the chase, to baffle the attacks of these enemies. It was very curious to see the "tame" ostriches indulging in these freaks even when unpursued by the dogs of the *estancia.* The birds would rush straight along, turn and twist, contort their necks into very comical shapes, jump, and not unfrequently tumble over in their efforts to perform some unusually complicated evolution.

I may add that in the course of some years of "ostrich-running," my brother once observed a troop of these birds swim a river that crossed their path. He himself followed, and found that the river was really out of their depth; they were not wading.

(2) *Snakes: the "Vivora de la Cruz."*—On October 6, 1888, we came across one of these common poisonous snakes, probably not long roused from its winter torpor. The dogs stood round it barking; and it remained, threatening a strike. With its tail (and against the grass?) it made a very distinct though not a loud burring, vibrating sound. This, my brother told me, was usual. Yet there is no kind of "rattle" on the tail.

At the end of March 1889, after I had returned to England, my brother killed a large *vivora de la cruz;* and, observing that it appeared to be very thick in the body, he cut it open in order to examine it. Inside was a string of transparent bags, six or seven in number, connected with one another. In each of these could be seen a fully-formed young snake, about 6 inches long, as far as he could say without exact measurement, coiled up. Two of the bags he cut open; and the young snakes, released, both threatened to strike anything that approached them, and made, though of course on a very small scale, the vibrating, burring noise with the tail.

(3) *The Intelligence (?) of Ants.*—One kind of small ant, if not more, makes large nests underground, in the shorter grass. A network of paths, clear of grass, about 2 inches wide near the nest, converge towards this latter; being the roads by which the ants bring home forage. These paths are of all lengths from 10 yards up to 100 yards; and, as one traces them further from the nest, they break up into smaller branches and are finally lost. As a general rule, one may say that streams of ants carrying leaves, buds, flowers, seeds, and other valuable odds and ends are always moving towards the nest, while empty-mouthed ants are meeting and passing them on their outward journey to the foraging grounds. Having, however, noticed a few burdened ants proceeding with great difficulty against the general stream of their burdened fellow-citizens, I tried the experiment of turning some of these carriers round when they had nearly reached home. The general conclusion I came to was that these ants did not then understand in what direction the nest lay, nor did they (as far as I could see) draw any conclusions from the fact that they now *met* the stream of carriers with which they had previously been travelling.

Thus, one ant, carrying a (relatively) huge burden, I reversed in direction when already near the nest. I then followed it for about 8 yards (or about 20 minutes of time as far as I can say) in its mistaken reversed course away from the nest. Though it met and collided with quantities of burdened ants, and was passed in the same direction as its own by unburdened ants only, it did not seem to take the hint. Its final return home was the result of accident, as far as I could tell; it having got up "the right way round" after a severe fall.

Still it must be noticed that among the undisturbed ants very few went the wrong way.

I dug a hole in one of the paths, on several occasions. The hole was small; and it was easy, though not so convenient, to go round by the side over the very short grass. Nevertheless it required the falling of very many ants into the hole, and the leaving of quite a pile of leaves there, before the stream learned to pass about about one inch to one or other side, and so to avoid the pitfall. Some ants even turned back; and I left them carrying their burdens back to the foraging grounds again.

(4) *Grasses.*—I noticed two grasses concerning whose seeds a remark or two may be of interest.

(α) One is called "*Flechilla.*" Its seed bears a very sharp point; and a number of hairs, turned back from the point, prevent the return of the seed from any body into which it has penetrated. Attached to the seed is always a piece of stem curiously twisted like rope. The whole answers somewhat to an arrow-head with barbed point and with shaft attached. My brother, whose observations extend over more than twenty years, tells me that this seed penetrates into the bodies of sheep, and is found in their internal organs. The spring lambs, which are left unshorn until summer is over, are especially troubled with the flechilla. When one of these animals dies and is skinned, it is very commonly found that quantities of these seeds have penetrated the skin, the heads being found in the flesh underneath; and my brother has found them in the liver. It is believed that this is the cause of some deaths among the animals.

(β) Another remarkable grass is the "*Paja voladora.*" This grows in tufts, not unlike those of a small "Pampas grass." From slender stems there stand out still slighter branches, at the end of which are the seeds; the whole, stem and seeds, having somewhat the appearance of a miniature fir-tree as regards shape, and having various lengths up to 2 feet or so. When ripe, these stems with the seeds blow bodily away in the first strong wind. I have seen them flying through the air, looking from afar rather like a dust-cloud against the sky; and half rolling, half drifting, over the living grass of the plain, before the sudden onset of a *tormenta* (storm with wind). This drifting movement over the grass had a curiously bewildering effect on the eye. When the storm is over, the grass is found in drifts against the posts and wires of the fences; these collections remind one strongly of snow-drifts.

On December 16 (or so), 1888, a terrible accident occurred on the railway between Candalaria and Guardia de la Esquina. A cutting had become filled with this *Paja voladora;* and the engine set fire to it as it passed. However, thanks mainly to a suitable wind, the train got safely through. But in the afternoon of the same day the train re-passed in the opposite direction, and the cutting had in the meantime become filled again with

these seed-stems. The wind was, in this return journey, in such a direction as to favour the tendency of the burning grass to set fire to the carriages. They caught fire. About eight persons were totally consumed, hardly even bones being found to tell the tale; and eighteen escaped up the banks in a pitiable condition. I do not know how many of these are now alive.

W. LARDEN.

ON THE PROPERTIES OF LIQUEFIED GASES

M. E. MATHIAS has just published in the form of an inaugural thesis (Gauthier-Villars, Paris), an important investigation on the latent heat of vaporization of liquefied gases. The value of this coefficient for sulphurous acid, carbonic acid, and nitrous oxide was determined experimentally throughout a considerable range of temperature by the following method. The gases were first liquefied in a small copper cylinder, 9 cm. in height, 3 cm. in diameter, with walls 0·38 cm. in thickness. The cylinder was then weighed and introduced into an ordinary Berthelot calorimeter, and the liquefied gas was allowed to evaporate slowly, the pressure being constantly read off on a Bourdon gauge and regulated by means of two conical screw taps. The calorimetrical method employed was a null one (devised by the author), the heat absorbed by the evaporation of the liquid being compensated for by adding sulphuric acid of known strength to the water in the calorimeter, at such a rate as to keep its temperature approximately constant. The total amount of heat absorbed was thus easily determined, while the correction for cooling was reduced to a minimum. When necessary the laboratory was heated by means of regulated gas-burners, so that the liquid in the calorimeter and the surrounding atmosphere were at the same temperature in all cases. Seven experiments on liquid sulphurous acid between the temperatures of $+5°·74$ and $+19°·95$ gave values for L, the latent heat of vaporization, which may be expressed by the empirical formula—

$$L = (91·87 - 0·384t - 0·000340t^2) \text{ Cal.}$$

Nineteen experiments on liquid carbonic acid between $+6°·65$ and $+30°·82$ may be expressed by the formula—

$$L^2 = [117·303\,(31 - t) - 0·466\,(31 - t)^2] \text{ Cal.}$$

The numbers obtained afford a satisfactory verification of Clapeyron's formula, as calculated from constants previously determined.

Owing to the great difficulty of obtaining nitrous oxide free from nitrogen, the results obtained with this gas can only be regarded as qualitatively correct. But the graphic representation of fifteen experiments, performed between the temperatures of $+5°·37$ and $+34°$, show that the curve representing the variation of the latent heat with the temperature is exactly of the same form for nitrous oxide as for carbonic acid. In both cases the tangent to the curve at the critical point is rigorously perpendicular to the temperature axis. We may from this conclude that at the critical point L is rigorously equal to zero, and hence from Clapeyron's formula that the specific volume of the liquefied gas and its saturated vapour are rigorously equal, a fact questioned lately by Cailletet and Colardeau. It follows, moreover, from the thermodynamical equation—[1]

$$m' = m + dL/dt - L/T,$$

where m' is the "specific heat of the saturated vapour"—that at the critical point,

$$m' = -\infty.$$

Now at the temperatures at which experiments have been made on ordinary liquids (water, ether, acetone, &c.), m', though negative, was found to increase with rise of temperature; but since we may conclude from these experiments that near the critical point it will decrease, it follows that it must at some intermediate temperature pass through a maximum. It follows also that if m', while increasing, pass through a zero value, that it will pass through a second zero value in the opposite sense. As m' for carbonic acid and nitrous oxide is negative and decreasing between $-50°$ and $+30°$, if a zero value exist for these gases, it must be at a very low temperature. In the case of sulphurous acid, the results obtained were shown to confirm certain formulæ given by Bertrand in his "Thermodynamique," and applicable to saturated vapours at temperatures much below the critical point. The memoir as a whole is masterly.

[1] See Verdet, "Théorie Mécanique de la Chaleur," i. p. 258.

NOTES ON INDIAN INSECT PESTS.

THE Government of India has evidently begun to realize the importance of the study of injurious insects, and the methods of combating them. Some time ago, Mr. E. C. Cotes, of the Indian Museum, published the first two numbers of "Notes on Economic Entomology," dealing respectively with the wheat and rice weevil in India, and with insecticides and the methods of applying them. This series has been discontinued, and its place has been taken by "Notes on Indian Insect Pests."

The first number of the new series contains notes on the Rhynchota, by Mr. Atkinson, the most detailed of which deals with the rice sapper (*Leptocorisa acuta*). These insects settle on the rice ear, sometimes to the number of ten on one ear, and, extracting the milky juice of the grain, leave the husk dry. Unfortunately, nothing is known of the life-history of this pest. Mr. Atkinson also describes a new genus and species of Coccidæ (*Pseudopulvinaria sikkimensis*), found on the under surface of the leaves of oaks, chestnuts, and cinchona: hitherto this has not appeared in sufficient numbers to effect much damage.

Mr. De Nicéville contributes an account of two injurious butterflies: one, which he identifies as *Suastus gremius*, devours the young and tender rice-shoots in the paddy-fields, but fortunately avoids the more mature plants. As a preventive for this pest, Mr. De Nicéville recommends raising the earthen walls round each plantation, so as to completely submerge the rice; this would prove fatal to all stages in the life-history of the butterfly, with the doubtful exception of the egg. The other report deals with *Lampides elpis*, whose larva devours the buds and young fruit of the Cardamom. Many allied forms have a gland on the eleventh segment, secreting a sweet fluid, much sought after by ants, but this seems to be absent in the species here described.

The remainder of the number is made up of thirteen reports and numerous short notes contributed by Mr. Cotes. Two of these reports are extensions of his "Notes on Economic Entomology," mentioned above. No mention is made of the Strawsonizer, whose value as a disseminator of insecticides has recently been recognized in England. The remaining eleven deal with caterpillars which attack the tea plant, sâl trees, rice, *Cedrela toona*, sugar-cane, sorghum, and the blankets belonging to the Army Clothing Department, and with various species of beetle injurious to the rice, bamboo, mango, and shorea tree. The beetle, which lives under the bark of the last-mentioned tree, closely resembles the *Tomicus chalcographus*, which is so injurious to the spruce in Europe.

The notes which conclude the work are often very fragmentary, but are full of suggestion, and there is no room to doubt that the lacunæ in the life-history of the insects with which they deal will soon be filled up, now that the authorities of the Indian Museum have provided a journal which will prove a means of intercommunication between the numerous entomologists and cultivators scattered over India.

The number is illustrated by four good plates of photo-etchings executed in Calcutta.

A. E. S.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—An elector (who must be a Head of a College) to the Sadlerian Professorship of Mathematics will be elected by the resident members of the Senate on June 3, from 1 to 2 p.m. The vacancy is caused by the death of Dr. Phelps, Master of Sidney Sussex College.

A. R. Forsyth, F.R.S., the author of the well-known "Treatise on Differential Equations," has been approved for the degree of Doctor in Science.

Dr. Hill, Master of Downing College, announces a class in Practical Histology, to be held during the Long Vacation.

The General Board of Studies recommend that the stipends of the following teachers of science be increased:—Mr. Gadow, King's College, Lecturer in Advanced Morphology, from £100 to £150; Mr. Marr, St. John's College, Lecturer in Geology, from £100 to £150; Mr. Harker, St. John's College, Demonstrator in Geology and Lecturer in Petrology, from £100 to £150; Mr. Barber, Christ's College, Demonstrator in Botany, from £100 to £150; an additional Demonstrator in Chemistry at a stipend of £100.

A conference on the local lectures under the University

Extension Scheme is to be held in Cambridge on July 9 and 10. Courses of lectures and practical work are to be arranged for students for local centres who are to reside in Cambridge during the month of August.

A very interesting report on the progress of the Extension movement by Dr. R. D. Roberts appears in the *Cambridge University Reporter* of May 27. The account he gives of the enthusiasm and energy displayed by certain of the Students' Associations attending Extension lectures is most encouraging, and shows how wide-spread is the influence for good exerted by the University in this connection.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for April 1890 contains papers on *Phymosoma varians*, Selenka, by Arthur E. Shipley (plates i. to iv.). The material for this paper was collected by Mr. Weldon in the Bahama Islands, where the species was fairly abundant in the soft coral rock. The general morphology and minute structure of this animal are described in great detail, and accompanied by some excellent illustrations; the head of Phymosoma is surrounded by a stiffened vascular horseshoe-shaped lip, the dorsal ends of which are continuous with the ends of a hippocrepian lophophor, which bears a crown of about eighteen tentacles—the number being always even; between this lophophor and the vascular lip is the crescentiform opening of the mouth. The author would keep the genus Phoronis as a form closely allied to the more normal *Gephyrea inermia*, and compares the head of Phymosoma as seen from above with a view of Phoronis.—On the spinning apparatus of the geometric spiders, by C. Warburton (plate v.). Proves by a series of interesting experiments that a spider's line does not consist of many strands fused or woven together, but ordinarily of two or four distinct threads; the ground line of the spiral is double only, and the two strands are bound together merely by the viscid matter, which envelops them.—On the structure and functions of the cerata or dorsal papillæ in some Nudibranchiate Mollusca, by Prof. W. A. Herdman (plates vi. to x.). In some six genera of British Nudibranchs examined, Herdman found that the dorsal papillæ, "cerata" of Lankester, were of two kinds—(1) those containing diverticula of the liver, as in the cases of Eolis and Doto, (2) those which were essentially but processes of the body-wall having no connection with the liver, as in Tritonia, Ancula, and Dendronotus. In Doris there are true branchiæ and no cerata. In Ancula both branchiæ and cerata are present. In Tritonia and Dendronotus there are cerata but no true branchiæ. In Doto and Eolis there are no true branchiæ. Morphologically all the forms of cerata are probably epipodial processes; they are not of primary importance either in respiration or in digestion, but give to the animals, by their varied shape and colours, appearances which are in some cases protective and mimetic, and in others conspicuous and warning, as may be best suited to the individual surroundings and mode of life.—Further observations on the histology of striped muscle, by C. F. Marshall (plate xi.).—On Chætobranchus, a new genus of Oligochætous Chætopoda, by Dr. A. G. Bourne (plate xii.). This remarkable worm was found in the mud from a "tank" in Madras town; it is furnished with a remarkable series of branchial processes, dorso-laterally placed—a pair to each of the anterior segments, commencing with the second segment; these processes completely surround a portion of the dorsal setæ bundles. The species has been named *Chætobranchus semperi*.—On the presence of Ranvier's constrictions in the spinal cord of Vertebrates, by Dr. W. T. Porter, of St. Louis (plate xii. *bis*).—A note to the editor from Prof. Bütschli, of Heidelberg, giving an account of his experimental imitation of protoplasmic movements. These protoplasma-like streaming properties of minute globules of a specially treated olive oil are of extreme interest.

American Journal of Science, May 1890.—Experiments with a pendulum-electrometer, illustrating measurements of statical electricity in absolute units, by Alfred M. Mayer. The apparatus described affords an inexpensive and ready means of presenting clearly to a class the nature of measurements of statical electricity in absolute units; the instrument may be made to measure to the $\frac{1}{10}$ of a dyne, and a series of experiments are given to show that it gives the law of inverse squares, serves to determine the law of dissipation of an electric charge, and that it allows measures to be made of electrical distribution on conductors and the determination of quantity and potential.—On electric potential as measured by work, by the same author. A graphical illustration is given of the fact that in the case of two electrified spheres the potential function is a measure of work.—An elementary proof of the earth's rigidity, by Geo. F. Becker. It is proved that a simple strain spheroid affords an approximation to the deformation of an elastic globe sufficiently close to serve as a basis for Sir William Thomson's demonstration of the rigidity of the earth; the whole subject also being presented in a clear and elementary manner.—On the hornblende of St. Lawrence County, N.Y., and its gliding planes, by George H. Williams. From the evidence brought forward it is concluded that an alteration of the symbols for the terminal planes of hornblende is necessary to show its analogy to pyroxene; and that this change must be made in accordance with the assumption that the gliding plane, now called the orthodome $P\overline{\infty}$ ($\bar{1}01$) is the basal pinacoid OP (001) as suggested by Tschermak in 1884.—Note on some secondary minerals of the amphibole and pyroxene groups, by Whitman Cross. In the course of the microscopical examination of some rocks from Custer County, Colorado, the author has observed two peculiar minerals of secondary origin, one an amphibole, and the other a pyroxene, and now describes their unusual properties, relationships, and mode of formation.—On spangolite, a new copper mineral, by S. L. Penfield. The specimen examined consisted of a rounded mass of impure cuprite mostly covered with hexagonal crystals of the new mineral. A full description of the habit, optical and physical properties, and chemical composition of the crystals is given.—Archæan axes of Eastern North America, by James D. Dana. The partly or wholly Archæan ranges in New England and Canada parallel and to the east of the Appalachian *protaxis* are described, and the geological importance of the included troughs or basins pointed out.—On the metamorphic strata of South-Eastern New York by Frederick J. H. Merrill.—The radiant energy of a standard candle; mass of meteors, by C. C. Hutchins. The whole radiant energy of the candle used was found to be $1 \cdot 23 \times 10^8$ ergs per second, and the radiant energy of the visible part $2 \cdot 46 \times 10^6$ ergs per second. The author also points out how such measures may be used to determine the mass of meteors.—Meteoric iron from North Carolina, by L. G. Eakins.—Distinctive characters of the order Hallopoda, by O. C. Marsh.—Additional characters of the Ceratopsidæ, with notice of new Cretaceous Dinosaurs, by the same author.

Botanische Jahrbücher, von A. Engler, vol. xi., contains the following papers:—An essay on the biological relations of the flower of *Aconitum*, by Dr. M. Kronfeld. He states that *Aconitum* is an excellent example of a flower adapted to a certain insect, and that it is dependent upon *Bombus* for its fertilization, a fact which is further borne out by a comparison of the geographical area of the two, that of *Aconitum* being entirely covered by the area of distribution of *Bombus*.—Dr. O. Drude, on the principles of distinction of the formations of vegetation (*Vegetationsformationen*) as illustrated by the flora of Central Europe.—A description, by L. Wittmack, of the plants belonging to the *Bromeliaceæ*, collected by Herr F. C. Lehmann in Guatemala, Costarica, Columbia, and Ecuador.—A description of new species of Nyctaginaceæ, by Dr. A. Heimerl, with one plate.—A monographic sketch of the genus *Helleborus*, by Dr. V. Schiffner.—A contribution to the knowledge of the distribution of the Scotch fir in Northern Germany, in which it is stated that on the mainland it extends north of the Elbe as a native plant, only as far as a line connecting Rostock, Schwaan, Güstrow, Wittenburg, and Geesthacht; in North-West Germany it is native only in the Upper Harz.—An anatomical investigation of the foliage leaves of the Arbutoideæ and Vaccinoideæ in relation to their systematic grouping, and geographical distribution leads Dr. Franz Niedenzu to the following conclusions: that the Arbuteæ are the oldest type, and of them more especially *Arbutus* and *Arctous*, while *Arctostaphylos* is more recent; the most recent group is the *Thibaudieæ*. These results are based upon details of the glandular and other hairs, of the teeth of the leaf, the epidermis and cuticle (130 pp. and 4 plates).—On the influence of the mean direction of the wind on the vegetation in the water, with references also to other phenomena of vegetation which depend upon the direction of the wind in the Western Baltic, by M. J. Klinge.—On a new *Potentilla* from Central America, by Dr. K. Fritsch.—Contributions to the knowledge of the Amaryllidaceæ, by Dr. F. Pax.—A list of the wild plants of the province of Wologda, by N. A. Ivanitzky.—On the

anatomical characters of the Hamamelidaceæ, examined with the object of using them as a basis for the systematic arrangement of the family, by A. Reinsch (1 plate).—A list of the Polypodiaceæ, Gramineæ, Cyperaceæ, and Juncaceæ, collected by Dr. Marloth in South Africa.—On *Cassine domingensis* Spr., by A. Garcke.—A treatise on the genus Platanus, with two plates, by J. Jankó. This paper has special reference to the detail characters of the leaf.—On two *Soldanellas* new to the flora of Hungary, by V. A. Richter.—At the end of the volume are abstracts of many recent memoirs published elsewhere, and a classified list of the most important works on systematic, geographical, and descriptive botany published in the year 1889.

Bulletin de la Société des Naturalistes de Moscou, 1889, No. 2.—On the origin of periodical comets, by Th. Bredichin (in French). The author examines into the cases of division of comets into two or more individuals, endeavouring to classify the better known ones into "families," and gives the formulæ for the cases when the impetus given to the corpuscles of a comet acted under a given angle to the plane of its motion.—Note on the genus *Bombus*, by General Radoszkowski (in French).—The Amphibian fauna of Europe: the *Anura*, by Dr. J. Bedriaga, being a full description (in German) of the two genera *Rana* and *Bufo*, their species, varieties, life, and geographical distribution.

No. 3.—On the modes of propagation of fresh-water fishes, by S. Nikitin (in French). M. Nikitin objects to the too hasty generalizations sometimes arrived at by men of science (especially with regard to Central Asia) as to the former communication between distant fluviatile basins and lakes which now have some species of fishes in common. He points out the possibility of the transport of the eggs of certain fishes by birds, and mentions the fact of young pikes, from six to ten centimetres long, being found in small temporary ponds on the banks of the Moskva river, where they could by no means have migrated themselves. Further inquiry is asked for.—The transport of electrical energy, by J. Weinberg (in German).—On the nesting of *Podoces Panderi*, by N. Zarudny.—The Amphibian fauna of Europe: the *Anura* (continued), by Dr. J. Bedriaga. The genera *Hyla*, *Pelobatus*, *Pelodytes*, *Discoglossus*, *Bombinatur*, and *Alytes*, are considered, and the author describes two new varieties of *Hyla arborea* under the names of var. *orientalis* and var. *Molleri*.—On the influence of weather upon plants and animals, by Alex. Becker (in German).

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 8.—"Experiments on Vapour-Density." By E. P. Perman, B.Sc. Communicated by Prof. Ramsay, F.R.S.

Vapour-density of Bromine.—This work was undertaken in order to see if the results of Prof. J. J. Thomson's experiments could be verified; these appeared to show that bromine vapour dissociated on continued heating at a low pressure, and a comparatively low temperature. The method used was a modification of the Dumas method, by which a series of vapour-density determinations were made at different pressures, with the same identical material. The chief conclusions arrived at are (1) that no dissociation takes place at temperatures as high as 280°, and pressures as low as 20 mm., even on continued heating; (2) that bromine vapour has no tendency to form molecules with more than two atoms, on approaching the liquid state.

Vapour-density of Iodine.—The density of saturated iodine vapour was determined by an adaptation of Kundt's method of determining the velocity of sound in gases. The mean result was 126·9, showing that liquid iodine has the formula I_2.

Induction Spark through Iodine Vapour.—The same apparatus was used as in finding the vapour-density of iodine. No alteration of the wave-length in the iodine vapour (as indicated by the heaps of finely-divided silica on the lower part of the tube) occurred on passing a series of sparks, and then causing the glass piston to vibrate. Sparking does not appear, therefore, to produce permanent dissociation of iodine vapour, notwithstanding the results pointing to a contrary conclusion, obtained by Prof. Thomson. Neither bromine nor iodine vapour, when saturated, threw the silica into heaps; it appears that sound-waves cannot be propagated in a saturated vapour, for condensation will be produced either by the waves of compression or those of expansion (according to the nature of the vapour, and its temperature) and the rates of propagation of the two sets of waves will therefore be different.

Vapour-densities of Sulphuric Anhydride, and Aqueous Hydrochloric Acid.—These were determined by the same method as the vapour-density of bromine. The vapour-density of sulphuric anhydride indicated a formula SO_3, and that of aqueous hydrochloric acid showed that it is a mixture of molecules HCl and H_2O, and not a compound. In all these experiments the quantity of substance in the globe was not found by weighing, but by estimating it volumetrically, portions being drawn off and absorbed in a suitable liquid. The globe was heated by means of a vapour-jacket; the vapours used were those of alcohol, chlorobenzene, bromobenzene, and bromonaphthalene.

The author is greatly indebted to Prof. Ramsay for constant advice and assistance in carrying out the work.

May 22.—"The Chemical Products of the Growth of *Bacillus anthracis* and their Physiological Action." By Sidney Martin, M.D., Pathologist to the Middlesex Hospital. Communicated by Dr. Klein, F.R.S.

The bacilli were grown in a solution of pure alkali-albumin (made from serum-proteids) and of mineral salts of the composition of the salts of the serum.

The cultivation of the bacilli was continued for ten to fifteen days, and the organisms removed by filtering through Chamberlain's filter. The filtrate contained the products of the bacterial growth, viz.:—

(1) *Proto-albumose* and *deutero-albumose*, and a *trace of peptone*: all with the same chemical reactions as the similar bodies formed in peptic digestion.

(2) *An alkaloid*.

(3) Small quantities of *leucin* and *tyrosin*.

The chief characteristic of the anthrax proto- and deutero-albumose is their strong alkalinity in solution—an alkalinity not removed by absolute alcohol, by benzene, chloroform, or ether, or by prolonged dialysis. Acid-alcohol dissolves from the alkaline albumoses a trace of a poisonous body, but this is not in proportion to the toxicity of the albumoses. The albumoses are precipitated in an alkaline condition by saturation with NaCl (proto-albumose) or $(NH_4)_2SO_4$. The alkaloid is soluble in absolute alcohol, amyl alcohol, and in water; insoluble in benzene, chloroform, and ether. It is strongly alkaline in solution, and a powerful base, readily forming salts with acids. The sulphate crystallizes in small needles or prisms; the oxalate in long, branching needles or flat plates. From the salts the alkaloid is easily regained. In solution, the alkaloid is precipitated by phosphotungstic, phosphomolybdic, and phosphoantimonic acids and platinic chloride, but not by potassio-mercuric iodide. It is slightly volatile, and, when kept exposed to the air, it becomes acid, and loses, to a great extent, its poisonous properties.

Physiological Action.

(1) The mixture of anthrax proto- and deutero-albumose is poisonous. In small doses it produces in mice a local subcutaneous œdema, with some sluggishness, ending in recovery. Larger doses produce a greater œdema with more signs of illness, sluggishness leading to prolonged stupor, coma, and death in twenty-four hours or longer. A fatal dose for a mouse of 22 grams weight is 0·3 gram (subcutaneously injected). In some cases the spleen is enlarged: no organisms being present, as shown by gelatine tube cultivations. Boiling for a short time diminishes the toxicity of the proteid, but does not completely destroy it, and death may result from the boiled albumoses.

(2) The *anthrax alkaloid* produces symptoms and lesions similar to the albumoses, but much more rapidly and severely. The animal becomes ill directly after the injection, gradually becomes more and more sluggish, and dies in coma, or, if a non-lethal dose be given, it recovers from the state of stupor gradually. After death, enormous local subcutaneous œdema is found, with congestion and sometimes thrombosis of the small veins. Peritoneal effusion is occasionally present, and the spleen is usually enlarged, dark, and congested, or simply congested without being greatly enlarged. The fatal dose for a mouse weighing 22 grams is between 0·1 and 0·15 gram, death occurring in two to three hours.

The anthrax bacillus in digesting the alkali-albumin forms (1) proto-albumose, (2) deutero-albumose, (3) an alkaloid. The alkalinity of the albumoses may explain their toxic properties, being due to the fact that the alkaloid is in a "nascent" condition in the albumose molecule. The bacillus forms the alkaloid

from the albumose, and it is possible that the living tissues have a similar action when the albumose is introduced into a living animal.

Entomological Society, May 7.—Captain Henry J. Elwes, Vice-President, in the chair.—Mr. H. Goss, the Secretary, read a letter from the Vicar of Arundel, asking for advice as to the course to be taken to get rid of the larvæ of a beetle which were destroying the beams of the parish church. Mr. C. O. Waterhouse said he had already been consulted on the question, and had advised that the beams should be soaked with paraffin oil.—Dr. Sharp exhibited specimens of *Caryoborus lacerdæ*, a species of *Bruchidæ*, and the nuts from which they had been reared. He stated that these nuts had been sent him from Bahia by the late Señor Lacerda, about six years ago, and that one of the beetles had recently emerged, after the nuts had been in this country for five years. Dr. Sharp also exhibited several specimens of Diptera collected by Mr. Herbert Smith in St. Vincent, and read a letter from him to Mr. Godman on the subject of the vast number of species of this order which he had recently collected in that island. Mr. McLachlan, F.R.S., Dr. Mason, Mr. Waterhouse, and Captain Elwes took part in the discussion which ensued.—Mr. R. F. Lewis, on behalf of Mr. W. M. Maskell, of Wellington, New Zealand, exhibited and read notes on about twenty-five species of *Coccidæ* from that colony. He also exhibited some specimens of the larvæ and imagos of *Icerya Purchasi*, Maskell, obtained from Natal, where the species had proved very destructive to orange, lemon, and other fruit-trees. He also showed specimens of the larvæ of an allied species from Natal, originally assigned by Mr. Douglas to the genus *Ortonia*, but which Mr. Maskell was inclined to regard as a new species of *Icerya*. Mr. McLachlan and the Chairman commented on the interesting nature of the exhibition, and the importance of a knowledge of the parasites of injurious insects, in connection with which special mention was made of the researches and discoveries of Prof. Riley.—The Secretary exhibited, on behalf of Mr. T. D. A. Cockerell, of Colorado, a large collection of insect-galls, and read a letter from Mr. Cockerell on the subject. Dr. Mason said he should be happy to take charge of these galls, with a view of rearing the insects and reporting the results.—Mr. W. H. Bates, F.R.S., communicated a paper entitled, "On New Species of *Cicindelidæ*."

Royal Meteorological Society, May 21.—Mr. Baldwin Latham, President, in the chair.—The following papers were read:—Rainfall of the globe, by Mr. W. B. Tripp. This was a comparative chronological account of some of the principal rainfall records. The earliest record is that of Paris, which commenced in 1689. The English records began in 1726. The rainfall observations in the southern hemisphere do not extend over a very long period; at Adelaide they were commenced in 1839, but they do not go back further than 1866 for New Zealand. The greatest fall in any particular year at the stations given by the author was 160·9 inches at St. Bernard in 1839, and the least 3 inches at Sandiego in California in 1863. By combining the stations in the northern and southern hemispheres the author finds that in recent times the years with the highest average rainfall were 1878, 1879, and 1883, and the years with the lowest average were 1854 and 1861.—Mutual influence of two pressure plates upon each other, and comparison of the pressures upon small and large plates, by Mr. W. H. Dines.—On the variations of pressure caused by the wind blowing across the mouth of a tube, by Mr. W. H. Dines. In these two papers the author gives the results of some experiments on wind pressure which he has made mostly on a whirling machine at Hersham, Surrey. From these experiments it seems probable that a decrease of pressure per square foot with an increase of size of plate may be taken as a general rule.

Geological Society, May 14.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—The so-called Upper Lias Clay of Down Cliffs, by S. S. Buckman. The blue clay of Down Cliff, Dorset, which has been referred to the Upper Lias, has yielded Ammonites of the genus *Dumortieria* to the author, notably *D. radians*. This blue clay is *below* the Yeovil Sands; but the position of *D. radians* in the Cotteswolds is in the limestone *above* the Cotteswold Sands, which has been placed in the Inferior Oolite series. The author, by combining the Down Cliffs and Chideock Hill sections, obtains a sequence of beds from the Middle Lias to the top beds of the Inferior Oolite, including the zones of *spinatum*, *commune*, and *falciferum*, *jurense*, *opalinum*, *Murchisonæ*, *concavum*, and *Parkinsoni*. The genus *Dumortieria* binds the *opalinum* and *jurense* zones together; while at Symondsbury Hill the author has found *Ludwigia Murchisonæ* and *Lioceras opalinum* in the same bed, which renders it difficult to draw a line of demarcation between Lias and Oolite at the top of the *opalinum* zone. The facts adduced in the paper furnish additional evidence of the untrustworthiness of a grouping which depends upon lithological appearances, and it was because no satisfactory line could be drawn between Lias and Oolite that the author, in a previous paper, supported the continental plan of grouping Upper Lias and part of the Inferior Oolite under the term Toarcian upon palæontological grounds. In the present paper he furnishes further statements in support of this view. After the reading of this paper some remarks were offered by Mr. H. B. Woodward, Mr. Hudleston, and the President.—On some new mammals from the Red and Norwich Crags, by E. T. Newton.—On burrows and tracks of Invertebrate animals in Palæozoic Rocks, and other markings, by Sir J. William Dawson, F.R.S.—Contact-alteration at New Galloway, by Miss M. I. Gardiner. Communicated by J. J. H. Teal.

Zoological Society, May 20.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. Gambier Bolton exhibited a series of photographs, principally of animals living at the Society's Gardens and in Mr. Walter Rothschild's menagerie.—Prof. Flower exhibited a photograph of a nest of a Hornbill (*Toccus melanoleucos*), taken from a specimen in the Albany Museum, Grahamstown, in which the female was shown "walled in."—A communication was read from Sir Edward Newton relating to the reported discovery of Dodo's bones in Mauritius in 1885, by the late Mr. Caldwell. It appeared that there had been some error in the matter, and that the bones discovered were not those of the Dodo.—Mr. Sclater, F.R.S., pointed out the characters of a new Toucan of the genus *Pteroglossus* from the Amazons, proposed to be called *P. didymus*.—Mr. R. Lydekker read a paper describing some bird-remains from the cavern-deposits of Malta. These remains indicated a Vulture larger than any existing species, which, from the characters of the cervical vertebræ, he referred to the genus *Gyps*, under the name of *G. melitensis*. They also comprised some bones of a crane, of the size of *Grus antigone*, for which the name *Grus melitensis* was proposed.—Dr. Hans Gadow gave an account of some cases of the modification of certain organs in Mammals and Birds which seemed to be illustrations of the inheritance of acquired characters.

EDINBURGH.

Royal Society, May 19.—After the reading of some obituary notices, Prof. Crum Brown read a paper written by himself and Dr. J. Walker on the synthesis of sebacic acid.—Prof. Tait communicated a note on some remarkable quaternion formulæ.—Dr. Alexander Bruce read a paper on the roots of the auditory nerve and their connections.

PARIS.

Academy of Sciences, May 19.—M. Hermite in the chair.—Experiments on the deformations experienced by a spheroidal envelope subjected to pressure; possible applications to the terrestrial globe, by M. Daubrée. From the experiments described in this and in a previous communication, it appears that the author has been able to produce in various spheroids configurations like those exhibited by the earth's crust. He finds that the southern parts of the three continental masses are not deviated towards the east because of the influence of the earth's rotation, but by the effect of simple torsion in a spheroidal heterogeneous envelope subjected to contraction, similar reasoning is extended to explain characteristic canals of Mars.—New method of calculation for the interpolation and correction of meteorological observations, by M. Marc Dechevrens. The interpolation formula generally used in researches into the laws of variations of meteorological phenomena, and due either to Bessel or Fourier, is long and tedious. From considerable use of this method the author has found that it may be simplified, and in the memoir presented shows how arithmetic—multiplication and addition—may replace trigonometry, angles and logarithms.—Observations of Brooks's comet (a 1890) made with the great equatorial of Bordeaux, by MM. Rayet and Courty. Measures of position are given. A photograph of the comet was obtained on May 15 with an exposure of one hour. It appears on the negative as a disk having a sensible diameter,

surrounded by nebulosity, and connected with a head about 2′ long.—On the asymptotic value of the polynomials of Legendre, by M. Stieltjes.—On the determination of a point, by M. Hatt.—On the isomeric states of chromic bromide, by M. A. Recoura. The author shows that, just as in the case of the chloride, two sesquibromides may be prepared which on treatment with alkaline hydrates both yield the chromic hydrate belonging to the series of violet chromic salts. He gives a method for the preparation of the green salt in crystals of the composition $Cr_2Br_6, 12H_2O$; these crystals are stable when solid, but the salt changes into the violet modification rapidly when in solution. The heat of combination of the green salt (in solution) with NaOH is given as 33·1 cal., whereas that of the violet salt is but 21·6 cal.; during the transformation from the green to the violet modification 11·5 cal. are disengaged.—On the existence of a crystallized hydrated ferric oxychloride, and its transformation into a dimorphous variety of göthite, by M. G. Rousseau. By prolonged heating of a concentrated solution of ferric chloride, in the presence of a little calcium or magnesium carbonate, in a sealed tube, crystals are obtained of the formula $Fe_2Cl_6, 2Fe_2O_3, 3H_2O$. In boiling water a reaction occurs which may be expressed thus—

$$Fe_2Cl_6, 2Fe_2O_3, 3H_2O + nH_2O = 3(Fe_2O_3, H_2O) + 6HCl + (n-3)H_2O;$$

the body Fe_2O_3, H_2O possessing the composition of göthite and only differing from the latter in some of its physical properties.—On some new double chromates, by MM. M. Lachaud and C. Lepierre. Double chromates of lead with potassium, lithium, and sodium are described, similar products being obtained in the case of each of the alkaline metals. Yellow bodies of the composition $PbCrO_4, M_2CrO_4$, and orange compounds of the formula $PbCrO_4, M_2CrO_4, 2PbO$, have been prepared.—On the crystallization of alumina and some other oxides in hydrochloric acid gas, by MM. P. Hautefeuille and A. Perrey.—Note on the *bouquet* of wines and brandies, by M. A. Rommier. It is shown that different ferments produce from the same grapes wine of different flavour, and that solutions of sugar fermented by means of the natural ferments obtained from different districts yield on distillation alcohols possessing different odours; and it is suggested that the characteristic bouquet is due to a compound ether formed from the alcohol combined with a fatty acid produced from the fat which each ferment manufactures from the sugar for its own use.—On the clinical characters of true intermittent fevers; the law and preventive treatment of relapses, by M. Alcide Treille. The author gives his method of treatment by sulphate of quinine, which he has used in fever cases in Algiers for about twenty years.

Berlin.

Physiological Society, May 9.—Prof. du Bois Reymond, President, in the chair.—Dr. Löhers spoke on the effect of inhalations of bromethyl and nitrous oxide on the circulation and respiration, deduced from experiments made with a view to obtaining a physiological basis for the use of these anæsthetics. Bromethyl slows the respiration, leaving the inspirations unaltered, but rendering the expirations weaker and weaker, until they disappear entirely; at an early stage of its action respiration becomes again normal if the animal is supplied with fresh air, but later on this is not the case, and death ensues by the action of the drug on the heart. The effect on the circulation is to quicken it at once; the blood-pressure falls, the pulse becomes arhythmic, and finally ceases; the left side of the heart is now found to be empty, the right gorged with blood; it appears that bromethyl affects the two halves of the heart differently, and thus probably gives rise to the asymmetry of the pulse. When the vagi are cut the effect of the drug on both circulation and respiration is longer in making its appearance. Nitrous oxide has a more powerful action on respiration, the inspirations diminishing rapidly and ceasing suddenly. Normal respiration may be restored by fresh air if the action of the drug has not been too prolonged. The effect on the heart is to increase the blood-pressure. It appears on the whole that bromethyl must be more cautiously employed than nitrous oxide as a narcotic.—Dr. Blaschko made a further communication on the architecture of the skin.—Dr. Löwy gave an account of experiments upon the irritability of the respiratory centre. The experiments were conducted on human beings in such a way as to discriminate between the effects of varying irritability of the centre and varying strength of stimulus applied to it in determining variations in the magnitude of the respiratory movements. The stimulus used was carbonic acid gas mixed in definite proportions with the inspired air. It appeared that dyspnœa did not supervene with less than 6 per cent. of CO_2 in the air; and that in the various states of sleep, whether natural or resulting from narcotics, and after the administering of alcohol and camphor, equal increments of CO_2 lead in all cases to an equal increase of the respiratory movements; hence in all these conditions the irritability of the centre must have been the same. Morphia, on the other hand, lessens the irritability.—Prof. Gad stated that he had some years ago observed a capillary network among the cells of the epithelium which covers the floor of the fourth ventricle, and that Retzius had observed a similar case in the internal ear. It now appears that the occurrence of blood-vessels between the cells of an epithelium is extremely rare, and he therefore urged morphologists to keep a look-out for and to investigate any cases which they may observe.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Treatise on Diseases of the Nose: Dr. G. Macdonald (A. P. Watt).—The Canary Book, Part 1: R. L. Wallace (Gill).—British Cage Birds, Part 1: R. L. Wallace (Gill).—Plant Organization, 2nd edition: Dr. R. H. Ward (Arnold).—Fifth and Sixth Annual Reports of the Bureau of Ethnology: J. W. Powell (Washington).—British Birds, Key to the present Classification: W. H. Wintringham (Grimsby News Company).—Traité Encyclopédique de Photographie, doux. fasc. (Paris, Gauthier-Villars).—Science Applied to Work: J. A. Bower (Cassell).—The Golden Bough: J. G. Frazer (Macmillan).—The Advancement of Science: E. Ray Lankester (Macmillan).—Mungo Park and the Niger: J. Thomson (Philip).—Epitomes of Three Sciences:—Comparative Philology, Psychology, and Old Testament History: H. Oldenberg, J. Jastrow, and C. H. Cornill (Chicago, Open Court Publishing Company).—The Birds of Essex: M. Christy (Simpkin).—Meteorology of Sheffield, 1887–89: E. Howarth.—Earthworks of Ohio: C. Thomas (Washington).—Textile Fabrics of Ancient Peru: W. H. Holmes (Washington).—The Problem of the Ohio Mounds: C. Thomas (Washington).—A Summer School of Science: Prof. P. Geddes (Edinburgh, Thin).—The Pterylography of Birds' Wings: W. P. Pycraft (Leicester).

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THURSDAY, JUNE 5, 1890.

TEA IN JAPAN.

Researches on the Manufacture of Various Kinds of Tea. Bulletin of the Imperial College of Agriculture and Dendrology. By Y. Kozai, Assistant in the Agricultural Chemical Laboratory. (Tokio, 1890.)

Y. KOZAI is a Japanese chemist who performed his researches under the control of Dr. Kellner, the Director of the Chemical Laboratory at Tokio. His paper includes the chemical constitution of tea, the effect of tea on mankind, the principal methods of manufacture employed in Japan, and the methods of preparing tea for consumption. These subjects are all treated mainly from the point of view of the analytic chemist. The author appears fairly well acquainted with what the German chemists have done in the matter of tea.

We need not abstract much of his account of the constitution and properties of tea, as it is largely taken from European sources. "The chief action of tea, after it has got into the blood, is to excite the nervous system; it thus harmonizes the mind, drives out drowsiness, and awakens thought, stops hunger, and cures repletion, refreshes the body, and prevents head-ache"—and (it might be added) if taken too strong keeps you awake half the night. As to its constitution, tea contains (besides the common plant-constituents) theine, a volatile oil, and tannin. Theine is a rank poison, in toxic doses causing convulsions and paralysis, in lethal doses death; but in small quantities is (like strychnine) a delicate tonic. Of the volatile oil, Y. Kozai can affirm little beyond its well-known exciting action upon the organs of taste and smell; nor is it easy to follow it analytically through the processes of manufacture; the hot steaming employed (at near boiling temperature) in the green-tea manufacture does not appear to diminish the volatile oil sensibly, though Y. Kozai intimates that preparing green tea by boiling does dissipate the aroma. As to the properties of tannin, it is an astringent remarkable for its strong affinity for the albuminoids; hence, if taken in excess, it may, by precipitating the ferments of the digestive fluids, cause indigestion.

The account of the chief Japanese methods of manufacture is of more interest and instruction to the European planter.

We may premise that there are two (main) kinds of tea, viz. black and green. In the manufacture of black tea there are four essential processes, viz. (1) withering; (2) rolling, (3) fermenting, (4) drying. In the manufacture of green tea, the fermenting is omitted, and in Japan (for some kinds of green) the rolling also.

For the manufacture of black tea there is no real difference between the Japanese method and that practised by English planters in Bengal. The fresh picked leaf (*i.e.* tips of the young shoots) must be first withered, or the petioles and leaves break under the rolling; the exposure of an hour or two in strong sun withers the leaf sufficiently; if there is no sun, the leaf must be withered by the aid of fire-heat. The rolling is done, even in Japan, by the aid usually of a box, and in Bengal often by steam-power (and very roughly). The juices are thus expressed, and the leaf given a "nice" twist, *i.e.* a twist pleasing to the fancy of the tea purchaser. What perhaps renders rolling so essential in the manufacture of black tea (for it is not esssential in the manufacture of green), is that it masses the leaf in a state conducing *without delay* to fermentation. Neither Y. Kozai nor the best Bengal authorities like to lose the juices more than can be helped. He also hazards the view that, by rolling, the juice is expressed from the cellular tissues of the leaves and impregnated upon their surface; thus is produced fine aroma, and the leaves are more easily infused. Fermentation is the most important point in the manufacture of black tea, and by it (*fide* Y. Kozai) the leaves lose their raw smell, and the tea acquires its fine flavour. The fermentation is really only carried a very little way: Y. Kozai says it should be allowed, in a temperature of 104° F., to proceed only for about an hour. He thinks the process is a true fermentation, because if permitted to run too far the tea acquires an acid taste. He thinks it probable that the ferment is caused by a living organism, but he adduces very slight ground for this opinion; and it has, in fact, been questioned whether there is any true fermentation in the process at all. But the English tea-makers are agreed with the Japanese in the importance of stopping the fermentation exactly at the proper point by drying the tea, which is usually done by placing it first in the sun and turning it over till it is fairly dry, and then thoroughly drying it by fire-heat.

The result of all the Bengal experience is that the black tea is at least as good when these four processes are done simply and rapidly, as when much labour and time are expended in complicating them. In the early days of tea manufacture by Anglo-Indians, great pains were taken to imitate with tedious minuteness the careful hand-processes (and repetitions of portions of the processes) as practised in China; but all planters now follow rapid short cuts to the finished tea.

The manufacture of green tea is nothing more than drying the leaf; it is so little practised in British India as to be of no commercial interest there, but Y. Kozai describes in detail three kinds of green tea manufactured in Japan.

(1) *Japanese* (not China) *green tea.* In this, the leaf is steamed in order to remove the raw flavour; it is then rolled and fire-dried, the two last processes being usually done together.

(2) *Chinese green tea.* In this, the leaf is roasted (while stirred with a stick) in an iron pan over a fire, then rolled a little, then roasted again; these processes being repeated even six or eight times, and the tea is then finally dried off.

(3) *Flat tea*, the highest class tea of all. For this tea, the shrubs are usually kept shaded for three weeks before picking, so that the leaf is partly etiolated. The choicest leaves are selected before the manufacture is commenced. They are steamed, but never rolled; nor, indeed, touched by hand at all, but carefully turned by the aid of a bamboo stick. After sufficient steaming they are simply dried.

The author finds by analysis that there is 30 per cent. more theine in etiolated leaves than in the leaves of the same plants grown in the light. He tried many experiments to test the chemical effect of the manufacturing

processes. Among other tables given by him is the following; a quantity of leaf was divided into three portions, whereof one portion is A, another portion is manufactured into green tea B, the third portion is manufactured into black tea C. Y. Kozai analyzses A, B, C, and finds—

	A.	B.	C.
Crude protein	37·33	37·43	38·90
Crude fibre	10·44	10·06	10·07
Ethereal extract	6·49	5·52	5·82
Other nitrogen-free extract	27·86	31·43	35·39
Ash	4·97	4·92	4·93
Theine	3·30	3·20	3·30
Tannin	12·91	10·64	4·89

He remarks that the general result of the green-tea manufacture is merely to dry the leaf; the black-tea manufacture alters materially its chemical constitution. The principal change is the remarkable diminution of the tannin. He does not explain how this is brought about, nor is it easy to see how the incipient fermentation should affect the tannin.

The only teas exported to Europe from Japan are of low class; they are frequently "faced," and sometimes mixed with the leaves of various Japanese plants. Any plentiful leaf, not too unlike the leaf of tea, will do for this adulteration; the leaves actually employed are (Y. Kozai assures us) all harmless, and several contain tannin, but none of them any theine. As to the "facing," he says it can hardly be called adulteration; the quantity of Prussian blue employed to improve the appearance of green tea is (according to Y. Kozai) about 0·001 per cent. the weight of the tea, perfectly innocent, and pleasing to a purchaser.

The author concludes with an account of the different ways of taking tea in Japan, with some analyses of the prepared liquor.

(1) In the case of flat tea, or of the very finest quality of Japanese green tea, the tea is ground to fine powder, and the whole infusion drunk.

(2) In the case of superior (*i.e.* from the Japan point of view) tea, the leaves are infused for two minutes in water at 120°–150° F.

(3) In the case of a medium tea, the leaves are infused for one minute in boiling water.

(4) In the case of inferior tea, the leaves are boiled in water.

The object to be aimed at in the preparation is to get the largest possible quantity of theine without dissipating the aroma, and accompanied by only a moderate amount of tannin. Y. Kozai gives analyses to show that this is effected (in the case of superior teas) by the infusion in water at 120°–150° F. for two to five minutes. By superior teas, he understands teas worth five to seven shillings a pound in Japan. It is probable, therefore, that the highest class teas we ever have to deal with in England come under the medium teas of Y. Kozai, which require infusion in boiling water—for one minute at least. The majority of English people like a good deal of chicory with their coffee, and probably a majority also like a good deal of tannin with their tea; and to them the analyses and recommendations of the Japanese writer are of small importance.

The paper will be of more use as food for reflection to the Anglo-Indian planter than as direct instruction. The palate of the Englishman is as yet only very roughly educated in tea. There can be very few Englishmen who would greatly prefer the superior teas of Japan and China to the ordinary Kumaon or Ceylon tea; most persons used to drinking the latter would probably prefer it to the most expensive tea made—say China tea worth forty shillings per pound in China. The English planter in Bengal has a tea-garden of 200 acres (possibly still larger). His object is, by the aid of a steam-engine or other coarse help, to put his tea through—to keep his factory clear when he has a strong flush on. He has to carry the daily make through by the aid of uncivilized labourers and overseers. He must reduce every step of his manufacture to a routine; he must have no special tea separately and differently manufactured, and no current experiments. Few planters have made much profit by Pekoe; and the green tea hardly exists commercially in India. There are no doubt many Englishmen who, having not a plantation but (literally) a garden with some tea in it in India, have manufactured, not unsuccessfully so far as the flavour of the tea is concerned, green tea, Pekoe, &c., but this has been a fancy article for their own drinking or for presents, and has never been put in any quantity on the market. To plant successfully in India, the Englishman has to proceed on a broad scale; his large cost and high expected profit cannot be got out of the close superintendence of elaborate hand manufacture. Or, at least, it will be a long time before the public tea taste at home is sufficiently elevated to be willing to pay so large a price for such teas as would remunerate the English planter. For the present, the object of the planter must be to produce the maximum quantity of tea that the English grocer can sell at 1*s.* 6*d.* to 2*s.* 6*d.* a pound. Hence to planters the utility of the paper of Y. Kozai must be mainly future.

CATALOGUE OF BRITISH FOSSIL VERTEBRATES.

A Catalogue of British Fossil Vertebrata. By Arthur Smith Woodward and Charles Davies Sherborn. Pp. i.–xxxv., 1–396. (London: Dulau and Co., 1890.)

A WANT long felt by all students of the fossil Vertebrates of the British Islands has been supplied by the issue of the present volume, which, so far as we have been able to examine it, is noteworthy alike for the absence of misprints, the accuracy of the references, and the care which has been taken in the selection of the correct names for the various genera and species, as well as for the orthography of the names themselves. The last edition of the late Prof. John Morris's "Catalogue of British Fossils" was published as far back as 1854, and the advances made by this branch of palæontology since that date—and more especially during the last ten years—have naturally rendered that work quite out of date. It is true, indeed, that the first part of Mr. R. Etheridge's "Catalogue of the Fossils of the British Islands," and the British Museum Catalogues of Fossil Vertebrates, have afforded some assistance to students of this subject;

but since the former deals with the Vertebrates of one particular epoch, while some of the latter include only such of the British fossil Vertebrates as are represented in the National Collection, they in no way cover the ground occupied by the present work.

It is, of course, needless to say that the work before us is essentially a technical one, and therefore appeals only to students of this particular branch of science, or to those stratigraphical geologists to whom it is important to know the correct horizon, localities, and nomenclature of the fossil Vertebrates of the British strata. So far as completeness and accuracy are concerned, the work is beyond criticism; but we trust we shall not be accused of any carping spirit if we venture in the course of our notice to indicate a few points in which, according to our judgment, it might be improved.

The greater part of the introduction is occupied by an entirely new and very valuable history of the chief collectors and collections of the fossil Vertebrate remains found in the British Isles. Then we have a careful explanation of the general plan of the work; followed by some judicious remarks as to the harm that has been done to the science by the publication of a host of undefined names. When, however, the authors hold "a single University Museum" "responsible for no less than seventy meaningless terms," we venture to think that the individual or individuals by whom such names were proposed should rather have been held responsible for the same. Following the introduction, a table (for which the authors are indebted to Mr. W. H. Brown) of the dates of publication of the fasciculi of Agassiz's "Recherches sur les Poissons fossiles" will be found of especial value, as fixing the date of many genera of fossil fishes. Scarcely less valuable is the determination of the respective dates of appearance of the three parts in which Sir R. Owen's well-known "Odontography" was originally issued.

In the table of the stratigraphical distribution of British fossil Vertebrate genera, which concludes the prefatory portion of the volume, we must take exception to the very insignificant deposit known as the "Forest-bed" being allowed to take rank as the *Forest Bed Series*, as though it were of equal importance with the Pliocene and Pleistocene; under one of which it should have been included as the *Forest-bed Stage*.

In regard to the plan of the work itself, the various genera and species are arranged alphabetically under the classes to which they respectively belong—a mode of arrangement in which the authors follow the Morrisian Catalogue. They depart, however, from the latter in not mentioning the order to which each genus is commonly referred. Here, we think, the innovation is not an improvement, since in the case of stratigraphical geologists, who may have occasion to consult the work, it would often be an advantage to know at once to what large group any particular genus belongs; and even a student of one particular class of Vertebrates may well be at a loss to know the ordinal position of a genus belonging to another class with which he is less intimately acquainted.

It also strikes us that it would have been advisable to state the authority for regarding various genera and species as synonyms of others; for, as it stands at present, there is no evidence to show whether such references are made for the first time on the authority of the authors themselves, or whether others are responsible. Thus, under the head of *Hyracotherium leporinum* (p. 356) we find *Pliolophus vulpiceps* given as a synonym, without any guide to the authority for such reference. In this particular instance we believe the identification of *Pliolophus* with *Hyracotherium* was first made by Prof. W. H. Flower in his article "Mammalia," published in 1882 in the latest edition of the "Encyclopædia Britannica," and some reference to this should have been made.

On the whole, the authors appear to have exercised a wise discretion in not amending for the first time the spelling of such generic and specific names as are obviously incorrect according to a true Latinized orthography. We cannot, however, follow them in their refusal to accept emendations which have already been published in other works, more especially as they are not consistent in either adopting or rejecting such emendations. Thus, for instance, they adopt the name *Machærodus* (p. 366) as amended from the original *Machairodus*; but they refuse to accept *Ælurus* in place of *Ailurus* (p. 311), although the amended name has been published more than once.[1] Again, they retain *Leiodon* (p. 245) and *Platecarpus* (p. 264), although the amended *Liodon* and *Platycarpus* have been published—the latter, we admit, but recently. The authors seem, indeed, to have a rooted objection to the transliteration of the Greek ει into the Latin *i* (as may be noticed in the root *Cheir* instead of *Chir* under the head of Pisces); but this transliteration, as every student of our Greek Testament knows, is just as binding as that of αι into *æ*, or ου into *u*, and if the one change is rejected the others ought not to be adopted.

As a rule, the authors have paid attention to the gender of generic names, which is too often neglected. They regard compound generic names as substantives, and, therefore, bring the gender of the specific name into accord with that of the terminal portion of the generic one. They state, however, on p. 395, that they have not followed this rule in regard to names ending in *lepis*, where they have allowed the specific names to remain with the masculine termination. They appear to have adopted the same course with regard to the termination *batis* (*Aëtobatis*, p. 9); but in the case of *aspis* the authors seem to have been unable to make up their minds, since on p. 79 we find *Eukeraspis pustuliferus*, while on p. 129 we have *Odontaspis cuspidata*.

As features of especial value in the work before us, we may notice that in every instance where it can be ascertained the place of preservation of the type specimens is indicated; while all the recorded localities are given under the head of the various species.

The compilation of a work like the present is a labour which only those who have had the misfortune to try it can fully comprehend, and the thanks of every student are therefore due to Messrs. Woodward and Sherborn for the production of a book which is absolutely indispensable to all those who are engaged in the pursuit of this branch of palæontology.

R. L.

[1] See Flower, Proc. Zool. Soc., 1870, p. 752; and Blanford, "Fauna of British India—Mammalia," p. 189 (1888).

AN EPHEMERIS.

Connaissance des Temps. Extrait à l'usage des Écoles d'Hydrographie et des Marins du Commerce. Pour l'an 1891. (Paris: Gauthier-Villars et Fils, 1889.)

THE *Connaissance des Temps* has, within the last few years, by successive improvements, been made quite the most convenient Ephemeris for general use. The information it contains is conveniently given, and almost excessive in amount; and the result of course is that the pages of tabular matter are a good deal crowded, in order to make the annual volume of reasonable size. For travellers whether by sea or land it, like our own *Nautical Almanac*, is not, however, quite what is wanted. Much of the information it contains is of no use to them, and the size and weight of the book is excessive for their purposes. This appears to have come to the notice of the Ministry of Marine, who, in 1887, directed the publication of a pamphlet of extracts from the *Connaissance* containing the necessary information for Navigators and students for certificates as Masters, a copy of which is now before us.

In making this effort to meet the wants of a very large class of practical men the French have but followed the example of other countries. Some forty years ago the Prussian Government caused to be compiled a *Nautisches Jahrbuch*, which in its present form appears to be the best adapted of those we have seen for geographers and voyagers. It is manifestly copied, as to form, from the *Nautical Almanac*, avoiding all the matter useless to geographers, which is relegated to the well-known *Berliner Jahrbuch;* the contents are all given with an accuracy sufficient for the purposes for which it is intended: a thoroughly practical mind seems to have guided the whole arrangement, and the changes which seem desirable are but small. The American Government next published an *American Nautical Almanac*, which is practically a reprint of those parts of their larger Ephemeris which are supposed to be required at sea. It is needlessly accurate in its data, and needlessly bulky, but no doubt fulfils its object. And again, just before the French, the Austrian Government published at Trieste a Nautical Ephemeris founded on our *Nautical Almanac*, but almost identical in contents with that of the German Government before spoken of. This, it would seem, is published with the text and headings in more than one language.

The French work approaches most nearly in type to the American: it is mainly a reprint. That part which is not so is the Ephemeris of the Moon, and here convenience is sacrificed to a small gain of space. Not only are the pages crowded unduly, but the arguments (being at 12-hour intervals) are so far apart that interpolation becomes inconvenient.

Before closing we would like to point out that while all these Governments have provided an Ephemeris for their Nautical men and Travellers which is meant to be specially suited to their limited wants; England alone, which owns probably half the sea-going ships in the world, and furnishes no small proportion of the explorers, makes no special provision for them. It is not that there is no want felt: for there are several almanacs which, availing themselves of the *Nautical Almanac*, give astronomical data, together with various other matters useful to seamen. Our *Nautical Almanac* took its present form on the report of a committee of the Royal Astronomical Society, to whom reference was made by the Admiralty in 1830. No great change has been made since then, and it is beginning to be thought time that its contents should be revised: if this is done, we trust it may be considered whether the wants of navigation and geography should not be specially taken into account. If we are right in believing that the Austrian Government have founded on our *Nautical Almanac* a publication which admits of all the tabular matter, which is so difficult and expensive to put in type, being combined with a text in varying languages, it might be possible by the adoption of a suitable form, to supply the wants of other nations as well as our own. Our *Nautical Almanac* in its present form is used, we believe, extensively by those maritime peoples who adopt Greenwich as a prime meridian, and it would, we think, not be difficult to arrange with their Governments for impressions suited to each language.

OUR BOOK SHELF.

The Wimshurst Electrical Influence Machine. By W. P. Mendham. (Bristol: King, Mendham, and Co., 1890.)

THIS little book, which partakes somewhat of the nature of a trade catalogue, briefly describes and illustrates the construction and action of the Wimshurst machines made by the firm of King, Mendham, and Co., and of the accessory pieces of apparatus needed for use with these machines in performing the antiquated experiments so much in vogue with the dabbler in frictional electrical science. The study of high tension electricity is coming to the front so much just now, that it is a great pity Mr. Mendham has not utilized his opportunity better, and given to the class of readers for whom this book is intended some notion of the many instructive and easily performed experiments on the disruptive discharge, and on electrical oscillations, which we owe to Hertz, Lodge, and others. The only concessions made to modern discoveries are in the descriptions of apparatus to show the action of the electric discharge on smoke, and of the Thomson quadrant electrometer. The latter, however, had better have been left alone, for the description is too meagre to enable the action of the instrument to be appreciated, and the reader may be apt to imagine that the quadrants are intended to be connected up directly to the terminals of a Wimshurst machine. We need scarcely say this would be very hard on the instrument. H. H. H.

Pawnee Hero-Stories and Folk-Tales. By George Bird Grinnell. (New York: Forest and Stream Publishing Company, 1889.)

THE Pawnees were at one time what Mr. Grinnell calls "a great people." They roamed over a vast territory, and enjoyed considerable material prosperity. Now, their numbers are greatly reduced, and the few who remain give a very inadequate idea of the vigour of the original stock. The author of the present book knew the tribe intimately twenty years ago. He used to camp and hunt with them in Nebraska, and at night they told him hero-stories and folk-tales which had been handed on to them from their forefathers. Many of these narratives he carefully translated and wrote down at the time; and quite lately he visited his old friends for the express purpose of inducing them to extend his collection. They were eager to meet his wishes, and so he was able to bring together the stories which he has now published. He claims that they are recorded exactly as he himself

heard them, and that they may therefore be regarded as faithfully reflecting the Pawnee character. As genuine documents, throwing light on the ideas and habits of a primitive people, the stories are of some scientific value; and students of anthropology will find in them a good deal that is interesting and suggestive. Mr. Grinnell adds various notes, in which he gives much well-arranged information as to the history, racial affinities, and institutions of the Pawnees.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Influences at Work in producing the Cerebral Convolutions.

DR. G. JELGERSMA, of Meerenberg, has recently published two remarkable papers,[1] in which he endeavours to explain the influence which leads to the production of the convolutions on the surface of the cerebrum and cerebellum. Many theories have been advanced to account for these. Several authorities have ascribed their presence to mechanical forces operating upon the brain from without, whilst others have sought to explain them by the supposition of different degrees of growth-tension acting upon the brain-surface; but in every case these theories, when submitted to the test, have broken down, in so far that it is impossible, by means of any of them, to show how it comes about that small animals have smooth brains, and large animals convoluted brains; how, in short, we should find in the beaver—an animal remarkable for its intelligence—a cerebrum almost entirely smooth, and in the sheep—an animal, shall we say remarkable for its dullness?—a brain with a high convolutionary system. Jelgersma not only explains this, but makes the apparent discrepancy the strongest pedestal of support to his theory. Briefly put, his views are as follows:—

The grey cortex of the cerebrum, which in different forms of the same animal group preserves a tolerably constant thickness, increases by surface extension. Now, if we extend the surface of a smooth-brained animal say four times, we must provide eight times as much white matter to fill the interior of the grey capsule, if we desire to keep the surface even; or, to put it in different terms, if we lengthen out the radius of the brain say ten times, we acquire a surface extension one hundred times greater, and an internal capacity one thousand times greater. The geometrical law involved is simply this, that in the growth of a body the surface increases with *the second*, but the interior with *the third* power of the radius.[2]

Such being the case, it is very evident, seeing that the proportion of internal white matter and external grey matter is in all cases a uniform one, that in the evolution of a large animal out of a small animal, a disproportion between the grey capsule and the white core of the cerebrum must result. This is compensated for by the extended cortex placing itself in folds or puckers, and thereby reducing the capacity of the capsule to a degree which brings it into correspondence with the white contents. Consequently, "the formation of the convolutions and furrows is simply the result of the tendency on the part of the superficial layer to increase by surface extension and of a mutual space-accommodation (*Raumaccommodation*) of the grey substance and of the white conducting paths."

I have not written this short account of Jelgersma's views—important though they be—simply for the purpose of giving them a wider circulation through the pages of NATURE, but with the object of stating that the theory advanced has received independent testimony in its favour at the hands of my colleague, Prof. George F. Fitzgerald. For two years or more I have been engaged in a research bearing upon the growth of the cerebral hemispheres, and have constantly had occasion to appreciate the unsatisfactory nature of the current theories as to the formation of the convolutions of the brain. Consequently, in February last, before I had read Jelgersma's first article, and before the appearance of the second, I explained to Prof. Fitzgerald, as far as I could, the conditions of cerebral development, and asked him if he could offer any geometrical explanation which would account for the appearance of the convolutions. The views which he then advanced were identical with those of Jelgersma, and further, they were expressed in very similar terms. I feel that this adds greatly to the weight of the hypothesis.

But Prof. Fitzgerald went further than Jelgersma, because the latter states that he is unable to explain why the fissures and convolutions should, within certain limits, assume the same formation in different animals. Fitzgerald, however, saw the importance of his theory in regard to the localization of function in different areas of the cerebral cortex. The surface extension of the cerebrum cannot be a uniform one: the bulgings out in the shape of the convolutions must necessarily be connected with the functions which the areas involved have to perform. Therefore if a given area of grey matter increases it must pucker out, unless an undue quantity of white matter grows all over the inside of the grey cortex.

D. J. CUNNINGHAM.

Anatomy School, Trinity College, Dublin, May 24.

[1] "Über den Bau des Säugethiergehirns," *Morphologisches Jahrbuch*, June 1889; "Das Gehirn ohne Balken; ein Beitrag zur Windungstheorie," *Neurologisches Centralblatt*, March 1890.

[2] It is right to state, although, indeed, Jelgersma does not mention it, that many years ago Baillarger ascribed the increase of the convolutions with the increase in the size of the animal to the same geometrical law.

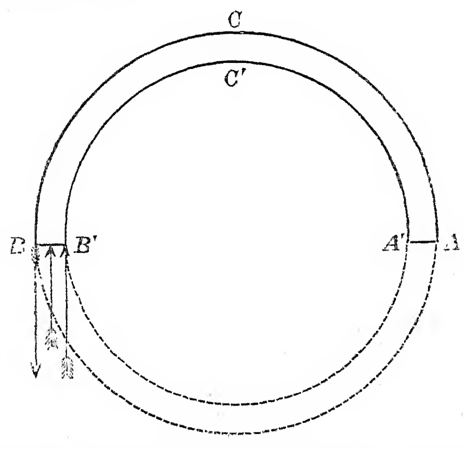
The Bourdon Gauge.

FROM Prof. Greenhill's letter on this subject in NATURE, vol. xli. p. 517, as well as from that of a writer in *Engineering*, I gather that I did not succeed by my letter (NATURE, vol. xli. p. 296) in making quite clear the point of my explanation of the action, since Prof. Greenhill argues that consideration of the longitudinal stresses in the walls leads to the conclusion that the tube would curl up under internal pressure rather than uncurl.

Towards the top of the second column on p. 296 in my letter I used the words "Consider now the equilibrium of any portion . . . when the internal pressure is applied and before uncurling takes place." Perhaps it would have been clearer to have written "*after the internal pressure has been applied*," &c. In the last figure on the same page the tension T is that exerted by the outer wall *of the already distended gauge* as it contracts, while P is the thrust of the inner wall, each on the part BC supposed solidified.

I desire specially to emphasize the words italicized, for my method of explanation amounts to an artifice for taking the distension into account. It is because Prof. Greenhill has overlooked this that he arrives at an opposite conclusion, and wishes apparently to reverse the forces in the figure referred to.

I hope to make this clear by putting the argument again in a slightly different form.



Starting, as before, with a tube of rectangular section, with the end AA' fixed and BB' free, we arrive at the uncurled condition by taking the tube in imagination through the following series of steps:—

(1) Remove the ends AA' and BB', and complete the annulus as indicated by the dotted lines of the figure.

(2) Now apply internal pressure. This distends the tube, stretching the roof and floor. The inner wall is compressed with a longitudinal thrust, and the outer wall stretched with a longitudinal tension, but the change in the diameter AB, or in the diameter A′B′, will be practically unobservable.

The action on the original gauge and its enclosed fluid of the added part and its fluid, consists now of the forces indicated in the figure, and which amount, as I have in my previous letter pointed out, to a couple (counter-clock-wise in the present figure).

(3) Now replace the ends at AA′ and BB′ (this makes no difference in the equilibrium), and holding AA′ fixed, remove the added part.

The gauge will now uncurl, for we are removing the counter-clock-wise couple necessary to maintain equilibrium. Or, to put it in other words, the outer wall ACB, on being released from the tension at B shortens, while the inner wall being released from the pressure at B′ becames longer, thus causing the gauge to uncurl.

As to Gauss's purely geometrical theorem, I fail to see how it is to be of any use in the analysis of the forces, which I take to be the real problem. All that Gauss says to us by his theorem is, "Pure bending in your gauge means uncurling; if, therefore, you can prove that the forces are such as to produce pure bending, you prove that they produce uncurling." But this is exactly what we cannot prove. Indeed, it is admitted that the bending is not pure. And it is, I think, of no use to urge, with Lord Rayleigh, that the bending is *nearly pure* on account of the comparative inextensibility of the material, for that argument would apply equally to the gauge with both ends fixed, or to a complete annulus which obviously cannot uncurl. In fact, if we could go back to Gauss and ask, "Is it any use showing that the bending is 'nearly pure'?" he would ask us what we meant by "nearly," and before we could answer that we should have to analyze the whole action. It is for these reasons that I consider the reference to Gauss's theorem not only unfruitful but misleading.

If we apply the method I have suggested to a tube of elliptical or other than rectangular section, we see that unless longitudinal stresses such as I have dwelt upon would exist in the walls were the annulus completed, the distended gauge will not uncurl on the removal of the added part, and the only reason for considering the curvature of either wall in a plane perpendicular to the circular axis, is that such curvature may, on account of the properties of the material by which it is able to distribute stress in different directions, lead to additions to or subtractions from (and conceivably therefore reversals of) the longitudinal thrust or tension that would exist in a tube of rectangular section. But this is obviously a question of the structure of the material and not of pure geometry. A. M. Worthington.

R.N.E. College, Devonport, May 14.

A Subject-Index and the Royal Society.

The following brief account of an offer I have made to the Royal Society will, I think, prove of interest to the readers of Nature, and especially to those correspondents who have emphasized the importance of a subject-index for the progress of science in all its branches.

Upon the conclusion, last autumn, of five years' work, during which my "Epitome of the Synthetic Philosophy" was compiled, I commenced to look for some literary work which would be of undoubted *practical* service to science, and which would if possible aid its further development. The articles and numerous letters then appearing in your columns urging the importance of a subject classification of the memoirs arranged under the authors' names in the "Royal Society Catalogue of Scientific Papers," led me to discuss the advisability of my undertaking such a proceeding with several friends upon whose judgment I could rely; with the result that, one and all agreeing upon the value of such a work, I wrote to Sir G. G. Stokes, P.R.S. (to whom I was directed), offering to compile the manuscript of the greater portion of such a work upon the condition that, as soon as the manuscript was completed and approved by the Council of the Royal Society, the Society should guarantee all expenses of print and publication. I was forced to say "the greater portion" of such a work ("70 to 80 per cent."), for examination of a large number of titles had shown me that a certain percentage of them could only be correctly indexed by specialists in their own departments, a fact which is emphasized when we call to mind that a title may be in any one of eight European languages.

After several interviews with Sir G. G. Stokes, and a somewhat protracted correspondence, I agreed to arrange a sample index of some 2000 entries upon a plan suggested by him, and warmly approved by that eminent bibliographer Dr. Garnett, of the British Museum, the plan being to take the leading word or words in the title of each paper, with a reference to the volume, page, author's name, and number of the paper, in the Royal Society's Catalogue, for subsequent arrangement in alphabetical order, by which means the subject-key would occupy but a quarter of the bulk of the Catalogue as now published. It would extend—that is, approximately—to three quartos of the size of the present volumes, in similar type, &c.

In the early part of May this plan was discussed by the Catalogue Committee of the Royal Society, when the following resolution was passed:—"That the offer of Mr. Collins be declined, and that the President be requested to convey to Mr. Collins the best thanks of the Committee for the trouble which he has taken."

The foregoing account will be sufficient to show that, contrary to an opinion expressed more than once in these pages, something more is needed than an offer to compile the subject-index. Were the manuscript now completed, and approved by the Royal Society, there would still be wanting a sum sufficient to bring it before the public.

In conclusion, I should like to express my warm thanks to Sir George Stokes for the kind and courteous way in which he has assisted me in my endeavour to develop what I am still convinced would be of immense service to science in all parts of the civilized world; and not only to science, but to many industries besides. For would not the chemical manufacturer and the dyer profit by a complete list of all the papers that had been written on the coal-tar colours; the agriculturist, by knowing the researches which had been undertaken to ascertain the nutritive powers of the bones and phosphates, and the fattening properties of the various cakes and foods; the engineer, the analyses of iron and steel with their accompanying properties; the physician, the physiological action of the various drugs; and the electrician, all the papers, for instance, which had been written upon that little understood subject, induced currents? Finally, how many millions might have been saved in the construction of harbours all over the British Empire had all the scattered information upon the flow of water in rivers and tidal estuaries been so gathered together as to make reference possible, not to say easy? F. Howard Collins.

Churchfield, Edgbaston.

Stream Lightning.

If a candle-flame is put between the poles of an electrical machine, while it is giving rough angular sparks, the discharge at once changes into a smooth single line of very easy curvature: it suggests the difference between sinuous and stream lightning: it is not merely that the spark is as if shortened by the conducting flame; the whole nature of the discharge is changed. If the flame is held two inches beneath the poles, the spark will go down to it. W. B. Croft.

Winchester College, May 30.

Atmospheric Circulation.

On March 9 and 10, 1887, the barometer rose to 30·92 inches over Iceland—a very exceptional height for that locality at that time of the year. The United States daily maps of the northern hemisphere show that a storm to the southward of this great anticyclone was carried westward a distance of over six hundred miles within twenty-four hours, in a manner similar to that in which West Indian hurricanes follow the course of the trade winds in August and September, although this storm was located in latitude 40° N., or in the usual situation of the anti-trades. Other instances of a similar character have been noted, but this one was unusually well defined, and throws much light upon the laws governing the atmospheric circulation.

Lyons, N.Y., May 20. M. A. Veeder.

Testing for Colour-Blindness.

In answer to Prof. Lodge's query (May 29, p. 100), why those concerned in testing for colour-vision do not avail themselves of instruments like Lord Rayleigh's, having tested some thousands in this city, I may state that experience has shown that they are

not suited for testing uneducated persons. A similar instrument, introduced by Chibret and Meyer, of Paris, is to be found in ophthalmic hospitals.

I may further remark that I do not consider any test satisfactory unless made by an ophthalmic surgeon, as he alone is accustomed to deal with such people every day of the week, and can alone eliminate such errors as refraction-disease and stupidity. D. D. REDMOND.

14 Harcourt Street, Dublin, May 3.

The Green Flash at Sunset.

YOUR correspondents (vol. xli. pp. 495, 538) seem to imply that this phenomenon is only seen at sea, but I observed it on May 17 while walking from east to west, near Worms Heath (Warlingham, Surrey). It had been an exceptionally fine day, since the morning, and about 8 p.m. there was not a cloud in the sky, except to westward, where strips of cloud were rapidly forming, and covering up the glow of sunset; the sun had sunk behind a hill, when, suddenly, my companion and I both saw a flash of green light against the thickest cloud; it lasted 1 or 2 seconds, just long enough for there to be no doubt about it. We compared it to the glare thrown by "green fire," extending over an area whose diameter appeared about four times that of the moon.

At 12 p.m. the same night it was raining.

I think this observation definitely negatives the sea-wave theory, while the appearance was seen at least in association with the condensation of aqueous vapour. Perhaps the reason it was not *bluish*-green was that this vapour absorbed the blue rays? T. ARCHIBALD DUKES.

16 Wellesley Road, Croydon, June 2.

THE THEORY OF SCREWS.[1]

THE book before us, a large octavo volume of over 600 pages, gives in a connected form the results of Sir R. S. Ball's investigation in the theory of screws, as contained in his "Theory of Screws" and a series of publications in the Proceedings and Transactions of the Royal Irish Academy. But as its scheme is that of a text-book on theoretical mechanics, it begins with a chapter on the postulates and methods of mechanics; whilst chapter vii. is on the theory of moments of inertia; chapter viii. on impulsive forces capable of imparting to a rigid body a given state of velocity; and chapter x., on kinetic energy, contains a number of propositions from analytical dynamics. Here expressions for the kinetic energy, for its change in consequence of an impulse, Lagrange's equations of motion in generalized co-ordinates, Hamilton's principle of least action, and various other propositions, are developed in the usual form—that is to say, without the use of screws. The rest of the book relates to the theory of screws and its applications. This alone, as forming the characteristic feature of the book, concerns us here, and of it we shall try to give an outline.

In order not to be unintelligible to those who have no knowledge of Ball's creation, it will be necessary to begin with the very elements of the subject; and in order to form a just idea of the scope and importance of the new method, it will not be sufficient to give a sketch of the results obtained—it will be necessary to take a wider view of the subject. We shall then be able to form some idea of the inherent capabilities of the theory. These I believe to be very great—very great indeed. One of its peculiarities lies in this, that all the results obtained in modern algebra and geometry, as distinct from analysis, seem to be directly applicable to it.

Friends of synthetic geometry and of graphical methods, too, will find here a wide field for investigations. Grassmann's "Ausdehnungslehre" has already been pressed into its service, and the theory of vectors and quaternions is easily applicable. Clifford, in fact, has generalized the latter theory into that of biquaternions to embrace screws.

Mr. Cartesius, to make use of Sir Robert's personifications, has been dethroned, and Mr. Anharmonic together with Mr. One-to-one reign in his place.

Poinsot, whose investigations form the starting-point of the theory of screws, has proved that a rigid body can always be transferred from one position to any other by a rotation about a certain perfectly determined axis, together with a translation along this axis. These two motions combine to a motion identical with that of a nut on a screw. It is completely determined if the angle through which the rotation takes place, together with the ratio of the translation to the rotation, is given. This ratio—*the "pitch" of the screw*—characterizes the screw. As the motion does not at all depend upon the diameter of the screw, we may suppose this to become infinitely small, and then we have the notion of Sir R. Ball's *screw*.

A screw, therefore, is a line in space which has connected with it a certain pitch—*i.e.* a certain length, as the pitch is a linear magnitude. The compound motion considered is called a "twist" about a screw, and is known if the screw and the "amplitude" of the twist, *i.e.* the amount of rotation, is given. In the same way a system of forces can, according to Poinsot, always be reduced, and that in one way only, to a single resultant and a couple turning about the resultant; and these two dissimilar parts Ball combines to a "wrench on a screw," the line of action of the resultant force being the axis of the screw and the ratio of the moment of the couple to the force giving the "pitch," whilst the magnitude of the resultant force is called the "intensity" of the wrench.

We have thus a new entity—the screw—and its introduction forms the characteristic distinction of the theory. Connected with it is a kinematical and a kinetic entity—the twist about a screw, and the wrench on a screw.

If we now consider a rigid body under the action of any forces, then the latter combine at every moment to a wrench on some screw, whilst the motion itself is always a twist about some other screw. If the body is constrained in any manner, then the reactions due to the constraint will also at every moment combine to a wrench about some screw.

The problem first to be solved is that of the combination of twists and wrenches. Let any two screws, α and β, be given, then wrenches on them constitute together a system of forces, and therefore combine to a wrench on some other screw, γ, which has to be determined. If the ratio of the intensities of the two given screws be varied whilst the screws themselves remain unaltered, then the screw, γ, of the resultant wrench also varies, and its axis describes a surface called the cylindroid. This is a ruled cubic surface which can be described as follows:—Let through a fixed line, l, a plane be drawn, and in it a circle be taken. Let a point, P, move uniformly in the circumference, whilst the plane itself turns uniformly about l, completing half a revolution whilst P describes the whole circumference. The perpendicular from P to l will then generate the cylindroid, and the screw on any generator will have a pitch equal to the length of the perpendicular from P to l. The line l is a nodal line of the surface and perpendicular to all screws on it. All cylindroids are similar, and through any two screws one cylindroid can always be drawn. The projections of all generators on a plane, perpendicular to the nodal line, form a flat pencil in which each ray corresponds to one screw. Also to each point on the circle corresponds one screw. We may here mention that this generation of the cylindroid stands in a very close relation to the plane representation of the cylindroid which is given in chapter xx. For if A, B are the ends of the diameter of the above circle which is perpendicular to the nodal line l, then to A and B correspond two generators of the cylin-

[1] "Theoretische Mechanik starrer Systeme auf Grund der Methoden und Arbeiten, und mit einem Vorworte von Sir Robert Ball, Royal Astronomer of Ireland." Herausgegeben von Harry Gravelius. (Berlin: Georg Reimer, 1889.)

droid which meet at right angles. Let the corresponding screws be a and β. Then if the circle when in a plane with a be turned about its diameter through a right angle it will be parallel to the plane of the pencil and may be taken to coincide with it. In this position we get the circle used in chapter xx. We recommend the reader to go through the first pages of this chapter when reading the third and fourth.

To combine two wrenches on two screws, a and β, we have to construct the cylindroid containing the screws and the flat pencil spoken of. If on the two rays in this pencil which are the projections of a and β the intensities of the wrenches be set off (they are the forces which together with couples constitute the wrenches), then their resultant gives not only the intensity of the resultant wrench, but it lies on the ray which is the projection of the screw of the resultant wrench. From this follows at once: Any two wrenches on screws of a cylindroid combine to a wrench whose screw lies again on the cylindroid; and conversely, a wrench on a screw belonging to a cylindroid can be decomposed into two wrenches on any two given screws on the cylindroid. Also, on any three screws of a cylindroid wrenches can be determined which are in equilibrium. It need scarcely be stated that the ratios only of their intensities are determined; but it is of importance to remember this.

The above results for the composition of wrenches hold also for twists about screws, provided that their amplitudes are very small, in conformity with the well-known fact that small rotations are combined in the same manner as forces. For this reason Sir R. Ball has limited his investigations to cases where the twist velocities have infinitely small amplitudes. These include equilibrium, beginnings of motion due to impulses and small oscillations. He also supposes the forces always to have a potential. Within these limits his results are of absolute generality.

The remarkable analogy between forces and rotations which appears in analytical mechanics rather as an accidental, though interesting, circumstance, is raised in the theory of screws to a principle of paramount importance.

If a rigid body acted on by a wrench receives a small twist, then the work done by the wrench is the product of the intensity of the wrench, of the amplitude of the twist, and of a geometrical factor which depends solely upon the two screws of the wrench and twist. Half this factor Ball calls "*the virtual coefficient of the two screws.*" If the screws meet it is proportional to the cosine of the angle between them; if the pitches of both screws vanish, or more generally if their sum vanishes, it becomes the moment of the two lines on which the screws lie. It partakes, therefore, of the nature of both these quantities, and its analogies to the cosine especially are, in many cases, very marked. If the virtual coefficient vanishes, then no work is done by the wrench in consequence of the twist. Now the virtual coefficient of two screws, a and β, depends symmetrically on both, hence if a wrench on a does no work when the body is displaced by a twist about β, then also a wrench on β does no work during a twist on a. For this reason two screws whose virtual coefficient vanishes are called *reciprocal*.

An immediate consequence of the definition of reciprocal screws is this, that a screw which is reciprocal to two screws, a, β, is reciprocal to all screws on the cylindroid determined by a, β. For a twist about any screw, γ, on the cylindroid can be decomposed into two about a and β; but the wrench can do no work against these, and therefore it can do no work against a twist about γ.

It is also proved that through every point in space there pass a single infinite number of screws, which are reciprocal to a cylindroid. These lie on the generators of a cone of the second order. Similarly, all screws in a plane which possess the property in question envelop a conic, and in chapter xxi. it is shown that this is always a parabola.

Two screws which meet can be reciprocal only if they meet at right angles or if the sum of their pitches vanishes. This gives rise to one of the most powerful methods for finding reciprocal screws. Thus, as every line meets a cylindroid in three points, and therefore cuts three screws on it, and as the cylindroid contains only two screws of equal pitch, it follows a screw, a, reciprocal to a cylindroid must cut one screw on it at right angles, and the two others which it meets must have equal pitches, viz. these must be equal and opposite to the pitch of a; and from this, again, it is easily deduced that every line which meets one screw on a cylindroid at right angles cuts, besides, two others which have equal pitch; for if on this line a screw be taken with a pitch equal to one of the two remaining screws which it cuts, it will be reciprocal to the cylindroid.

Just as two wrenches on screws a and β always combine to a wrench on a screw lying on a certain cylindroid, so three wrenches on screws a, β, γ, which do not lie on a cylindroid, combine to a wrench on a fourth screw which is connected with the three given ones, and which depends on the two ratios only of the intensities of the three given wrenches.

The entirety of all the screws which are got by varying these ratios forms a system of a double infinite number of screws, which has been called a screw-complex of the third order.

If any four screws belonging to such a complex are selected, then a wrench on one of them can be decomposed into three wrenches on the others. It is also always possible to determine wrenches on the four screws which are in equilibrium, and the ratios of their intensities alone are then determined. Similarly, five independent screws, *i.e.* screws which do not belong to a complex of lower order, give rise to a complex of order five, and six independent screws to a complex of order six. To this latter complex all screws in space belong, for in chapter v. it is shown that in general any wrench can be decomposed into six wrenches on six arbitrarily selected screws. A screw-complex of order two is nothing but a cylindroid, and a complex of order one consists of one single screw. That a complex of order six exhausts all screws in space, says only that the number of all screws is ∞^5, if ∞^1 denotes the number of points in a line, or the number of values which a single real variable, x, may assume. That the number of all screws is ∞^5 is also at once evident if we consider that the number of lines in space is ∞^4, and that on each line we have a single infinite number of screws which are obtained by giving its pitch all possible values from $-\infty$ to $+\infty$.

There is an important theorem that the screws which are reciprocal to all screws in a complex of order n form themselves a complex of order $6-n$.

One of the chief uses made of these results consists in the introduction of screw co-ordinates, viz. six independent screws are selected as co-ordinate screws. Then the intensities of the components of a wrench on these six screws are taken as the co-ordinates of the wrench. In the same way the co-ordinates of a twist are obtained. Lastly, by the co-ordinates of a screw are understood the co-ordinates of a wrench of unit intensity on the screw, or those of a twist of unit amplitude about it. To get, then, the co-ordinates of any wrench on, or a twist about, a screw, the co-ordinates of the latter have only to be multiplied by the intensity of the wrench or the amplitude of the twist. Between these screw co-ordinates exists, however, an equation of the second degree, just as between the ordinary homogeneous point co-ordinates there exists a linear equation. A screw is thus completely determined by the ratios of its six co-ordinates, *i.e.* by five numbers, which again shows that

there are ∞^5 screws in existence. Having established the notion of these co-ordinates, there are next given, in chapter v., expressions in terms of the co-ordinates for the resultant of a number of wrenches or twists, for the work done by a wrench on one screw during a twist on another, and so on. These expressions are much simplified by selecting the screws of reference in a particular manner, viz. so that any two of them are reciprocal, and such a system of "co-reciprocal" screws is afterwards always used.

The expression for the virtual coefficient of two screws is in general a lineo-linear function of the co-ordinates of both screws. But this is simplified for the special system of co-ordinate screws just mentioned, in reducing to an expression of six terms only, each being the product of the co-ordinates of the two screws relating to the same co-ordinate screw into the parameter of this screw. This expression must vanish if the two screws shall be reciprocal. Hence the condition that a screw shall be reciprocal to a given screw is expressed as a linear equation between its co-ordinates, and it is important to add that every linear equation between its co-ordinates can be interpreted as meaning that the screw is reciprocal to some other screw. But one linear equation enables us to express one of the co-ordinates in terms of the others, so that all the co-ordinates of all screws which are reciprocal to a given screw can be expressed in terms of five co-ordinates, in other words, a screw in a complex of order five is determined by five co-ordinates. In the same way two linear equations limit a screw to a complex of order four, and so on, till we come to five equations as determining one single screw; which also shows that there is always one screw which is reciprocal to five given screws.

We leave for the moment the line followed by Ball and Gravelius, in order to indulge in some very general speculations, in close connection with chapter xix., which seem best suited to give, in as short a compass as possible, a clear insight into the nature of the whole system of screws.

We are accustomed to express the fact that the number of points in a plane is ∞^2 by saying a plane, or in fact any surface, is of two dimensions if we consider the points as elements. Space is, in the same sense, of three dimensions, whilst it is of four dimensions if we consider the lines as elements.

We may extend this language, and say the aggregate of all screws forms a space of five dimensions, or as Clifford would have said, it is a five-way spread. If we now assume between the co-ordinates one equation, we may speak of the locus of screws whose co-ordinates satisfy this equation. It will be a four-way spread, or a space of four dimensions. This locus is called by Ball a screw-complex of order five and degree m, if m is the degree of the equation. The complexes spoken of before are of the first degree.

The geometrical theory of screws becomes thus identical with the geometry of a space of five dimensions, which latter we may call the screw-space.

Let us consider now two such complexes of 1st degree, one of order m, the other of order n. The first is determined by a set of $6 - m$ linear equations between the co-ordinates, the second by $6 - n$ such equations. All screws common to both have therefore to satisfy $12 - m - n$ equations, and in case that this number is not greater than six, they will constitute a complex of order $6 - (12 - m - n) = m + n - 6$. Thus a complex of order 4 and a complex of order 5 will have a complex of order 3 in common, whilst two complexes of order 3 will in general have no screw in common, though they may have a single screw or a whole cylindroid in common.

The geometrical theory of screws as the geometry of a particular space of five dimensions is not a mere extension of the ordinary Euclidian geometry. The six homogeneous co-ordinates of a screw are, as has already been mentioned, connected by an equation. This is of the form $R = 1$, where R is a quadratic expression of the co-ordinates. All elements at infinity in our screw-space are given by the equation $1 = 0$ or by $R = 0$. The *absolute* is thus a quadric locus, and therefore we have to deal with non-Euclidian geometry.

The advantage to the theory of screws to be derived from a study of this geometry are apparent at every step. We may in our screw-space conceive curves and surfaces of from 1 to 4 dimensions, by taking one or more equations between the co-ordinates. Of these, equations of the first degree determine the screw-complexes. But equations of the second degree, which determine quadric complexes, or as Ball calls them screw-complexes of second degree, are also constantly of use. Such an equation may be taken in a complex of order n. In the treatise before us they appear in congruences of the 3rd and 6th order. We will give here one illustration.

Let $p_1, p_2, \ldots p_n$ be the pitches of the n co-reciprocal co-ordinate screws, and let $\alpha_1, \alpha_2, \ldots \alpha_n$ be the co-ordinates of a screw α with pitch p_α. Then is p_α given by the equation

$$p_\alpha = p_1\alpha_1^2 + p_2\alpha_2^2 + \ldots\ldots + p_n\alpha_n^2.$$

This equation can be made homogeneous by aid of $R = 1$, and becomes

$$Rp_\alpha = p_1\alpha_1^2 + p_2\alpha_2^2 + \ldots\ldots + p_n\alpha_n^2,$$

where R also is supposed to contain n of the α only, the others being replaced by aid of the linear equations which determine the complex of order n. It follows that the absolute $R = 0$ is the locus of screws of infinite pitch, whilst

$$p_1\alpha_1^2 + p_2\alpha_2^2 + \ldots\ldots + p_n\alpha_n^2 = 0$$

is the locus of screws of zero-pitch. Both are quadrics.

If we take a screw β, we may form its polar with regard to any quadric. If we select the last quadric mentioned, the polar is

$$p_1\alpha_1\beta_1 + p_2\alpha_2\beta_2 + \ldots\ldots + p_n\alpha_n\beta_n = 0.$$

But this equation is also the condition that α and β are reciprocal screws. In each complex the quadric of zero-pitch becomes thus of special importance, reciprocal screws being conjugate poles with regard to them.

As we cannot directly realize a space of more than three dimensions, it becomes of importance to represent the elements in such a space by other elements in ordinary space, and, when possible, by elements in a plane. That this is always possible is clear.

For instance, as all conics in a plane are ∞^5 in number, we have as many conics in a plane as there are screws in space, and we may therefore represent each screw by a conic in a plane. To screws on a cylindroid would then correspond all conics in a pencil. We might then speak of the cross-ratio of four screws as given by the cross-ratio of the corresponding conics in the pencil. All screws belonging to a complex of order 3 would be represented by conics forming a net, *i.e.* by conics having a common polar triangle.

We thus get a graphical representation in a plane, and can obtain our results by constructions in a plane. But the geometry of conics in a plane has scarcely been far enough developed to make general use of them, and for screw-complexes of lower order simpler representations may be found. Thus the screws on a cylindroid can be represented most conveniently by the points on a circle which stands in close relation to the cylindroid and gives rise to a graphical solution of problems relating to a body with two degrees of freedom. This is done in chapter xx., full of interesting detail. Again, screws in a complex of order 3, whose number is ∞^2, can be represented by

points in a plane. This has been worked out in chapters xxi. and xxii. In fact, here the screw co-ordinates, three in number, are simply taken as tri-linear co-ordinates of a point. It follows at once that the locus of points with equal pitch must be a conic, the "absolute" being the locus of pitch ∞, and one conic relates to zero-pitch. This latter may, without loss of generality, be made a circle.

It is of interest to notice that for a screw-complex of order 3 the screws which have a given pitch form themselves a quadric surface, viz. they form one set of generators on a hyperboloid, the other set of generators having pitch $-p$, and containing thus screws in the complex reciprocal to the others.

Other quadrics enter the theory, especially one containing the locus of screws about which a body may twist without receiving kinetic energy, and which is, of course, imaginary; and one connected with the potential. These last two determine the principal screws of inertia, of which more later on.

For screw-complexes of order 4 no graphical representation is given. The difficulty lies here in this—that the dynamics require constantly metrical relations, and these are not very simple in the plane representation, by conics for instance. It is here that the non-Euclidian character of the geometry comes out.

These speculations are in close connection with the contents of chapter xix., where projective relations between two congruences of the same order are investigated. It is here that Herr Gravelius has more particularly introduced original work of his own in bringing Sir R. Ball's Mr. One-to-one more prominently to the foreground.

Up to this we have considered chiefly the geometry of systems of screws. It is now time to consider the kinematics of a rigid body and the action of forces on it.

If a body is perfectly free it can twist about every screw in space. As these can be decomposed into six twists about the co-ordinate screws, the body is said to have six degrees of freedom. If the body is constrained in any manner—and here the generality of the nature of the constraint has to be noticed—then it will not be able any longer to twist about all screws. But we have seen already if it can twist about n screws it can twist about all screws belonging to the complex of order n derived from them. The freedom of a body is therefore fully characterized by the complex which contains all screws about which the body can twist. If this is of order n, then the body has n degrees of freedom. An attempt to twist the body about any other screw will evoke a reaction due to the constraint which will reduce to a wrench upon some screw. Such a wrench cannot do any work against a possible twist of the body, hence the screws on which wrenches of constraint are possible must be reciprocal to the screws which determine the freedom of the body; they form, therefore, the reciprocal complex. We thus get the very general theorem about the equilibrium of a body. If a body has n degrees of freedom then it will be in equilibrium under the action of all wrenches on screws of a certain complex of order $6 - n$. This complex may be called the complex of constraint.

Again, if a body is subjected to an impulsive wrench upon a screw, η, not belonging to the complex of constraint, it will begin to turn about some screw, α, called the instantaneous screw. At the same time an impulsive wrench of constraint will be evoked. Conversely, in order to produce a twist on α as instantaneous screw we may apply an impulsive wrench on η, but with this we may combine a wrench on any one of the screws belonging to the complex of constraint. As the latter is of order $6 - n$, all screws derivable from these, together with the screw η will form a complex of order $7 - n$ This complex of order $7 - n$ and the complex of order n which determine the freedom have $7 - n + n - 6 = 1$ screw in common (see above). This screw is called the reduced impulsive wrench.

We thus have proved if a body has freedom of order n, then there is always one and only one screw, η, in the complex which determines the freedom, such that an impulsive wrench on it makes any given screw, α, the instantaneous screw. The converse, also, is evidently true. Between the impulsive and instantaneous screw in the complex exists, therefore, a one-one correspondence, or, to express this differently, the complex of instantaneous and that of impulsive screws are projective. They are also coincident. But if we have two coincident projective spaces of $n - 1$ dimensions, then there are always n screws in one which coincide with their correspondents. This proves if a body has n degrees of freedom, then there exist n screws, and in general only n, such that an impulsive wrench on one of them produces a twist on the same screw. These n screws—and the discovery is one of the triumphs of the theory—are called the principal screws of inertia, as they depend on the distribution of matter in the body. These screws are also co-reciprocal, and may therefore be taken as co-ordinate screws. They are a generalization of the principal axes of inertia in the ordinary theory; and to show their importance it is sufficient to point to the importance of the principal axes of inertia in the ordinary theory of a free body, or of a body of which one point is fixed, and to remember the simplification obtained by taking them as axes of reference.

For a free body the screws of inertia lie on the principal axes of the body which pass through the mass-centre, two on each, with pitches equal to the corresponding radius of gyration, taken positive for the one and negative for the other. The ordinary theory has no analogon to this if the body is constrained, excepting in the few cases where a point or an axis of the body is fixed, or where the body has plane motion only.

It is in such generalizations that the theory of screws excels. It has given us here the best and simplest co-ordinates for all cases of the motion of a single rigid body acted on by any forces and constrained in any manner conceivable.

We will now suppose that the co-ordinates thus pointed out are used, and find the instantaneous screw corresponding to any given impulsive wrench. Each component wrench produces a twist about its own screw, whose amplitude depends in a very simple manner on the intensity of the impulsive wrench; so that the intensities of the component twists are known, and these give the resultant twist.

We next consider the kinetic energy, T, of the body due to a twist on a screw, α. Let $\alpha_1, \alpha_2, \ldots$ be its components, $p_1, p_2, \ldots$ the pitches of the co-ordinate screws, and $\dot{\alpha}$ the twist velocity. It is then shown that, M being the mass of the body,

$$T = M\dot{\alpha}^2(p_1^2\alpha_1^2 + p_2^2\alpha_2^2 + \ldots + p_n^2\alpha_n^2).$$

Denoting the expression in the brackets by u_α^2, we have $T = M\dot{\alpha}^2u_\alpha^2$. The quantity u_α is a length; the expression for T is therefore of the same form as that for the rotation of a body about an axis with angular velocity $\dot{\alpha}$, the radius of gyration being replaced by $u_\alpha/\sqrt{2}$. This last expression deserves a name. If we adopt Clifford's word "spin-radius," instead of radius of gyration, the name twist-radius suggests itself as suitable for u_α or $u_\alpha/\sqrt{2}$.

We now come to consider the problem of small oscillations. Let there then be a body of n degrees of freedom in a position of equilibrium under a system of forces which have a potential V. Let A denote the complex defining the freedom. If the body be displaced by a small twist about a screw, α, belonging to the complex A, then the forces are not any longer in equilibrium; hence they will give rise to a wrench on some screw λ. This wrench may be combined with any wrench of constraint; but just as in case of impulsive wrenches there is one single screw

λ belonging to the complex A, hence now also we have in the complex A a one-one correspondence between the screws a and the screws λ. There are therefore, again, n screws a, which coincide with their corresponding screws λ. These have got the name of "principal screws of potential." They depend on the system of forces or on the potential V, just as the screws of inertia depend on the distribution of matter. These n screws, again, are co-reciprocal. They have the property that a twist about one of them evokes a wrench on the same screw, the wrench being due to the applied forces. To show the importance of these principal screws of potential it will be sufficient to remark that the potential is, under the circumstance explained, a homogeneous function of the second degree of the n co-ordinates by which the displacement is defined. This function becomes the sum of n terms containing the squares only of the co-ordinates if the principal screws of potential are taken as co-ordinate screws.

Now, suppose that the body has been displaced by a twist about a screw a, this could be done by a wrench upon the screw η, which as impulsive screw corresponds to a as instantaneous screw. At the same time this displacement calls a wrench on a screw λ into play due to the potential V. To every screw a corresponds thus one screw η and one screw λ. Hence the latter are also connected by a one-one correspondence, and there are therefore n screws a such that the corresponding screws η and λ coincide. The screws a thus obtained are called "harmonic screws." They possess this property: A twist about a harmonic screw evokes a wrench which in its turn tends to produce a twist on the original harmonic screw. Hence if the equilibrium is stable this wrench will tend to twist the body back to the position of equilibrium, and thus produce small oscillations about the harmonic screw. From this we get the following theorem, distinguished again by its great generality:—

If a rigid body having n degrees of freedom is in a position of stable equilibrium under the action of a system of conservative forces, then it can, on being disturbed, perform n distinct oscillations, which consist each of a twisting about a single screw. Every other oscillation is a combination of these.

These are the chief results which so far have been obtained by the theory of screws as applied to a single rigid body. They form the contents of chapters vi. to xii. These general results are, in the next six chapters, applied and considered more in detail for each of the six possible cases of degrees of freedom which a rigid body may have. Then there follow four chapters on graphical methods, already referred to.

All the former investigations relate to one single rigid body. But Sir R. Ball, in 1881, published a paper in which he extends his theory to systems of rigid bodies by a method as beautiful as it is suitable to the purpose.

The bodies, of which we suppose there are μ, are taken in a definite order. Every body of the system will at every moment twist about some screw. We thus get a et of μ screws, about which at any moment the bodies twist. If we take two consecutive twists, then their resultant depends only on the ratios of the two amplitudes, and conversely the screw of the resultant determines this ratio. If the screws about which two consecutive bodies twist are given, and also the screw on which their resultant lies, then the amplitude of the first twist determines that of the other. If, therefore, the screws about which the μ bodies twist at any moment are given, and besides the $\mu - 1$ screws on which the resultant twists of consecutive bodies lie, then the amplitude of the first determines that of every other twist. The set of $2\mu - 1$ screws thus obtained is called a screw-chain, and it is said that the system of bodies twists at any moment about a certain screw-chain.

In case of systems of rigid bodies, the screw-chain has to be considered as the fundamental entity, which takes the place of a screw in case of a single body.

In a finite number of bodies we get a screw-chain of a finite number of screws. These will, in the screw-space of five dimensions, be represented by a finite group of points (elements). If, however, the number of bodies increases and becomes infinite, as in the case of the molecules of a fluid, this group of points may form a continuous locus of one or more dimensions. We may thus get, instead of screw-chains, continuous curves and surfaces of screws, and their geometry will be that of a group of points in five-dimensional non-Euclidian space.

This suggests an enormous field for investigation, and it is of interest to see that every progress in the algebra and geometry of such a space must indicate also progress in dynamics.

But these are speculations far beyond the contents of the book under review.

All results obtained for twists can at once be transferred to wrenches. Accordingly a system of forces acting on a system of bodies can be reduced to a wrench upon a screw-chain.

There are reciprocal screw-chains, screw-chains of inertia, complexes of screw-chains, complexes of freedom and of constraint, and complexes reciprocal to them. In fact, the screw-chain seems now to take in every respect the place of the screw in the theory of a single body. These screw-chains in their kinematical and dynamical applications to systems of rigid bodies form the contents of the chapters xxiii. and xxiv.

The last two chapters in the book give Sir R. Ball's theory of content, in which the author tries successfully to overcome the difficulty which offers itself in the determination of metrical relations without any reference to measuring a length. By "content" is understood the aggregate of all elements in what Clifford called a three-way spread. The investigation is carried on quite algebraically by aid of the methods of Grassmann's "Ausdehnungslehre." In the book before us this is worked out, partly with reference to Clifford's theory of biquaternions, and ends with the introduction of Clifford's vectors in non-Euclidian space.

It will be asked what progress in the science of dynamics, and through dynamics in natural philosophy, has been made by Ball's creation. The theory of screws is a mathematical speculation full of life, full of interest and charm for the mathematician who likes to find new physical interpretations for geometrical and algebraical results and methods. The physicist, however, may say that the theory does not increase our power over Nature. But I am inclined to think that when further developed it will be a great, perhaps a very great, help to progress. Does not every molecule of a fluid having rotational motion twist about some screw? And does not a vortex-line suggest a screw-chain containing an infinite number of elements?

The theory of screw-chains, containing a finite number of elements belonging to a system of bodies with one degree of freedom, seems to indicate a truly scientific classification of mechanisms, and may conceivably render great aid in the invention of mechanisms which answer a given purpose.

The essentially geometrical character of the new method seems particularly well adapted to give graphical solutions of dynamical problems, and thus a "graphical dynamics" appears to find here a sound foundation. In this direction much has been done already, but much remains to be done. Also the restrictions of infinitely small amplitudes of the twists has to be broken through, and the infinitesimal calculus has to be pressed into the service.

Meanwhile, we congratulate Sir Robert Ball on the results which his persevering labours have achieved, and Herr Gravelius on the courage which led him to under-

take the task of writing a text-book on this subject, and on the success with which he has accomplished it. The book ought to give a great impulse to the study of this theory, and to enlist many friends in its service.

O. Henrici.

THE SIXTH SCIENTIFIC CRUISE OF THE STEAMER "HYÆNA" WITH THE LIVERPOOL MARINE BIOLOGY COMMITTEE.

The Liverpool Salvage Association having kindly placed their s.s. *Hyæna* once more at the disposal of the Liverpool Marine Biology Committee, a four-days' dredging cruise was arranged and successfully carried out at Whitsuntide. The old gunboat left the Mersey on Friday, May 23, and steamed to the Menai Straits. Some of the party spent the afternoon and evening collecting on the shore at Puffin Island, off which the *Hyæna* was anchored for the night. On the following morning, after a few hauls of the dredge near Puffin Island, and between Penmon Point and Beaumaris, and again off Port Dinorwic, the steamer went through the straits to Carnarvon Bay, and commenced working along the southern coast of Anglesey.

The dredges and various kinds of tow-nets, surface and bottom, were used at intervals. Mr. W. E. Hoyle's deep-water closing net, which has now been modified so that its movements of opening and closing are effected by the passage of an electric current, was experimented with frequently during the cruise—not so much with the object of collecting specimens, as for the purpose of detecting and remedying any possible defects in the construction, and of guarding against conditions which might interfere with the proper action of the apparatus. On the whole the net worked satisfactorily, the causes of occasional failures were discovered, and when the improved form of frame used by the Germans has been adopted, the apparatus will no doubt be a most useful addition to the implements of the marine biologist.

The *Hyæna* anchored for the night in a small rocky bay, Porth Dafarth, on the south side of Holyhead Island, Anglesey, and half the party of over twenty biologists were landed to sleep on shore. After dark those who remained on board commenced tow-netting by electric light, and repeated with some modifications the experiments which had been made during the last two cruises of the *Hyæna* at the Isle of Man (Nature, vol. xxxviii. p. 130, and vol. xl. p. 47) in 1888 and 1889. The large arc lamp was hoisted over the side of the ship so as to throw a strong glare on the water, and Edison-Swan incandescent lamps were sent down to the bottom in tow-nets which were hauled up at intervals. Comparatively few Cumacea, Amphipoda, and Schizopoda were obtained this time, but shrimps and young fishes were—for the first time in our experience—attracted by the light to the surface, and some of them were caught and preserved. One of the ship's boats was kept in the area illuminated by the arc lamp, and by leaning over her side the small objects in the surface-layer of water could be most distinctly seen, and particular animals picked out and captured with a hand-net as they darted about in the neighbourhood of the light.

Two of the party got up at 3 a.m., and took a surface tow-netting about dawn, which was afterwards found to contain a much greater number of Copepoda, and more variety, than any of the other tow-nettings, either day or electric light, surface or bottom. Amongst other interesting things it contained a large number of *Peltidium depressum*, which had not been taken at all during the day, and only in very small numbers with the electric light bottom net. This same species has recently been taken in quantity at Puffin Island by leaving a tow-net out all night attached to a buoy. It is usually found sticking on *Laminaria* in the day-time, but evidently comes to the surface in abundance late at night or early in the morning.

The following day was spent in steaming slowly about off the southern coast of Anglesey, dredging and tow-netting at frequent intervals. The surface life was found to be very poor—comparatively few Copepoda and almost no representatives of other free-swimming groups being obtained; but Mr. Thompson noticed the relative abundance in all the tow-nettings, both surface and bottom, during the day, and also with the electric light, and at dawn, of unusually large specimens of *Dias longiremis*, and also the prevalence of the somewhat uncommon *Isias clavipes* in all the surface gatherings, though none were taken in the bottom ones.

The dredging results were fairly good: some very fine sponges were obtained, and Ascidians were plentiful. One patch of rich ground was discovered near Rhoscolyn Beacon, where *Comatula* was brought up in abundance along with various Tunicata, Holothurians, Nudibranchs, Zoophytes, Polyzoa, and large sponges. After dark, in Porth Dafarth, the electric lights were again used for a couple of hours. This time the large arc lamp was taken to the stern and suspended close to the surface of the water, but as it was not working steadily one of the incandescent submarine lamps was lowered over the side and kept a few inches under water, and this proved most effective in attracting animals to a stationary tow-net or a hand-net beside it. On the fourth day the *Hyæna* returned through the Menai Straits to Liverpool. As usual the specimens collected have been distributed to specialists, and the detailed reports upon the various groups will appear in the next volume of the "Fauna of the Liverpool District."

W. A. Herdman.

W. S. DALLAS.

The death of this genial and accomplished man will awaken feelings of no ordinary regret, not only among geologists, but among naturalists all over the country. For two-and-twenty years his tall, handsome person has been the most familiar figure at the rooms of the Geological Society in Burlington House. Always at his post, with a pleasant smile of welcome, ever ready with assistance from his large treasures of knowledge and experience, knowing more intimately than anyone else the affairs and traditions of the Society, proud of its history and keenly sensitive for its scientific reputation, he had come to be looked upon as a kind of *genius loci*—the living embodiment of the Society's aims and work.

Of those who knew Mr. Dallas only in his later years, and saw his whole-hearted devotion to the geological labours intrusted to him, probably few were aware that he was not always a geologist. He began life with zoological inquiries, and devoted his attention more especially to insects. His early papers appeared in the Transactions of the Entomological Society, but he prepared also a Catalogue of the Hemipterous Insects in the British Museum, which was published as far back as the years 1851–52. Yet he did not confine himself to one branch of zoology; on the contrary, his reading and knowledge ranged over a wide domain in natural history. In the year 1856 he published his "Natural History of the Animal Kingdom," by far the best work of the kind in its day, which rendered important service to biology, in making the study of living forms more attractive, and in providing for that study a much more accurate groundwork than had ever before been obtainable. The value of his labours was recognized not long afterwards by his being appointed Curator of the Yorkshire Philosophical Society's Museum at York—an office which he held for ten years, until in 1868 he obtained the post which he held up to the last—that of Assistant Secretary, Librarian, and Curator to the Geological Society of London.

After his return to reside in London he found the duties of the office he had undertaken so engrossing, and the cares of domestic life so exacting, as to leave him little or no spare time for original inquiry. He devoted such leisure as he could command to translating, editing, and other scientific labour of a literary kind. Biologists will especially remember the appearance of his translation of Fritz Müller's "Facts and Arguments for Darwin," shortly after the beginning of the controversy aroused by "The Origin of Species." His wide range of knowledge in natural science, and his literary tact and experience, made him an unrivalled editor of a scientific periodical. The volumes of *The Quarterly Journal of the Geological Society* for the last twenty years will remain as a memorial of the accuracy, skill, and punctuality of his work. It will be difficult to find another assistant secretary so deft and helpful as he: it will be, however, still harder to discover one who to ample scientific acquirements and long experience will unite a nature so gentle and kindly as his, and a character so honourable and sincere. Mr. Dallas may be said to have died in harness. Though for some time he had been growing gradually feebler, he attended the evening meeting of the Geological Society only a fortnight ago. But the hand of death was then visibly upon him. Two days afterwards he was struck down with paralysis, and, after lingering a week, died on the morning of May 28, at the age of sixty-six. Last Monday his associates of the Geological Society laid him in his grave in the Norwood Cemetery. A. G.

NOTES.

BESIDES the death of Mr. W. S. Dallas, the Assistant-Secretary of the Geological Society, the ranks of the geologists of this country were further thinned last week by the loss of another well-known and most esteemed student of geology—Mr. John Gunn, of Norwich. Though not distinguished as a writer on geological subjects, he has long been looked up to as the chief authority on that most interesting deposit—the Cromer Forest-bed; and as the most indefatigable and successful collector of its organic contents. He had, moreover, an extensive knowledge of all the geological formations of East Anglia. He was, likewise, fond of antiquarian researches, and in early life did good service among the archæological and ecclesiastical antiquities of his county. But while always eagerly seeking fresh information and gathering a vast store of facts in many departments of inquiry, he refrained from rushing frequently into print, while on the other hand, with generous self-abnegation, he was ever ready to place his materials at the service of science and the public. Every honest inquirer was always welcome to any information or assistance he could give. After amassing a magnificent suite of fossils, illustrating especially the mammalian life of Pliocene time in England, he presented it to the Norfolk and Norwich Museum, where it forms one of the most attractive and instructive features of the collection, and fills what is called after him the "Gunn Room." Mr. Gunn had reached his eighty-ninth year.

WE are glad to gather from the statement made in the House of Commons on May 22 by Sir John Gorst, in reply to a question from Sir Henry Roscoe, that the new regulations which will shortly be issued by the Civil Service Commission for the competitions for admission to the higher branch of the Indian Civil Service are, in the opinion of Sir John Gorst, likely to satisfy the desire which is widely felt at the Universities and elsewhere that they "shall secure more equal prospects of success for those whose chief studies have been in science than are at present accorded in these competitions." Those who are interested in this important educational question will be glad that Sir Henry Roscoe has directed the attention of the authorities at the India Office to this matter, and they will hope that if the new regulations are not found to satisfy the necessities of the case, he will continue his exertions. We do not wish to be prophets of evil, but experience unfortunately shows that the Civil Service Commissioners are by no means likely to put science subjects on anything like a fairly equal footing with classics except under considerable pressure from public opinion. It will therefore be important that prompt combined action shall be taken in support of Sir Henry Roscoe by those who have interested themselves in the question, if the new regulations do not prove to be of a satisfactory character. If the present opportunity of securing that the conditions of admission to this important service be put on a proper footing be lost, it may be long before another occurs. Such action has, however, succeeded in other cases, and ought to do so in this case also.

IN moving the Education Estimates on Tuesday evening, Sir W. Hart Dyke gave an elaborate and most careful account of the new Code, the leading provisions of which we have already discussed. Among the speakers who took part in the subsequent debate or conversation was Sir Henry Roscoe, who congratulated the Vice-President on having for the first time carried out some of the recommendations of the Royal Commission on which he had had the honour to serve. He welcomed the proposal to give a grant for manual instruction. He was also pleased to learn that the Vice-President took to heart one of the recommendations which laid the foundation for technical instruction—a foundation which many of them for a long time had hoped would be laid. It was gratifying to learn that already great progress had been made in several of the larger towns with regard to technical instruction. He hoped that the question of drawing would progress. He thought the specialization of science ought not to be made before the fourth standard. The question of training teachers was one which referred to probably the most important portion of the Code. He welcomed all that it was proposed to do. He believed that the new Code would mark an era in the educational progress of the country. Mr. Mundella, in the course of a short speech, said he had risen only to express his thanks to the Vice-President of the Council for the liberal provisions of his Code. He regretted, however, that these provisions had not been somewhat extended. Why had the Vice-President not gone somewhat further with respect to the recommendations of the Royal Commission as to raising the standard of age, and extending the school life of the child? They might make the best and most liberal arrangements for education, but if the child's school life was to end at ten years of age, they were wasting their money. In large towns there were thousands of children who went to full-time labour after the fourth standard. In many rural districts, especially in the west, the second standard was the half-time standard, and two years ago that had been the case in Bradford. Why could not the right hon. gentleman screw up his courage and adopt the recommendation of the Royal Commission, and do for England what was done in Scotland? They should have a minimum standard for half-time. He hoped that later on the right hon. gentleman would be able to announce that he had made some provision for meeting the suggestions which had been offered with regard to raising the age at which the school life of the child should end, and raising the full and half-time standards.

A DEPUTY Linacre Professor of Human and Comparative Anatomy is to be appointed at Oxford. He will hold office during the continuance of Prof. Moseley's illness. Candidates must send in their applications on or before June 21.

GOOD progress has been made with the arrangements for the fifty-eighth annual meeting of the British Medical Association, under the presidency of Dr. W. F. Wade, senior physician to

the Birmingham General Hospital, to be held in Birmingham on July 29, 30, and 31, and August 1 next. There will be three addresses—an address in medicine, by Sir W. Foster, M.D., M.P., of Birmingham; an address in surgery, by Mr. Lawson Tait, of Birmingham; and an address in therapeutics, by Dr. William Henry Broadbent, of London. The scientific part of the meeting will be carried on in twelve sections. It is now fifty-six years since the Association first held its meeting at Birmingham.

At a meeting of the London Committee of the Edinburgh Exhibition on Tuesday, Mr. S. Lee Bapty, the general manager of the Exhibition, said the visitors during the first month had numbered 470,000. This was largely in excess of his most sanguine anticipations, and was all the more remarkable considering the state of the weather during most of the month. If the same number of visitors continued each month till October, there would be a total of over four millions. A very important exhibit of electrical appliances from forty manufacturers in France had just arrived, and these would be on view at the time of the approaching visit of the Lord Mayor and Sheriffs of London to Edinburgh.

At the meeting of the Society of Arts on May 15, Mr. C. Washington Eves read a valuable paper on Jamaica and its forthcoming Exhibition. Apparently there is good hope that the Exhibition will be a decided success. The exhibits will be divided into six groups—raw materials; implements for obtaining raw materials; machines and processes used in preparing and making up the raw materials into finished products; manufactured goods; educational appliances; fine arts, literature, and science. The section devoted to science will include maps and charts of the West Indies, and objects relating to engineering, sanitation, gas, electricity, astronomy, and anthropology. After the reading of the paper there was a discussion, in the course of which Mr. Morris, of Kew, said there was every indication that makers of machinery and others would send out appliances, and there was but little doubt that immense good would result to the island from the Exhibition.

The last Friday evening discourse at the Royal Institution will be given on June 13, by Prof. Silvanus P. Thompson. The subject will be "The Physical Foundation of Music."

The authorities of Wadham College, Oxford, announce that in the election to one of several exhibitions which are open to competition preference will be given to any candidate who shall undertake to read for honours in natural science from the time of his admission into College, and to proceed to a degree in medicine in the University of Oxford.

The *American Naturalist* states that the Marine Biological Laboratory at Boston, U.S.A., has issued a satisfactory annual report. The laboratory was crowded last summer, and the trustees appeal for donations to the amount of 7000 dollars for additions to the building, an increase in the library, and a steam-launch.

The Botanical Society of Regensburg—one of the oldest societies of the kind in Germany—celebrated its hundredth anniversary on May 15.

The late Herr M. Winkler, of Görlitz, has bequeathed his fine herbarium, comprising 150,000 specimens, and his botanical library, to the Botanical Garden at Breslau.

The members of the German and Austrian Alpine Club have elected a scientific committee, consisting of Prof. Penik, Vienna, Dr. Finsterwalder, Munich, Councillor Hann, Vienna, Prof. Partsch, Breslau, and Prof. Richter, Graz. This committee will investigate scientific questions relating to the Alps, devoting especial attention to glaciers and mountain streams. The results will be made known in the official publications of the Club.

The new Zoological Garden and Park at Rock Creek, Washington, to which we referred the other day, will be under the direction of Mr. W. T. Hornaday. It is stated that Prof. Frank Baker will be prosector, and will have charge of the department of comparative anatomy in the United States National Museum.

Telegrams received at New York on June 3 stated that shocks of earthquake had been felt at Lima on the previous morning. The earthquake was one of the severest that had been experienced there for years. There were three distinct shocks.

We learn from *Science* that the *Princess Louise*, which arrived at Victoria, B.C., from Skidegate and way ports, on the evening of April 24, brought news that on February 24 an earthquake shock was felt on all the islands around Skidegate, especially on the west coast of Queen Charlotte Islands, where a few old shanties were levelled to the ground. The totem-poles of the Indians shook like leaves, and in some places the earth was cracked. The shock lasted for about thirty seconds, during which time the Indians were wild with fright. A number of them ran to the church and crowded in. Since that time there have been about twenty different shocks, the last one being on April 12, although none was nearly so severe as the first. A very slight shock was felt at Skeena.

Dr. David P. Todd, writing to the *Nation* from the U.S.S. *Pensacola*, at Ascension, on March 16, refers in terms of high appreciation to the work done in meteorology by his colleague Prof. Abbe. A "nephoscope" was specially constructed for the Expedition on board the *Pensacola*. Prof. Abbe has elaborated a method for the use of this instrument in determining the actual height and velocity of clouds by combining observations made when the vessel or observer moves successively in two different directions, or with two different velocities; and he calls this the "aberration method," to distinguish it from ordinary parallax methods. His main work has been a determination of the motions of the atmosphere from a study of the lowest winds and the successive strata of clouds; and, to this end, he has maintained daily observations with the nephoscope at sea, and when possible on shore. The visible clouds, he concludes, give little or no information as to the motions of the atmosphere in the widest sense, but prove that the atmosphere is everywhere divided into local systems of currents, so that we have winds circling around a storm-centre, a high barometer, an ocean, or a continent; and, at least in the Atlantic, have no winds that circulate exactly as they would do on a rotating, uniform, smooth globe. The angles of inflow and outflow have been determined for three or four successive strata of air in mid-Atlantic; also the relations of the cloud-appearances to distant storms, squalls rains, and changes of wind, with such accuracy that on many occasions predictions of such phenomena have been made and verified.

Mr. S. H. C. Hutchinson, Meteorological Reporter for Western India, has written an excellent "Brief Sketch of the Meteorology of the Bombay Presidency in 1888-89." The meteorology of that year was characterized, Mr. Hutchinson says, by strongly marked deviations from the weather conditions of an average year. Of these, the most noteworthy were, a general rise of abnormal barometric pressure for a considerable period, a general deficiency of rainfall in September, and the scanty rainfall throughout the year. Mr. Hutchinson points out that all these variations are of much practical importance, and, from a scientific point of view, of considerable interest, inasmuch as they confirm the laws or principles deduced from the meteorological data of many past years. These laws or principles are, that the rainfall is deficient when barometric pressure is above

the normal height, and excessive when the barometric pressure is lower than usual; that at or about the epochs of minimum solar spotted area, high abnormal barometric pressure movements make their appearance, and that at or about the epochs of maximum solar spotted area, abnormally low pressure movements take place in India and over greater part of the tropics; that cyclones are formed in the trough of a relatively minimum barometric pressure; and lastly, that the number of atmospheric disturbances is great at the epoch of minimum sunspots.

IN Dr. A. Petermann's *Mitteilungen* (Heft v., 1890), Dr. A. Supan gives some particulars respecting Emin Pasha's meteorological journal, which will shortly be published. The registers extend from August 1, 1881, to February 27, 1890, and, omitting the interruptions, contain observations for seven years and ten months. They are said to have been taken with great care, and may be divided into three periods: August 1, 1881, to April 24, 1885, at Ladó; July 13, 1885, to December 5, 1888, at Wadelai; and March 1 to December 4, 1889, during the march with Stanley to the coast. On the latter date Emin Pasha met with his serious accident, but so great was his desire to continue the observations, that he resumed them on January 5, 1890, in the German hospital at Bagamoyo. Dr. Supan regrets the non-publication of Mackay's observations at Rubaga (Uganda), which were sent to the Royal Geographical Society in 1886, as they promise to be the most important contribution to the climatology of the interior of tropical Africa.

A CHEAP bunsen burner is being sold by Messrs. John J. Griffin and Sons, which possesses many advantages over the ordinary burner, with central gas jet constructed so that the gas and the air may be simultaneously regulated. In the new patent burner the gas passes into the tube through a way cut in the side of the tube, which is therefore open from top to bottom. Such an arrangement is a considerable improvement, inasmuch as there is no jet to become choked. The burner can also be easily taken apart in order to clean the tube when corroded. To regulate the flow of gas under varying gas pressures small movable disks are provided, which, however, are little better for the purpose than the older method of rotating a cylinder concentric with that containing the air-inlets. Combinations are also made in which each burner can be regulated or extinguished separately, thus rendering them very suitable for combustion furnaces.

AN elaborate Report on the Natal forests, by Mr. H. G. Fourcade, has just been issued. He arrives at the following conclusions:—(1) The Natal forests, more particularly the timber forests, are well worth preserving, whether from an economic or climatic point of view, and the Government alone is competent to undertake the work. (2) The condition of the forests is, for the most part, lamentable, and the result of past abuses; their destruction is proceeding apace, and the following measures are recommended to insure their preservation and utilization to the best advantage: (*a*) The survey and demarkation of the principal forests. (*b*) Their protection from fires, from depredations, from destruction by natives or cattle, by means of suitable measures, such as the clearing of fire-belts, the establishment of small wattle plantations, the prohibition of wattle-cutting and cattle-grazing, with the aid of proper supervision and special legislation. (*c*) The closure of the forests pending survey, demarkation, and settlement. (*d*) The adoption of sound methods of forestry to secure a steady yield, improvement of the forest, and most profitable management. (*e*) The utilization of colonial woods for railway sleepers. (3) Plantations of conifers and hard woods, designed to supply the future requirements of the country, can be made profitably along railway lines in the upland and the midland districts. (4) The most urgent work of a Forest Department in Natal would be to save what is left of the native forests, and plantation work should be deferred till it can be undertaken without detriment to the progress of survey and demarkation.

IN the new number of the *Zoologist* there are some interesting notes, by Mr. R. J. Ussher, on crossbills in the county of Waterford. This spring he has had exceptionally good opportunities of observing the breeding habits of these birds, as four of their nests were found in his neighbourhood, three of them being within fifteen hundred yards of his house. Of the four male birds, three were red, or red interspersed with brown. One had yellow plumage, similar to that of a specimen which Mr. Ussher presented last year to the British Museum. This bird had all the appearance of having arrived at full maturity, being large, active, vigilant, and with mandibles conspicuously crossed. When Mr. Ussher climbed to the nest, both male and female perched within four feet of him, "calling excitedly." "On April 17," he says, "these crossbills were seen to carry bits of something in their mouths to the nest, as if to feed their young. The nature of the food has not been ascertained, but is suspected to be largely composed of the green opening buds of the larch, on which I have repeatedly seen the male feeding—*e.g.*, on April 4." Mr. Ussher thinks that crossbills are on the increase in Ireland at present.

IN the Journal of the Bombay Natural History Society (vol. iv. No. 3) Mr. E. Giles records a curious fact which ought to have some interest for entomologists. In June 1888 he was standing one morning in the porch of his house, when his attention was attracted by a large dragon-fly of a metallic blue colour, about 2½ inches long, and with an extremely neat figure, who was cruising backwards and forwards in the porch in an earnest manner that seemed to show he had some special object in view. Suddenly he alighted at the entrance of a small hole in the gravel, and began to dig vigorously, sending the dust in small showers behind him. "I watched him," says Mr. Giles, "with great attention, and, after the lapse of about half a minute, when the dragon-fly was head and shoulders down the hole, a large and very fat cricket emerged like a bolted rabbit, and sprang several feet into the air. Then ensued a brisk contest of bounds and darts, the cricket springing from side to side and up and down, and the dragon-fly darting at him the moment he alighted. It was long odds on the dragon-fly, for the cricket was too fat to last, and his springs became slower and lower, till at last his enemy succeeded in pinning him by the neck. The dragon-fly appeared to bite the cricket, who, after a struggle or two, turned over on his back and lay motionless, either dead, or temporarily senseless. The dragon-fly then, without any hesitation, seized him by the hind legs, dragged him rapidly to the hole out of which he had dug him, entered himself, and pulled the cricket in after him, and then, emerging, scratched some sand over the hole and flew away. Time for the whole transaction, say, three minutes."

A CATALOGUE of the Birds in the Provincial Museum, N.W.P. and Oudh, Lucknow, has been printed by order of the Museum Committee. Like the previous catalogue, it records the purely Indian birds in the Museum, now 783 in number, represented by 5360 specimens. Mr. George Reid, who is in honorary charge of the natural history department of the Museum, says no pains have been spared to make the work both accurate and complete. "It contains, he believes, in a convenient form, all the information requisite to enable workers at a distance to avail themselves, if necessary, of the contents of the Museum; while it places in the hands of all an absolutely trustworthy record of localities for a considerable number of species, and so contributes to an accurate knowledge of their geographical distribution, which, after all, is, or ought to be, the primary object of all local catalogues."

Mr. L. Fletcher, F.R.S., contributes to the current number of the *Mineralogical Magazine* a valuable paper on the meteoric iron of Tucson. The other contents of the number, in addition to abstracts and a review, are:—The hemimorphism of stephanite: the crystalline form of kaolinite, by H. A. Miers; on zinc oxide from a blast-furnace, by J. Tudor Cundall; on zinc sulphide replacing stibnite and orpiment—analyses of stephanite and polybasite, by G. T. Prior; index to mineralogical and petrographical papers, by H. A. Miers.

The Marine Biological Association of the United Kingdom has issued the third number of its Journal, and we need scarcely say that the papers present a record of much valuable work. The following are the contents:—The Director's Report, No. 3; the sense-organs and perceptions of fishes, with remarks on the supply of bait, by W. Bateson (with plate); notes on oyster culture, by Dr. G. Herbert Fowler (with plate); the generative organs of the oyster, by Dr. P. P. C. Hoek—abstract by G. C. Bourne (with plates); letter on oyster culture, by Lord Montagu of Beaulieu; flora of Plymouth Sound and adjacent waters (preliminary paper), by T. Johnson (with a woodcut); report of a trawling cruise in H.M.S. *Research* off the south-west coast of Ireland, by Gilbert C. Bourne; notes on the Echinoderms collected by Mr. Bourne in deep water off the south-west coast of Ireland in H.M.S. *Research*, by Prof. F. Jeffrey Bell; anchovies in the English Channel, by J. T. Cunningham (with an illustration in the text); notes and memoranda (with plate); and price list of specimens. In his Report, Mr. G. C. Bourne mentions that Dr. Dohrn, the founder and Director of the Naples Zoological Station, writing to Prof. Ray Lankester about the choice of a site for the laboratory of the Marine Biological Association, said that the source from which the sea-water was derived was not of so much importance as the size of the storage reservoirs, for no water that could be drawn from the sea would be as suitable for hatching and rearing delicate marine organisms as that which had been for some time in the reservoirs. "Our experience," says Mr. Bourne, "proves the wisdom of Dr. Dohrn's advice."

Messrs. Friedländer and Son, Berlin, have issued two numbers of *Abhandlungen und Berichte* of the Zoological and Anthropological Museum of Dresden. The first number includes an elaborate report, for the year 1887, of the ornithological stations in the Kingdom of Saxony, by A. B. Meyer and F. Helm; a paper on *Sus celebensis*, by A. Nehring; Lung Ch'üan-Yao, or old Celadon porcelain, by A. B. Meyer; Coleoptera collected in the years 1868–77 during a journey in South America by A. Stübel, arranged by T. Kirsch; and an obituary notice of T. Kirsch, by A. B. Meyer. The second number consists of a monograph, by Dr. K. M. Heller, on "Der Urbüffel von Celébes: *Anoa depressicornis* (H. Smith)." Both numbers are admirably printed and illustrated.

Messrs. William Wesley and Son have issued No. 100 of their "Natural History and Scientific Book Circular." It contains a list of works relating to entomology and botany.

Messrs. Joseph Torrey, Jun., and Edwin H. Barbour, in a letter dated Iowa College, Grinnell, May 9, have sent to *Science* an account of a remarkable meteor, or meteoric shower, which passed over the State of Iowa on Friday, May 2, at 5.40 p.m. In spite of the brightness of the sun, shining at the time in a nearly cloudless sky, the light of the meteor was very noticeable. Its great size, powerful illumination, discharge of sparks, comet-like tail 3° to 5° in length, and the great train of smoke which marked its course for fully ten minutes after its passage, made a strong and lasting impression on the minds of all who saw it. Unfortunately the clamour over an exciting game of ball prevented the many members of the college who saw it from making as careful observations as they would otherwise have done; so it was impossible to tell whether its passage was accompanied by sound or not, but farmers in the neighbourhood report a faint hissing noise. It appeared to enter the atmosphere about 20° to 30° south of the zenith, and descending at an angle of about 50° to 60°, passed below the horizon north-north-west of Grinnell. By telegraphing, one small meteorite, weighing one-fifth of a pound, and several fragments from a 70-pound one, were secured, and analyses and microscopic sections at once made. They contain a large amount of metal for the "stone" class of meteorites. The following is the analysis of the matrix of the 70-pound meteorite: silica, 47·03; iron oxide, 29·43; oxide aluminium, 2·94; lime, 17·58; magnesia, 2·96; total, 99·94.

Mr. George F. Kunz, writing to *Science* from New York on May 8 about the same meteor, says it was seen over a good part of the State of Iowa at 5.15 p.m., standard western time. According to his account, the passage of the meteor was accompanied by a noise like that of heavy cannonading or thunder; and many people rushed to the doors, thinking it was the rumbling of an earthquake. The meteor exploded, he says, about eleven miles north of Forest City, Winnebago County, in the centre of the northern part of Iowa, lat. 43° 15′, long. 93° 45′ west of Greenwich, near the Minnesota State line. The fragments were scattered over a considerable surface of ground, and a part of the main mass was believed to have passed down into Minnesota. Up to the time at which Mr. Kunz wrote his letter, there had been found masses of 104 pounds, 70 pounds, and 10 pounds, and a number of fragments weighing from one to twenty ounces each. The pieces are all angular, with rounded edges. Mr. Kunz says the meteor is apparently of the type of the Parnallite group of Meunier, which fell February 28, 1857, at Parnallee, India. "The stone is porous, and when it is placed in water to ascertain its specific gravity, there is a considerable ebullition of air. The specific gravity, on a fifteen-gramme piece, was found to be 3·638. The crust is rather thin, opaque black, not shining, and, under the microscope, is very scorious, resembling the Knyahinya (Hungary) and the West Liberty (Iowa) meteoric stones. A broken surface shows the interior colour to be gray, spotted with brown, black, and white; the latter showing the existence of small specks of meteoric iron from one-tenth to four-tenths of a millimetre across. Troilite is also present in small rounded masses of about the same size. On one broken surface was a very thin seam of a soft black substance, evidently graphite (?), and soft enough to mark white paper; a felspar (anorthite ?) was also observed, and enstatite was also present." Mr. Kunz points out that this is the fourth meteorite that has been seen to fall in Iowa. The other three falls were as follows: at Hartford, Linn County, February 25, 1847; at West Liberty, Iowa County, February 12, 1875; and the great fall of siderolites at Estherville, Emmet County, May 10, 1879, which fall comprised over two thousand pieces weighing from a tenth of an ounce to 400 pounds.

A valuable contribution to the study of the natural causes which check the tendency of plants and animals to increase in too great numbers appears in a recent issue of the *Bulletin* of the Moscow Naturalists (1889, No. 3). It is by Mr. Alexander Becker, whose ideas on the subject are based upon direct observation. For several years, various species of grasshoppers appeared in great quantities in South-East Russia (about Sarepta), but then came one year of sudden death for most of them: they were seen sitting motionless on the grasses, and dying. A few years ago the butterfly, *Melithæa Phœbe*, var. *ætherea*, appeared in immense numbers, and it was expected that in the following year it would be still more numerous, but in reality it became exceedingly rare. The like was true of *Zegris eupheme*, which

suddenly became most numerous in 1883, and disappeared in 1884; even in 1883 its caterpillars could hardly be found on the plants they usually feed upon. Some hostile influence had prevented its further multiplication. Similar facts have been observed in the case of Mammalia as well. The *Spermophilus citillus* is usually met with about Sarepta; but sixty years ago it suddenly disappeared in the course of one summer—probably succumbing to some contagious disease. During the following years it could hardly be found, but by and by it multiplied again to such an extent that each inhabitant of Sarepta had to undertake to kill every year a certain number of the *Spermophilus.* Their numbers were diminished, but still they are very numerous in the steppes, thus illustrating the small importance of even a systematic attempt at extermination, as compared with the importance of natural checks. Many birds suddenly appear in great numbers, and as suddenly disappear. The *Merula rosea* for several years nested in very great numbers at Sarepta. Mr. Becker also mentions interesting facts as to a yellow dust which spread over Saratoff in April 1864, and must have been brought from Central Asia. Under microscopical examination, it was found to contain a great number of germs and Infusoria. The seeds of *Typha stenophylla,* which formerly was never found at Sarepta, must have been imported quite recently by wind, either from Caucasia or Siberia. In the course of one summer this pretty plant became numerous in a pond in the Ergheni Hills, but it disappeared next year. As a rule, a decrease of all kinds of insects is noticed about Sarepta, and it can be explained only by the general conditions of weather resulting in indifferent crops, and a general diminution of hay-crops in the surrounding steppes.

A COMPREHENSIVE paper upon the simpler derivatives of hydroxylamine, NH_2OH, by Drs. Behrend and Leuchs, will be found in the current number of *Liebig's Annalen* (p. 203). The great interest with which hydroxylamine derivatives have recently been invested by the discovery among them of geometrical isomers, and the considerable importance of hydroxylamine as a reagent for investigating the constitution of organic compounds, rendered it very desirable that something more definite should be ascertained regarding the compounds obtained by replacing the hydrogen of hydroxylamine by simple organic radicles, than the few isolated facts hitherto acquired. There are five types of derivatives possible, which are classified as follows, R representing a monad radicle: $H_2N—OR$ α-monalkyl, $RHN—OH$ β-monalkyl, $RHN—OR$ α-dialkyl, $R_2N—OH$ β-dialkyl, and $R_2N—OR$ trialkyl hydroxylamine. In the case of the radicle benzyl, $C_6H_5—CH_2—$, a complete series of such compounds have been prepared and fully investigated. The first member of the series, α-benzylhydroxylamine, $H_2N—OC_7H_7$, was prepared some time ago by Janny, and Drs. Behrend and Leuchs utilized Janny's reaction, improved very considerably in its details, in order to prepare this substance in quantity. The reaction consists in warming hydrochloric acid with the benzyl derivative of the well-known acetone compound of hydroxylamine, acetoxin, $(CH_3)_2C{=}NOH$. Its course may be represented by the equation $(CH_3)_2C{=}NOC_7H_7 + HCl + H_2O = H_2N—OC_7H_7 . HCl + (CH_3)_2CO$. The hydrochloride thus obtained crystallizes in large, flexible, lustrous plates, which sublime at 230°–260° C. without fusion. The free base itself, α-benzylhydroxylamine, $H_2N—OC_7H_7$, is a liquid which cannot be distilled at the ordinary pressure without decomposition, but at a pressure of 30 mm. distils unchanged at 118°–119°. It may also be safely distilled in steam. α-dibenzylhydroxylamine, $H(C_7H_7)N—O(C_7H_7)$, is readily obtained from the mono-compound just described by the limited action of benzyl chloride. It is also liquid at the ordinary temperature, and more difficultly volatilizable even in steam than the mono-compound. Its hydrochloride crystallizes well from alcohol in glittering needles. The di-compound is readily transformed by further action of benzylchloride into tribenzylhydroxylamine, $(C_7H_7)_2N—OC_7H_7$. The tri-compound is likewise a liquid, and is not volatile without decomposition even *in vacuo.* Its hydrochloride is readily and completely decomposed by water. The β-mono-compound, $(C_7H_7)HN—OH$, is obtained by boiling the α-di-compound with concentrated hydrochloric acid, or heating the two together in a sealed tube to 130°. $(C_7H_7)HN—O(C_7H_7) . HCl + HCl = C_7H_7HN—OH . HCl + C_7H_7Cl$. The free base, which is liberated as an oil upon addition of sodium carbonate solution to the concentrated solution of the hydrochloride, crystallizes on standing, and gives well-developed crystals on recrystallization from petroleum-ether. It also reduces Fehling's solution, in these two points differing markedly from the liquid α-mono-compound. The β-di-compound is best obtained by boiling hydroxylamine hydrochloride, benzyl chloride, and soda crystals with alcohol for an hour, using a reflux condenser. On cooling, crystals of the solid base are deposited, which melt at 123°. The preparation of this complete series shows in a very striking manner the different effects of substituting alkyl radicles for the hydrogen attached to nitrogen or for the hydroxylic hydrogen. The α-compounds are both liquids, while the β-derivatives are solids. The alkyl radicle replacing hydroxylic hydrogen is also very much more easily detached by the action of hydrochloric acid than that attached to nitrogen. It is also interesting to note that the basic character of hydroxylamine diminishes with the number of alkyl radicle groups introduced.

THE additions to the Zoological Society's Gardens during the past week include a Masked Parrakeet (*Pyrrhulopsis personata*) from the Fiji Islands, presented by Mr. Geo. Lawson; a Lanner Falcon (*Falco lanarius*), European, presented by Miss Marjorie Barnard; three Common Vipers (*Vipera berus*), British, presented by Mr. A. W. Cotton; two Andaman Starlings (*Sturnia andamanensis*) from the Andaman Islands, three Ceylon Fish-Owls (*Ketupa ceylonensis*) from Ceylon, six Tufted Ducks (*Fuligula cristata* 3 ♂ 3 ♀), European, purchased; a Great Bustard (*Otis tarda* ♀), European, received in exchange; two Japanese Deer (*Cervus sika* ♂ ♂), two Barbary Wild Sheep (*Ovis tragelaphus* ♂ ♀), a Burrhel Wild Sheep (*Ovis burrhel* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on June 5 = 14h. 57m. 6s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4045	—	—	15 0 54	+ 2 2
(2) G.C. 4058	—	—	15 3 24	+56 11
(3) 342 Birm.	4·5	Reddish-yellow.	14 55 50	+66 22
(4) τ_1 Serpentis	5	Reddish-yellow.	15 20 41	+15 49
(5) μ Virginis	4	Whitish-yellow.	14 37 18	− 4 49
(6) R Ursæ Majoris ...	Var.	Reddish.	10 36 51	+69 21

Remarks.

(1) The spectrum of this nebula has not been recorded. In the General Catalogue it is described as: "Very bright; pretty large; round; pretty suddenly much brighter in the middle to a nucleus; following of two." The companion is apparently very faint.

(2) This is a bright oval nebula under the body of Draco. Smyth states that it is "rather faint at the edges, but not so as to obscure the form." The General Catalogue description is "Very bright; considerably large; pretty much extended in the direction 146°; gradually brighter in the middle." In 1848

Lord Rosse observed a longitudinal rift, but this has not been confirmed by later observations. According to Dr. Huggins, the spectrum is continuous, but it is not at all unlikely that further observations may show that it is not entirely so. The whole nebula appears to resemble that in Andromeda, even to the dark rift, and we now know that the spectrum in that case is not perfectly continuous. Intending observers of this class of nebula spectra will do well to examine the spectrum of Comet Brooks, which was referred to in last week's notes. The spectrum of the comet is apparently continuous at first sight, but careful observation shows beyond question the existence of the usual flutings of carbon. These flutings may also be expected in the nebulæ having so-called "continuous" spectra.

(3) D'Arrest likens the spectrum of this star to that of β Pegasi, but Dunér thinks it more like α Herculis. All the bands 1–9 are very strongly marked, and are wide and dark. Observations similar to those suggested for other stars of the group are required.

(4) According to Vogel, this star has a well-marked spectrum of the solar type, but Dunér classes it with stars of Group II. He states that the bands 1–8 are seen, but that they are narrow and not very dark, 4 and 5 appearing as lines. He also suggests that the spectrum is an intermediate one between Group II. and Group III. As I have pointed out on previous occasions, it is these intermediate stages which require a detailed study. It is pretty certain that the passage from one group to another will not be abrupt, but that there will be intermediate stages between each successive two. The star in question is probably slightly less advanced in condensation than Aldebaran.

(5) This is a star of Group IV. (Konkoly), but in addition to the hydrogen lines, D and *b* are distinctly visible. The usual observations are required.

(6) The range of this variable is from 6·0–8·1 at maximum to 13·2 at minimum, in a period of about 302 days. The increase of light is very rapid, whilst the decrease is slow and irregular (Sawyer). The spectrum is a fine one of Group II., the bands 1–9 being wide and dark; 7 and 8 are especially remarkable (Dunér). The usual bright lines which are now expected to appear at the maxima of stars of this class should be looked for. The maximum will occur about June 12.

A. Fowler.

Actinic Light of the Solar Corona.—Prof. Frank H. Bigelow, in *Bulletin* No. 15 of the United States Scientific Expedition to West Africa, dated April 19, 1890, gives an interesting note on the law of distribution of the actinic light of the solar corona. His paper on "The Solar Corona, discussed by Spherical Harmonics," noted in Nature, vol. xli. p. 595, assumed that the surface distribution of the electro-magnetic potential was expressed by C cos θ, the constant representing the maximum, and θ the angular distance from the coronal pole; the visible lines of the corona being shown to coincide in direction with the lines of force generated under these conditions. It is now suggested that the corresponding equipotential lines denote the position and direction of the surfaces of iso-actinism as referred to the same pole, or, in other words, that the actinic brightness is directly proportional to the potential. From the discussion it follows that the poles become the critical points for examination as to the actinic intensity of the corona; the sky in the neighbourhood should also be examined with great care; these two results, combined with the visible linear distance of the contour of merging of the polar rays in the sky light, in terms of the radius of the sun corrected from the covering lunar disk when taken in combination with the formula for these surfaces, will enable the whole of the coronal light to be discussed as simple phenomena.

On the Rotation of the Sun.—Prof. N. C. Dunér has made a series of observations of the displacement of lines in the spectrum at the eastern and western edges of the sun for the purpose of deducing the time of rotation (*Astr. Nach.*, 2968). The observations were made from 1887–89 with a Rowland grating spectroscope of high power attached to the refractor of Lund Observatory, the distances between several lines in the a group of the solar spectrum at opposite edges of the sun being micrometrically determined. The results of the measures are given in the following table, where ϕ is the heliocentric latitude of the points on the sun's edge, v the velocity in kilometres with which the point on the edge approaches the earth, ξ the angle of rotation in 24 hours, and n the number of measures made in the different years:—

ϕ. °	v.	$\xi \cos \phi$. °	ξ. °	n.
0·4	1·98	14·14	14·14	107
15·0	1·85	13·19	13·66	104
30·0	1·58	11·31	13·06	104
45·0	1·19	8·48	11·99	106
60·0	0·74	5·31	10·62	107
74·8	0·54	2·45	9·34	107

These values of ξ, deduced from spectroscopic observations, show that the equatorial zone of the solar surface has a shorter time of rotation than zones in higher latitudes, the results agreeing with those found from sun-spot observations. The advantage of the method used by Prof. Dunér, however, lies in the fact that it allows observations of rotation to be made in the neighbourhood of the poles. A comparison of the spectroscopic and the spot observations shows that the former gives a slightly smaller velocity of rotation than the latter.

It may be remembered that the work done under the direction of Prof. Rowland at Baltimore showed that "the absorbing layer of gases by which the Fraunhofer lines are formed does not behave like the sun-spots, but is slightly retarded at the sun's equator."

Pulkova Observatory.—The magnificent volume issued in commemoration of the jubilee of the Pulkova Observatory has been received. In the volume an account is given of the 30-inch refractor and the Astro-physical Laboratory, and the twelve plates which illustrate it are worthy representations of an enviable reality. It is hardly necessary to say that the history of the Observatory is fully delineated, and that technical descriptions of the instruments are given, whilst Hermann Struve gives a long account of the determination of the instrumental constants. A *résumé* is also given of the work done in the Astro-physical Laboratory, and a comprehensive bibliography of the various astronomical, geodetical, and other studies that have been completed. Indeed, the whole of the splendidly finished work is a fitting memento of the jubilee that it celebrates.

Telluric Lines of the Solar Spectrum.—M. J. Janssen presented a note to the Paris Academy, on May 27, relative to some results he has obtained during a stay in Algeria, where he has been for about four months investigating the action of the atmosphere on the solar spectrum by means of photographs taken when the sun is on the meridian and horizon. The photographs were taken with the aid of a Rowland's grating, and isochromatic plates were used in order to obtain records of the less refrangible portions of the spectrum. The work is not yet finished, but M. Janssen notes that, without the purity of the heavens in Algeria and the continuance of favourable days, it would have been impossible to obtain any results.

Brooks's Comet (*a* 1890).—The following ephemeris computed by Dr. Bidschof is given in *Astronomische Nachrichten*, No. 2969:—

1890.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
June 4	18 56 29	+60 46·8	0·2816	0·1958	3·57
5	48 0	61 28·7			
6	39 9	62 7·9			
7	29 54	62 44·3			
8	20 17	63 17·8	0·2820	0·1978	3·53
9	10 20	63 48·1			
10	0 3	64 15·1			
11	17 49 30	64 38·7			
12	38 44	64 58·6	0·2827	0·2027	3·44
13	27 48	65 14·8			
14	16 44	65 27·5			
15	5 37	65 36·6			
16	16 54 31	65 42·1	0·2837	0·2102	3·31
17	43 29	65 44·0			
18	32 35	65 42·6			
19	21 53	65 37·7			
20	11 25	65 29·5	0·2849	0·2200	3·15

The brightness at discovery has been taken as unity.

NEWTON'S INFLUENCE ON MODERN GEOMETRY.

In the appendix to his "Arithmetica Universalis" Newton states that a study of the ancient philosophers had led him to the inevitable conclusion that those early pioneers of science had introduced geometry in order to escape from needlessly long and laborious calculations. So, too, the author of the "Principia" had a predilection for graphic as distinguished from analytic methods. Indeed, anyone who has perused that great work will readily endorse the truth of this assertion. Yet Newton was born some forty years after the death of Viète and only eight before that of René Descartes, whose writings gave such a wondrous impulse to analytical studies.

During the closing period of the seventeenth, and nearly the whole of the eighteenth century, analysis reigned supreme; whilst graphic methods languished from the wilful neglect, nay even undisguised contempt, of the new philosophers. But at length men grew weary of abstract thought, and, as was quite natural after an undue pursuit of one branch of science to the exclusion of all others, a strong reactionary current in favour of concrete geometrical studies supervened. Then, as now, the question of the respective merits of the two methods gave rise to serious, not to say heated, controversy. But why sane people should quarrel and then fall out over a purely mathematical difference of this sort is quite as incomprehensible to a sober-minded critic as the passionate resistance shown to the postal reforms of Sir Rowland Hill was to the placid and imperturbable mind of Lord Melbourne.

The general weariness of the scientific mind, brought on by an excess of analytical work, prepared the way for a great revival of the graphic *culte*. Carnot, following to a certain extent the previous example of Simpson, courageously resolved to continue the work of Pascal, Newton, and Desargues. In consideration of his treatises on projective geometry and the theory of transversals, Carnot has a definite claim to be deemed the leader of this modern insurrection against the excessive use of analysis. Contemporary with him we find Monge, one of whose pupils, Poncelet, may be justly termed the author *par excellence* of modern methods. Since Poncelet's time the further

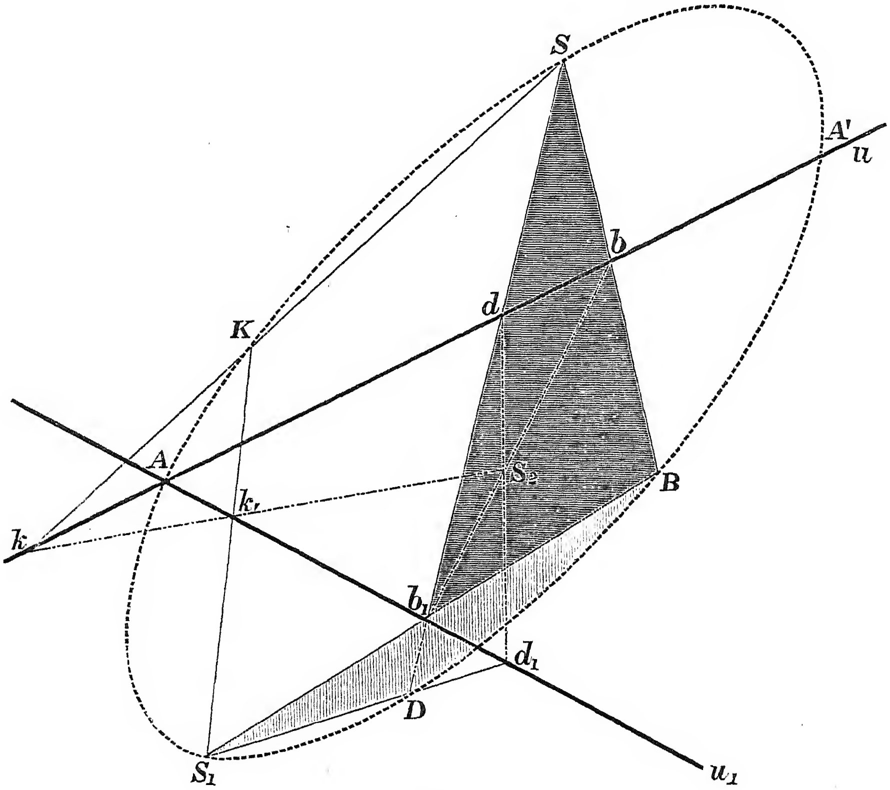


Fig. 1.

development of the system has been confined for the most part to Germany and Switzerland, under the guidance of such leaders as Steiner and Staudt. But, unfortunately, Staudt undertook the arduous, if not impossible, task of expounding projective geometry without the aid of diagrams, in regard to which Hankel well remarks, "that such an attempt was possible only in Germany, the land of scholastic methods and scientific pedantry."

Strange to say, Culmann, who was nothing if not a practical man of science, presupposes a knowledge of Staudt's geometry in all who would rightly understand his own epoch-making work on graphic statics.[1] Luckily, however, it is possible to understand every line of Culmann without having read a single word of Staudt. Now it is precisely the object of this paper to show that, in some of its more salient features, this so-called *Geometrie der Lage* is but a luxuriant offshoot of Newton's "Principia," in illustration of which we will here proceed to prove how the general method of constructing a conic, five points on which are given, may be deduced from the similar proposition in Newton. Further, in order to make the connection between Newton and Staudt more apparent, it will be advisable first to give the modern solution of the problem, and then show how the same solution can be geometrically deduced from Newton's principle.

Solution.—Take any two of the given five points, for instance S and S_1 (Fig. 1), as centres of projection. Through a third point, A, draw any two lines u and u_1. Then, from the centre S_1, project the remaining two points B and D by rays S_1B and S_1D intersecting line u_1 in the points b_1 and d_1.

Similarly, from the second centre S, project the same two points B and D by rays intersecting the line u in b and d. Join bb_1 and dd_1, meeting in the centre of perspectivity, S_2, of the lines u and u_1.

Then, to find a sixth point on the curve, draw any ray through

[1] Published in the year 1864, not, as was recently stated in a contemporary, in 1866. The date is of importance when discussing priority of discovery in the matter of reciprocal figures; for Maxwell's paper on the subject in the *Philosophical Magazine* was also published in 1864. The question cannot, however, be discussed in a footnote.

S_2, intersecting u and u_1 in k and k_1; and project k from S and k_1 from S_1 by rays meeting in K, a point on the required curve.

PROOF.—Newton has solved this problem for a particular case, of which we will now give a short account, and thence deduce the more general modern method just described.

CASE I.—Let ABCD (Fig. 2) be a quadrangle inscribed in an ellipse, and P a point on the ellipse outside of the quadrangle; then, if PS and PQ be two chords meeting the sides of the quadrangle in S and T, R and Q respectively, the ratio

$$\frac{PR \cdot PQ}{PS \cdot PT}$$

will be constant for all positions of P. For, in the first instance, let PR and PQ be parallel to the side AC, PS and PT parallel

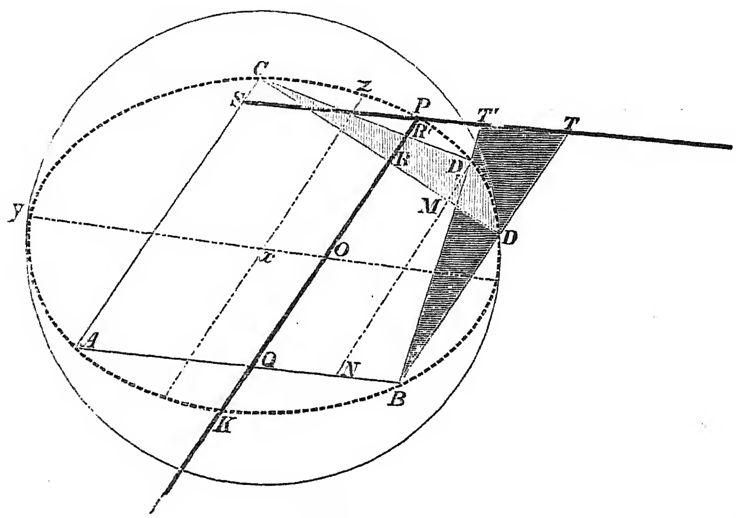


FIG. 2.

to the adjacent side AB, whilst the opposite sides AC and BD are parallel to each other. Owing to the parallelism of the sides AC and BD, a line bisecting those sides will be a diameter of the ellipse, bisecting the line RQ in O. The line PO will be the ordinate of P parallel to the axis conjugate of this diameter.

Now produce PO to K, making OK equal to OP; then K will be a point on the ellipse, wherefore—

$$\frac{PQ \cdot QK}{AQ \cdot QB} = \left(\frac{xz}{xy}\right)^2, \text{ a constant ratio.}$$

But

$$PR = QK,\ PS = AQ,\ PT = QB;$$

therefore—

$$\frac{PQ \cdot PR}{PS \cdot PT} = \frac{PQ \cdot QK}{AQ \cdot QB}, \text{ a constant.}$$

CASE II.—When BD_1 and AC are not parallel, draw BD parallel to AC, meeting the conic in D and the line PST in T. Join CD, intersecting PQ in R and D_1N, a line parallel to PQ, in M. Then, by similar triangles—

$$\frac{BT \text{ or } PQ}{TT'} = \frac{D_1N}{BN}; \quad \ldots \ldots \ldots (1)$$

also—

$$\frac{R'R}{AQ \text{ or } PS} = \frac{D_1M}{AN}; \quad \ldots \ldots \ldots (2)$$

whence, by multiplication—

$$\frac{PQ \cdot RR'}{PS \cdot TT'} = \frac{D_1N \cdot D_1M}{AN \cdot BN}. \quad \ldots \ldots (3)$$

But, by Case I., since D_1 is a point on the ellipse and similarly situated with respect to M and N as P is with respect to R and Q, we have—

$$\frac{PQ \cdot PR}{PS \cdot PT} = \frac{D_1N \cdot D_1M}{AN \cdot BN}. \quad \ldots \ldots (4)$$

Hence, from equations (3) and (4)—

$$\frac{PQ \cdot RR'}{PS \cdot TT'} = \frac{PQ \cdot PR}{PS \cdot PT};$$

or, by subtraction—

$$\frac{PQ \cdot RR'}{PS \cdot TT'} = \frac{PQ}{PS}\left(\frac{PR - RR'}{PT - TT'}\right)$$

$$= \frac{PQ \cdot PR'}{PS \cdot PT'};$$

wherefore—

$$\frac{PR'}{PR} = \frac{PT'}{PT}. \quad \ldots \ldots \ldots \ldots (5)$$

Thus, the lines R'T' and RT are parallel; so that, in order to construct the ellipse, it is necessary to divide the two lines PT and PQ by a series of parallel lines, meeting them in points T and R, which, being projected from the centres B and C respectively, will determine, by means of the points of intersection of corresponding rays, any number of points on the required conic.

Such is Newton's method of constructing a conic five points on which are given. We will now proceed to prove the intimate connection existing between it and the more modern method illustrated in Fig. 1, the discovery of which has been sometimes attributed to Pascal.

It will be observed that when, as in Fig. 2, the lines PT and PR are drawn parallel to the adjacent sides AB and AC, the rays R'T' and RT are parallel (5); and that, therefore, the centre of perspectivity of the punctuated lines PT and PR lies at an infinite distance. When, however, the lines PT and PR are shifted into the positions PT' and PR' (Fig. 3), being then no longer parallel to the adjacent sides branching from A, the points DEO on the ellipse are projected from B upon line PT' in $d_1e_1o_1$, and from centre C upon the line PR' in $d_1'e_1'o_1'$. Now, in order to demonstrate the method given at the beginning of this paper, it is necessary and sufficient to show that the lines joining d_1 and d_1', e_1 and e_1', o_1 and o_1', all meet in one and the same point, S; or, in other words, that the punctuated lines PT' and PR' are in perspective.

Thus, if the transversal o_1d_0 be drawn parallel to PT, we have—

$$\frac{o_1e_0}{oe} = \frac{d_0B}{dB}; \quad \frac{o_1e_1}{o_1e_0} = \frac{e_1P}{eP};$$

and, if the transversal $o_1\delta$ be drawn parallel to PR', we have—

$$\frac{o_1\epsilon}{o_1e_1} = \frac{e_1'P}{e_1P};$$

wherefore, multiplying together the right and left hand members of the last three equations, we obtain—

$$\frac{o_1\epsilon}{oe} = \frac{d_0B}{dB} \cdot \frac{e_1'P}{eP} \quad \ldots \ldots (6)$$

Again, if $o_1'l$ be drawn parallel to AC or PR, we have—

$$\frac{o_1'e_1'}{o_1'\epsilon'} = \frac{e_1'P}{e'P}; \quad \frac{o_1'\epsilon'}{o'e'} = \frac{sl}{sP};$$

$$\therefore \frac{o_1'e_1'}{o'e'} = \frac{e_1'P}{e'P} \cdot \frac{sl}{sP} \quad \ldots \ldots (7)$$

Let

$$\frac{o'e'}{oe} = \frac{o'd'}{od} = n, \text{ a constant};$$

then, by eq. (7),

$$\frac{o_1'e_1'}{oe} = n\frac{e_1'P}{e'P} \cdot \frac{sl}{sP}; \quad \ldots \ldots (8)$$

and, from equations (6) and (8),

$$\frac{o_1\epsilon}{o_1'e_1'} = \frac{1}{n}\frac{d_0B}{dB} \cdot \frac{e'P}{eP} \cdot \frac{sP}{sl}$$

$$= \frac{d_0B}{dB} \cdot \frac{sP}{sl} = k.$$

By a similar process of reasoning it can be shown that

$$\frac{o_1\delta}{o_1'd_1'} = k,$$

wherefore—

$$\frac{o_1\epsilon}{o_1'e_1'} = \frac{o_1\delta}{o_1'd_1'};$$

so that the lines e_1e_1' and d_1d_1' must meet in the same point on line o_1o_1', which point of convergence is therefore the centre of perspective, S.

The perspectivity of the punctuated lines PT′ and PR′ may be deduced from the projective relations of Fig. 3 by means of the following two well-known theorems of projective geometry.

THEOREM I.—If the correlative or coharmonic points a and a_1 of the punctuated lines u and u_1 (Fig. 4) coincide with their common point of intersection, forming what may be termed a coharmonic point, the lines u and u_1 are in perspective. For, let bb_1 and cc_1 be any other two pairs of coharmonic points, and meet in the centre S; then S will be the centre of perspectivity of the two lines u and u_1; seeing that in a harmonic or other system of ratios, any three members of a compound proportion

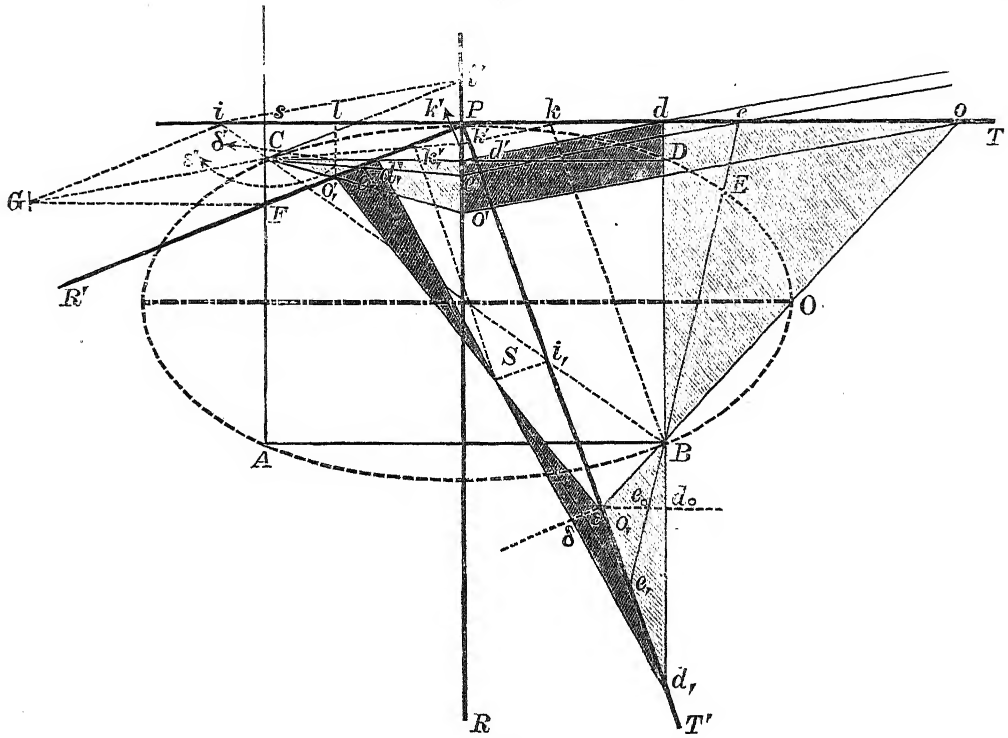


FIG. 3.

consisting of four terms suffice to determine the fourth or unknown term. Then, this fourth point, being known and taken in conjunction with any pair of the other three known terms, will serve to determine a fifth; and so on *ad infinitum*.

THEOREM II.—Similarly, if in Fig. 5 any pair of coharmonic rays Sa and S_1a_1 of two different pencils lying in the same plane are coincident or coperspective, the two pencils will be perspective of the same line, and consequently perspective of each other. For, let the coharmonic rays Sb and S_1b_1 meet in a point B, and the rays Sc and S_1c_1 in C; then the line BC will be perspective of each pencil; seeing that, if three coharmonic rays of the pencils meet upon the line BC, it necessarily follows that a fourth pair of such rays will meet upon the same line.

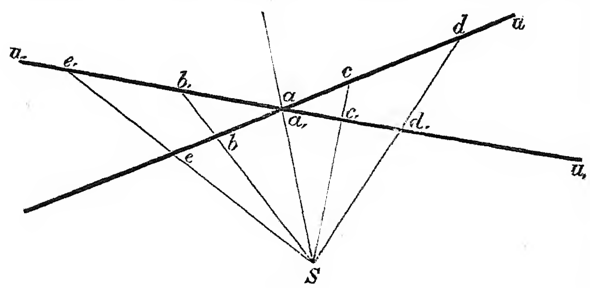


FIG. 4.

Now it will be observed that lines PT and PR (Fig. 3) are in perspective, being projected from infinity on the right of the figure. So also are lines PT and PT′ as projected from centre B, and lines PR and PR′ as projected from centre C. Further, the point P is a coharmonic point common to lines

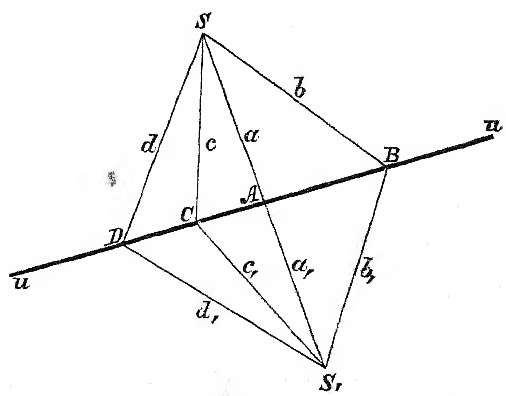


FIG. 5.

PT′ and PR′ as projected from centres B and C respectively; for it will be seen that P does not change its position when projected through centre B from line PT to line PT′, nor does it change when projected, first from infinity parallel to dd', ee', &c.,

from line PT to line PR, and thence through centre C upon line PR′. Moreover, the point P is the point of intersection of lines PT′ and PR′; wherefore, by Theorem I., the lines PT′ and PR′ are in perspective.

In order to find the centre of perspectivity of lines PT′ and PR′, we have the point k on line PT correlative of the point at infinity on PT′, k being determined by drawing Bk parallel to PT′. The point k_1' on PR′, correlative of k on PT, is found as before by projecting k first upon PR in k' and thence upon PR′ in k_1'. Thus, the required centre of perspectivity must lie somewhere on the indefinite line joining k_1' with the point at infinity upon PT′. Again, the point i' on PR corresponds to the point at infinity on PR′, to point i on PT, and to point i_1 on PT′; hence the sought centre mu-t lie somewhere on the indefinite line joining i_1 to the point at infinity on PR′; wherefore it coincides with the intersection of lines i_1S and k_1'S.

Similarly it can be shown that the lines PT and PR′ are in perspective; for the line drawn from C, the centre of projection for line PR′, to S_∞, the centre of projection for line PT, or, in other words, the line drawn from C parallel to dd', ee', &c., is a coharmonic ray common to both lines; therefore, according to Theorem II., the lines PT and PR′ are in perspective. The corresponding centre of perspectivity is determined as follows. When the line oo', moving parallel to itself, passes to infinity, or, in other terms, when the points o and o' pass to infinity on lines PT and PR, the ray Co' takes up the position CF, parallel to PR. Hence, F is the point on line PR′ corresponding to infinity on PT; wherefore the required centre must lie somewhere on the line through F parallel to PT. But i is the point on PT corresponding to infinity on PR′; therefore the centre must lie somewhere on the line through i parallel to PR′. Hence we conclude that it must coincide with the point of intersection of lines FG and iG.

In this short paper we have made an honest attempt to trace one filament of the great stream of modern science to its original source. The space at our service may not admit of much more. Still, such a study, however limited its scope may be, is interesting, not only on account of the novel nature of the demonstrations which the proof of connection involves, but even more because of the reflex light thus cast upon recent invention.

ROBERT H. GRAHAM.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Observatory Syndicate Report as follows:—

That they have considered the proposal made in the Report of the Newall Telescope Syndicate for the purchase of an acre or an acre and a half of land adjoining the grounds of the Observatory for the erection of the Newall telescope and its appurtenances, and they are of opinion that, in view of possible future requirements of the Observatory, it will be desirable to secure now the larger area—namely, an acre and a half.

They have consulted the Bursar of St. John's College, and have learnt that the College is willing to sell to the University an acre and a half at the price of £250. Further, Prof. Adams has offered to contribute £100 towards expenses.

Under these circumstances the Syndicate recommend that Prof. Adams's generous offer be accepted, and that the Vice-Chancellor, on behalf of the University, be empowered to enter into an agreement with St. John's College for the purchase of an acre and a half of land adjoining the grounds of the Observatory.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 1.—"Photographic Determination of the Time-relations of the Changes which take place in Muscle during the Period of so-called 'Latent Stimulation.'" By J. Burdon Sanderson, F.R.S.

It is now forty years since Helmholtz published his fundamental experiments on the time-relations of muscular contractions. The purpose of this investigation was to ascertain "the periods and stages in which the energy of muscle rises and sinks after instantaneous stimulation"; the word energy being defined as the "mechanical expression of activity"; and one of the most important conclusions of the author was that, in the muscles investigated by him, contraction does not begin until nearly one hundredth of a second after excitation. This interval has, by subsequent writers, been called the period of "latent stimulation."

Helmholtz subsequently (1854) showed, by experiments of surpassing ingenuity, that during this period an electrical change of very short duration occurs, which culminates at about one two-hundredth of a second after excitation. The fact discovered by Helmholtz was further investigated by Bernstein in 1866, with the aid of the repeating rheotome, and subsequently (1875) by du Bois-Reymond, whose statement of the actual time-relations of the electrical response to an instantaneous excitation of the gastrocnemius of the frog is embodied in a curve which denotes that the muscular surface becomes negative to the tendon about three thousandths of a second after excitation, that this effect culminates at seven thousandths of a second, and that it is immediately followed by a change of opposite sign, which culminates at about ten thousandths.

By a new method—that of photographing in succession the mechanical and electrical responses in muscle on a rapidly moving sensitive surface—the author has shown that the mechanical response occurs much earlier than has been hitherto supposed; and that it is, in fact, simultaneous with the electrical change above described—that is, with the so-called negative variation.

The method consists in projecting the movement to be recorded, whether of the muscle or that of any instrument which serves as an index of change, on a vertical slit on which the vibrations of a tuning-fork and the motion of a signal are also shadowed. Immediately behind the slit is a photographic plate, which is carried by an equilibrated pendulum. The approximately uniform rate of motion of the sensitive surface which receives the light-written record is about one metre per second, but is determined in each experiment by reference to the rate of vibration of a tuning-fork.

In the experiments on direct excitation, the muscles used were the *gastrocnemius* and *sartorius* of the frog. In the former the movement of contraction was communicated to a light index, which was supported by a fine spring. One end of the index rested on the muscle, while the other occupied the front focus of a projection apparatus, the slit being in the other focus. When the sartorius was used the surface of the muscle was itself brought for a moment into the focus, at the seat of excitation. The unavoidable exposure of the structure to the electric light, which this method involved, lasted scarcely more than a second. In successful experiments, the interval between excitation and the beginning of the contraction was $2\frac{1}{2}$ thousandths ($= \frac{1}{400}$) of a second.

For measurement of the delay in indirect excitation, the gastrocnemius (with the index) only was used, the exciting electrodes being applied either at 12 or at 37 mm. from the muscle. The results were not so constant. Corrected for loss of time by propagation along the nerve, the intervals between excitation and beginning contraction varied from 0·0025″ to 0·0035″.

In the experiments for determining the time after excitation at which the electrical response begins and culminates, the capillary electrometer was used, as in the author's experiments on the heart and on the leaf of *Dionæa*, as a signal, but with much improved apparatus for recording.

In the gastrocnemius of the frog, the electrical response to an instantaneous stimulus is indicated by a sudden movement of the mercurial column of so short a duration, that to most persons it is invisible. Its photographic expression is that of a spike projecting from the dark border of the part of the plate which is unprotected by the mercurial column. The electrical interpretation of this spike is that between the contacts two electrical changes of opposite sign and not more than one two-hundredth of a second in duration have immediately followed each other, or, more explicity, that the spot excited became, for about 0·0005″, first negative, then for a similar period positive, to the other contact.

In the muscle (the leading off contacts being on the Achilles tendon and muscular surface respectively, and the nerve excited at a distance of 12 mm.) the electrical response begins at 0·004″ and culminates at about 0·012″ after excitation. Deducting the delay due to transmission along the nerve, we have, as the time between excitation and response, 0·0035″. It is thus seen that the electrical response, instead of preceding the mechanical, is

contemporary with it. The electrical change *may* therefore so far as concerns the time at which it occurs in muscle, be immediately connected with that sudden change of the elastic properties of muscle of which the contraction is the sign.

The author exhibited at the Society photographs in proof of all the facts above stated. Further details, particularly those relating to the character of the "electrical response" to instantaneous stimulation, for which the photographic method of recording the movements of the capillary electrometer on a rapidly moving surface has afforded new facilities, will be the subject of a later communication.

Anthropological Institute, May 13.—Dr. J. G. Garson, Vice-President, in the chair.—Mr. Francis Galton exhibited a new instrument for measuring the rate of movement of the various limbs. The method adopted was explained by referring to the action of a spring measuring-tape. When the end of one of these is pulled out and then let go, it springs sharply back, the tape running cleanly through a slit. If it runs back more quickly than the hand could follow it, then, if the end of the tape be retained in the hand that gives the blow, the tape will run through the slit at the exact rate at which the blow is given. The hand need not be near the tape; it may be connected with it by a long thread, and the instrument will thus be guarded from injury. The thread, during part of its course, is arranged to travel vertically, and passes through a small inverted cone which is fixed to it; it then passes loosely through a cylindrical bead of white ivory, the lower end of which rests on the base of the cone. When the moving thread is suddenly arrested, the bead is tossed up to a height dependent on the velocity of the thread at the time and place when it was stopped. The momentary pause of the white bead when it ceases to ascend, and before it begins to descend, enables the height it has attained to be read off upon an appropriate scale, which tells at how many feet per second the thread was moving at the time it was checked.—Dr. G. W. Leitner read a paper on the ethnographical basis of language, with special reference to the customs and language of Hunza. The Hunzas are nominal Mohammedans, and they use their mosques for drinking and dancing assemblies. There is little restriction in the relation of the sexes, and the management of the State, in theory, is attributed to fairies. No war is undertaken unless the fairy gives the command by beating the sacred drum. The people are not true Mohammedans, but represent what is still left of the doctrine of the Sheik-ul-Jabl, or the Ancient of the Mountain, the head of the so-called Assassins. The language of the Hunzas is one of the most primitive, and has not yet emerged from the state in which it is impossible to have such a word as "head," as distinguished from "my head," or "thy head, or "his head;" for instance, *ak* is "my name," and *ik* is "his name." Take away the pronominal sign, and *k* alone is left, which means nothing. *Aus* is "my wife," and *gus* "thy wife." The *s* alone has no meaning, and in some cases it seemed impossible to arrive at putting anything down correctly; but so it is in the initial stage of a language. In the Hunza language that stage is important to us as members of the Aryan group, as the dissociation of the pronoun, verb, adverb, and conjunction from the act or substance only occurs when the language emerges beyond the stage when the groping, as it were, of the human child between the *meum* and *tuum*, the first and second persons, approaches the clear perception of the outer world, the *suum*, the third person.—Mr. A. P. Goodwin read some notes on the natives of the interior of New Guinea, and exhibited a fire-stick.—Mr. G. F. Lawrence exhibited two crania from the Thames.

Geological Society, May 21.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—On some Devonian and Silurian Ostracoda from North America, France, and the Bosphorus, by Prof. T. Rupert Jones, F.R.S. After the reading of this paper Dr. Hinde said he wished to express the obligations of geologists to Prof. Jones for the excellent work which he had done amongst the Entomostraca; and particularly on the present occasion, for the clear manner in which he had explained the wide distribution of some of the species. The President alluded to the long years of arduous labour which Prof. Jones had bestowed on these minute fossils, and to the interesting results he had obtained from them. —On the age, composition, and structure of the plateau-gravels of East Berkshire and West Surrey, by the Rev. Dr. A. Irving. —Further note on the existence of Triassic rocks in the English Channel off the coast of Cornwall, by R. N. Worth.—On a new species of *Coccodus* (*C. Lindströmi*, Davis), by J. W. Davis.

PARIS

Academy of Sciences, May 27.—M. Hermite in the chair. —Note on the works of M. Louis Soret, by M. A. Cornu.—On the recent work done in Algeria, by M. J. Janssen (see Our Astronomical Column).—On meteorological observations made at mountain stations in Europe and the United States, by M. H. Faye. The author discusses some observations of temperature at various altitudes during cyclones and anticyclones, and the conclusions arrived at by M. Hann at Vienna, and Prof. Hazen in the United States, with respect to the variations found.—On the Turonian flora of Martigues (Bouches-du-Rhône), by M. A. F. Marion.—On the automatic resolution and integration of equations, by M. H. Parenty. An extract of a memoir presented by the author is given.—On the nutation of the axis of the earth, by M. Folie.—On the theory of heat, by M. Appell.—On the elliptical double refraction of quartz, by M. F. Beaulard.—On the conductivities of compounds of ammonia and aniline with the oxybenzoic acids, by M. Daniel Berthelot. One circumstance worthy of attention is that, in spite of the difference of conductivities of the three oxybenzoic acids, the conductivity of the mixture of equivalent parts of each acid and ammonia is almost the same for the three isomerides as for benzoic acid. The author has previously called attention to a similar fact in the case of salts of sodium. It is also noted that the conductivities of ammonium salts are superior to those of the corresponding salts of sodium.—Experiments on magnetization by single and double touch, by M. C. Decharme.—Researches on the dispersion of organic compounds (alcohols of the fatty series), by MM. Ph. Barbier and L. Roux. The authors show—(1) In the alcohols of the fatty series that they have examined, the dispersive powers are continuous functions of the molecular weights, and, contrary to what occurs in the aromatic series, *the dispersive powers increase with increase of molecular weight*. (2) The long-chain isomeric alcohols, primary and secondary, have sensibly the same dispersive power and obey the same laws; but the primary alcohols studied, other than normal, possess less dispersive powers, without, however, departing far from the values shown by long-chain alcohols. (3) The abstraction of hydrogen is accompanied by a considerable increase in the dispersive power.—M. Ed. Grimaux discusses the formula and reactions of homofluorescein.—On the employment of artificial sea-water for the preservation of marine animals, particularly oysters, in great aquaria, by M. Edmond Perrier. The solution recommended contains 81 grams sodium chloride, 7 grams magnesium sulphate, 10 grams magnesium chloride, and 2 grams potassium chloride, dissolved in 3 or 4 litres of water.—Observations on submarine vision, made in the Mediterranean by means of a diving apparatus, by M. H. Fol.—Two new hermaphrodite *Pélécypodes*, by M. Paul Pelseneer.—On the chemical examination of mineral waters from Malaysia; the formation of tin ore, note by M. Stanislas Meunier. An incrustation from the hot spring of Azer-Panas possesses the following composition: SiO_2, 91·8; H_2O, 7·5; SnO_2, 0·5; Fe_2O_3, 0·2; and traces of alumina. This is the first instance of the present formation of a tin-ore.—Observations on the structure of some ferruginous deposits of the Secondary rocks, by M. Bourgeat.—Discovery of a Turonian flora in the neighbourhood of Martigues (Bouches-du-Rhône), by M. G. Vasseur.—On the employment of copper salts as a remedy for the potato-disease, by M. Aimé Girard. The author demonstrates that a solution of sulphate of copper used as a preventive of the disease is very efficacious, and results in a gain in the quantity of the crop such as more than pays for the expense of treatment. Even when used purely as a curative agent, the yield of healthy potatoes is increased by 20·2 to 22·9 per cent.

BERLIN.

Meteorological Society, May 5.—Prof. Schwalbe, President, in the chair.—Dr. Kiewel spoke on the diurnal periodicity of the wind with special reference to Dr. Sprung's theory of the rotation of its direction. It appeared from his investigation that in addition to the influence of the sun's radiation, the variations of barometric pressure also produce a distinct effect, as also does the difference in the rate of the wind in the upper and lower layers of the atmosphere. A discussion followed, in which Dr.

Sprung took part.—Dr. Wagner announced that arrangements had been made for endeavouring to take simultaneous photographs of flashes of lightning at widely separated stations during the approaching summer. It was hoped that by this means, if successful, it would be possible to obtain some idea of the spacial and dimensional relations of the flash.

Physical Society, May 16.—Prof. du Bois-Reymond, President, in the chair.—Dr. Köpsel exhibited and described an apparatus for the calibration of the torsion-galvanometer of Siemens and Halske. The magnet which is used in that form of the galvanometer which is employed for technical purposes, frequently changes its magnetism in presence of other powerful magnets or currents, hence the instrument requires constant calibration and adjustment. Dr. Köpsel explained his method of effecting this with the help of a Clarke element. He further described a new form of resistance to be used in the measurement of very powerful electric currents. The older form, consisting of a brass tube filled with water, in communication with a reservoir of water, had proved useless in practice. The new resistances consist of nickel wires, surrounded by an insulating layer, inserted into a tube of lead and immersed in water. These wires were not rendered incandescent by currents of 80 to 90 amperes, and have been proved to be practically useful.

Physiological Society, May 23.—Prof. du Bois-Reymond, President, in the chair.—Prof. Falk gave an account of a case of a man who was found dead, and who must have died suddenly. A *post-mortem* examination showed that all the tissues and organs were in a normal state with the exception of the pancreas, which was infiltrated with blood. This he regarded as the cause of death, although it is as yet impossible to suggest how the lesion leads to death. Rupture of a blood-vessel in the pancreas is of rare occurrence.—Dr. Heymans had recently tested Engelmann's statement that the ureters contain ganglia at their upper and lower ends, but no nerves, employing the ureters of mice. Using gold chloride he observed, with low powers of the microscope, nerve-fibres accompanying the blood-vessels which surround the ureters. After removing the peritoneum and spreading out the excised ureters, he also found fine fibres between the muscle-cells, some of which appeared, under high magnification, to be attached directly to the muscle-cells. He was not able to make out that a nerve-fibril supplies each muscle-cell.—Dr. Bruhns gave an account of his researches on adenin and hypoxanthin, with a view to determining their chemical constitution; in this he has not as yet been more than partially successful. During his researches he came across a compound of adenin and hypoxanthin, whose properties explain many opposing statements of the less recent authors. The silver salts, with picric acid of the above bases, are the ones most suited for discriminating between them. Their salts with mercury are also extremely interesting from a chemical point of view, owing to their close resemblance to the amido-compounds of mercury.—Prof. Zuntz described a modified form of intestinal fistula which he and Dr. Rosenberg had recently applied.

BRUSSELS.

Royal Academy of Sciences, April 3.—M. Stas in the chair.—The following communications were made:—Researches on the volatility of carbon compounds, by M. Louis Henry.—On monocarbon derivatives, by the same author.—Reply to a note by General Liagre relative to M. Ronkar's work "On the Mutual Impulse between the Crust and Interior of the Earth on account of Internal Friction," by M. Folie. The criticism referred to appeared in *Bulletin* No. 3 of this year, and in reply to it M. Folie adduces proofs of diurnal nutation.—On the extent of the curative action of hypnotism: hypnotism applied to alterations of the visual organ, by M. J. Delbœuf, with the collaboration of M. J. P. Null and Dr. Leplat. An extended account is given of the treatment of a patient suffering from an eye-disease which was completely cured by hypnotism.—A new Nematoid of a Galago from the coast of Guinea, by M. P. J. Van Beneden.—Note on the law existing between unit of variation of vapour tension and absolute temperature, by M. P. De Heer.—On the structure of the equatorial bands of Jupiter, by M. F. Terby.—On the thickness of the earth's crust deduced from diurnal nutation, by M. E. Ronkar. From an extended investigation it is concluded that the thickness of the earth's crust does not exceed $\frac{1}{10}$ of the radius.—On the mutual impulse between the crust and interior of the earth on account of internal friction (second note), by the same author.—Experimental methods for determining whether polarized light, of which the plane of polarization is in vibration, exercises any influence on a magnetic field, by M. H. Schoentjes.—Experiments on the absence of bacteria in the ducts of plants, by M. Emile Laurent.

STOCKHOLM.

Royal Academy of Sciences, May 14.—On the discovery of Tertiary volcanic rocks near Lake Dellen in Helsingland and Lake Mien in Smaland, Sweden, by Dr. N. O. Holst and Dr. F. N. Svenonius. Specimens exhibited and commented upon by Baron A. E. Nordenskiöld.—Report on an entomological tour in Norrland and Jemtland, chiefly for the study of the Poduridæ of these countries, by H. Schött.—Some observations on the distribution of the sexes in the galls of *Andricus ramuli*, by Prof. C. Aurivillius.—On the Graptolithidæ of the island of Gotland, by Dr. G. Holm.—On the employment of indefinite determinants within the theory of linear differential equations, by H. von Koch.—Invariant expressions for the generalized substitution of Poincaré, by F. de Brun.—On a generalization of the functions of Klein of the third family, by G. Cassel.—The form of the integrals in linear differential equations, by A. M. Johanson.—Contributions to the knowledge of the Chlorophyceæ of Sweden, by O. F. Andersson.—Helminthological researches from the west coast of Norway, Part I., Cestoda, by Dr. E. Lönnberg.—Some Muriceidæ of the genera Acanthogorgia, Paramuricea, and Echinomuricea in the Zoological Museum of Upsala, by T. Hedlund.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Synonymic Catalogue of the Recent Marine Bryozoa: E. C. Jelly (Dulau).—The Colours of Animals: E. B. Poulton (K. Paul).—Rambles and Reveries of a Naturalist: Rev. W. Spiers (C. H. Kelly).—Pond Life; Algæ and Allied Forms: T. S. Smithson (Sonnenschein).—Faune des Vertébrés de la Suisse; Vol. v., Histoire Naturelle des Poissons, 2me. Partie: Dr. V. Fatio (Genève, H. Georg).—Gesammelte Mathematische Abhandlungen, 2 vols: H. A. Schwarz (Berlin, J. Springer).—Hints on Reflecting and Refracting Telescopes, &c., 5th edition: W. H. Thornthwaite (Horne).

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THURSDAY, JUNE 12, 1890.

ELECTRIC VERSUS GAS LIGHTING.

L'Éclairage Électrique actuel dans Différents Pays. By Jules Couture. (Paris: J. Michelet, 1890.)

IN this, the second edition of this pamphlet, a comparison is made between the prices of lighting by electricity and gas at Milan, Rome, Paris, Tours, Manosque, Perpignan, Marseilles, and New York. In Milan, the electric energy is either charged for by a fixed rate per lamp per year, plus a payment for each hour during which the lamp is turned on (the fixed annual rate and the hourly payment depending on whether the lamps be of 10 or of 16 candles), or the payment may be made to depend entirely on the consumption. In the latter case, however, the hourly payment per lamp diminishes with the number of lamps employed in the building, the rate per hour varying from 6 centimes for a 10-candle lamp if there be not more than 40 lamps, to $3\frac{3}{4}$ centimes per 10-candle lamp if the number exceed 151. The incandescent lamps at Milan appear to require 6 watts per candle, and are therefore evidently specimens of the old Edison lamp; at the present day, however, there are lamps that can be incandesced with 40 per cent. less power, and still have a long life. In comparing the price of lighting by electricity and gas, M. Couture assumes that one Bengel gas-burner consuming 105 litres of gas per hour gives 10 candles. This is equivalent to 5·9 cubic feet per hour for 16 candles. Now a good Argand burner with London gas will give 16 candles for a consumption of 5 cubic feet per hour, whereas a common burner will not give more than 5 or 8 candles. M. Couture's typical burner and the Milan gas must therefore be good, whereas, as already mentioned, the incandescent lamps employed in Milan must be of an old character. Nevertheless, since electric lighting is supplied at 9 centimes per hour for a 16-candle lamp, and at 5·6 centimes (or about one halfpenny) in buildings using many lamps, the Milan Gas Company thought it wise to drop their price from 36 to 25 centimes per cubic metre of gas in the regions of the town supplied with electricity. For the benefit of readers who may study the copious information contained in this French treatise, we may mention that the London price of 2*s.* 6*d.* per 1000 cubic feet of gas is equivalent to almost exactly one penny per cubic metre.

In Rome, the Anglo-Roman Gas Company have utilized alternate-current transformers to distribute electric energy throughout the town from an electric station with 2700 horse-power, constructed on their own grounds, and have thus avoided the erection of expensive central stations in the town itself. The price charged is 8 centimes per hour for a 16-candle glow-lamp, and this M. Couture regards as a very remunerative one. It represents about $2\frac{1}{2}$ times the cost in Rome of lighting with improved 16-candle Wenham gas-burners. M. Couture speaks of the two dynamos employed there as being the largest in the world, but, as they are only of some 500 horse-power each, it is clear that the author has not heard of the dynamos at Deptford, one of which has been running for some time, and which produces 1500 horse-power. Indeed, M. Couture is wonderfully silent about the electric lighting of towns in England, and makes no reference whatever to the prices charged for electric energy at various places in this country, or to the regulation of these prices by the systematic action of the Board of Trade.

Various installations employing transformers are referred to, amongst others that at Tours, and it is mentioned that the new price charged there for electric energy is only about 30 per cent. more than for an equivalent amount of gas. The only reference to electric lighting in Great Britain is to the first use of transformers when the Metropolitan Railway stations at "Edgard Road" and "Olgate" were experimentally lighted in 1883; and of the large amount of electric lighting work that has been carried out on the Continent by English engineers nothing is said, if we except the statement that "Badkok" and Wilcox boilers are used at Marseilles and at other towns. The value of the book, which is considerable, would have been increased if some reference had been made to "Sir Siemens" and "Sir Crompton," since certain large towns abroad, not mentioned in this book, owe their electric lighting to the exertions of these firms.

Formerly, electrical companies maintained installations at a loss, for the sake of advertisement; while gas companies, to avoid losing the streets that were lighted, have continued the lighting for a return less than the cost price. The financial results of electric supply companies have often not been sufficiently prosperous to enable interest to be paid upon the capital invested, but the same may be said of many gas companies in the early days of their existence. At Dijon, a central electric station has been rendered commercially impossible by the Gas Company dropping their price from 45 to 25 centimetres per cubic metre when they heard that the erection of one was contemplated.

The Marseilles Gas Company was the first gas company to establish a central station for the purpose of ascertaining the conditions under which the new method of lighting could be advantageously used in conjunction with gas and oil. Since 1881, when the Marseilles Gas Company first took up electric lighting, they have gradually extended their operations, so that to-day the electric lighting of the town is considerable. At the rates charged, it is 20 per cent. dearer to obtain 10 candles for 1000 hours in the year by electricity than with a *good* gas-burner; but if the time of lighting be extended to 2000 hours, electric lighting is only 6 per cent. dearer than gas lighting, and in the case of a 10-candle lamp lighted for 3000 hours in the year, electricity and gas come out equal in price. Consequently, if the gas be burnt in common burners, or if governors be not employed to keep the pressure of the gas constant, lighting with gas will actually be dearer in Marseilles than by means of electric glow-lamps. However, as the author points out, it is very important to see that a 10-candle glow-lamp really gives an illumination of 10 candles, since it is by the lamp, and not by the light, that the charge is made.

Electricity is, of course, now being much used where gas was formerly employed, but an interesting example is given in this book of electricity being resorted to for the lighting of a town in which gas could not be adopted. Manosque, in the Basses Alps, was lighted, up to 1888, with oil, the municipality not being able to introduce gas,

as nearly all the houses had cellars under the public street; but for the last two years the town has been lighted by means of electricity. The price charged is either at a fixed rate per month, independently of the number of hours the lamp is lighted, or the consumer can pay for the actual time the lamps are used at the rate of $5\frac{1}{2}$ centimes per hour for a 16-candle lamp, 4 centimes for a 10-candle lamp, and $3\frac{1}{2}$ centimes if the lamp be of 8 candles. It is interesting to notice that this charge of $5\frac{1}{2}$ centimes per hour for a 16-candle lamp is almost exactly equivalent to 8*d.* per Board of Trade unit—the legalized rate for London.

At Perpignan, the prices per hour per lamp are the same as at Manosque, but if the lighting be charged at a fixed rate per month, the rate for a 10-candle lamp is either $3\frac{1}{2}$ or 6 or 8 francs per month, depending on whether the lamp is put out every night at 10 o'clock or at midnight, or is left alight all night.

In Paris, gas costs 30 centimes per cubic metre, or, roughly, 7*s.* 6*d.* per 1000 cubic feet, while, for a 10-candle glow-lamp, 4·8 centimes per hour is charged; the user has also to pay an annual subscription of 4 francs, and he has to replace his lamp after 1000 hours at a cost of 4 francs. With these figures the author concludes that electric lighting with glow-lamps is 40 per cent. dearer than lighting with gas.

The largest single electric installations of *arc*-lamps is at Brooklyn, in America, where the Thomson-Houstin Company supply current for 1325. The following the author gives as the prices paid per hour for the current for one arc-lamp:—

Detroit	29 centimes.	
Paris	40 ,,	
Milan	44 ,,	if burning all night.
,, ...	50·2 ,,	,, only until midnight.
Marseilles ...	52·5 ,,	burning only until midnight.

To enable a comparison to be made, we have ascertained that the yearly price charged for the arc-lamps on the Parades of the English watering-places is equivalent to about 35 centimetres per arc-lamp per hour.

M. Jules Couture concludes his treatise in a very judicious manner as an old director of the Marseilles Gas Company:—"Little Jack Gas lives still, and will continue to live, I hope, for a long time yet. To prolong its existence, gas will not hesitate to inscribe on its banner, 'Gas-Electricity,' because its advocates are not at all adverse to progress."

The treatise forms a handy reference-book for those interested in the progress of electric lighting. It would have been well, however, if the author had given the price of *electric energy* in the various towns, instead of the price per ampere or the price for the current for a particular lamp. Constant reference is made to the supply of the light "*au moyen de compteurs*" in the various towns. Electrical engineers would have gladly welcomed some information as regards the character and the behaviour, satisfactory or otherwise, of the meters employed. We have already said that we think statistics of electric lighting in Great Britain might well have been added, seeing that it is now nearly two years ago since the Englishman's backwardness in taking up electric lighting began to disappear.

In justice to the consumers of gas, more stress should have been laid on the fact that ordinary gas-burners are very inefficient things. It might, for example, have been pointed out that, while as much as 6 candles per cubic foot of gas consumed per hour can be obtained with a Welsbach or with an albo-carbon burner, as little as $\frac{3}{4}$ of a candle per cubic foot consumed per hour is the meagre efficiency of certain twopenny-halfpenny nondescript burners. And if such a nondescript burner be ungoverned, as it probably will be, since people who have not the sense to buy good gas-burners are not likely to buy governors for them, then, when the pressure of the gas rises in the mains, the burner will not give more than $\frac{1}{2}$ a candle per cubic foot. So that a burner passing 5 cubic feet of London gas per hour may give any illumination from 3 to 30 candles, depending on the nature of the burner.

In dealing, therefore, with the vexed question of the relative cost of lighting with electricity and gas, we must remember that, apart from glow-lamps causing much less damage than gas to books and decorations of rooms, lighting with glow-lamps at 8*d.* per Board of Trade Unit costs no more than lighting with ungoverned common gas-burners using gas at 2*s.* 6*d.* per thousand cubic feet. On the other hand, lighting with glow-lamps costs about 3 times as much as lighting with governed Argand burners, and 4 or 5 times as much as using albo-carbon or Welsbach burners.

A TEXT-BOOK OF GEOLOGY.

The School Manual of Geology. By J. Beete Jukes, F.R.S. Fifth Edition. Edited by A. J. Jukes-Browne, B.A., F.G.S. (Edinburgh: A. and C. Black, 1890.)

THE title and success of this handy little book lead one to inquire how far and where geology is taught in schools. There is no doubt that the subject has for scholars, particularly in the country, the strongest fascination; and the fine museum of Marlborough is an example of how "natural history" studies may be kept alive in seats of youthful learning. But it would be interesting to know how many schools, excluding special evening-classes, can give such a work as this a place in their curriculum, and thus carry back the history of England, Rome, and Greece to the earliest dawn of life upon the globe. The preliminary training for the appreciation of geological features such as every lad can see around him need not be excessively severe; the mere appreciation is at first the great thing—the knowledge that there is something to be learnt in road-side quarries, in familiar hollows of the hills, beside which the "Dictionary of Antiquities" seems like a fashion-book of yesterday; while at the same time, perhaps, the kinship of the boy with his favourite classic hero becomes something more real and inspiriting in face of the enormous past beyond them both.

While the work before us is a concise and convenient text-book, we suspect that, from mere force of circumstances, it aims more at the individual student than at the school-boy and the class. Just as William Smith, at the beginning of the century, pleaded for geology as a study advantageous to land-owners, so the editor appeals in his preface to the practical good sense of parents. The

claims of education in natural history as a means to individual and mutual happiness are perhaps too well known to need assertion; at any rate, it is not fashionable to put them forward.

For schools, one would like in this book a little more of the breath of the open country, such as appears in the few lines descriptive of the Cotteswolds; but the direct appeal to Nature in chapter ix. is very refreshing and characteristic of the author. Where, indeed, the work has been altered from the first edition of 1863, it is in matters of more recent discovery, its tone being fully preserved. Some little notes have gone, such as that on the difficulties of the Welsh "ll" on p. 216 of the original (p. 261 of the present edition is more serious); but the references to history and familiar authors remain, even to Wilkie Collins, while the introduction of derivations has been considerably and interestingly extended. Quaint effects in such matters cannot always be avoided, as in the following (p. 247), "Illænus (*squint-eye*) Davisii (*after Mr. Davis*)."

It is difficult in such a book to deal with rival theories; but the discussion of coral-islands, carried over seven pages, scarcely does justice to Darwin's position, and is certainly not complete—as in accounting for the atoll—in its statement of more recent views. Nor can we consider the treatment of the specific gravity of the earth (p. 8) as altogether beyond question, accepting as it does the continuous compressibility of crystalline bodies.

To come to small matters, the use of "potash" in different senses on pp. 46 and 48 may mislead the tyro; the spelling "tachylite" is adopted for "tachylyte"; and "Protospongia fenestella" for "fenestrata" occurs on both p. 238 and p. 239. On p. 329 we have, freshly inserted, the Pterodactyl from Owen's "Palæontology." This figure, arising from the difficulty of interpreting some of the earlier specimens, still appears in well-regulated text-books, but is sometimes accompanied with warning foot-notes; here it is aggravated by having its digits numbered, and the existence of a fifth, "answering to the little finger in our own hands," is distinctly stated in the text. But the other woodcuts are numerous and effective; and we have a few bold drawings of natural features as they actually appear, which always appeal strongly to the untrained observer. It is too much to ask for full-page sketches of our British scenery; but we look back in this matter somewhat regretfully on that earlier work of Jukes, the "Popular Physical Geology," illustrated by Du Noyer, and published with undoubted spirit by Reeve and Co. in 1853. G. C.

OUR BOOK SHELF.

Magnetism and Electricity. By W. Jerome Harrison, F.G.S., and Charles A. White. (London: Blackie and Son, 1890.)

We note one or two features in this work which make it worthy of commendation; for example, the authors have avoided speaking of magnetic or electric fluids, and have endeavoured to bring out the fact that these forces are but "states or affections of matter," and their endeavour is much to be praised. It is also good to see an introductory chapter on "Matter and Force," and a special chapter on "Potential," about which elementary students, as a rule, know very little. Most of the diagrams, however, are of the stock kind, and with the exception of the above points the book possesses nothing to distinguish it from many other elementary manuals dealing with the same subject

Science applied to Work. By John A. Bower. (London: Cassell and Co., 1890.)

There is much that is praiseworthy in this little work; it is an easy introduction to mechanics, and free from all mathematical formulæ, is written in very clear language, and deals entirely with the mechanics of every-day life. The book has been designed especially for the artisan section of the National Home Reading Union, and will doubtless be a means of eradicating the rule-of-thumb work which is still characteristic of a large proportion of the artisan community. Many hints are given for making simple apparatus to demonstrate the principles laid down, the applications of these principles are well pointed out, and the work altogether meets the requirements of the class for whom it is intended.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Testing for Colour-Blindness.

Dr. Oliver Lodge asks (May 29, p. 100) why those interested in testing for colour-vision do not employ Lord Rayleigh's arrangement, in which yellow is matched by certain proportions of red and green.

This suggests to me a difficulty I have felt for many years. I am partially colour-blind, and have the usual difficulty in seeing whether a fuchsia or a *Pyrus Japonica* is in flower or not. I have noticed that many persons speak of flowers such as *Lychnis flos-Jovis*, or *Epilobium angustifolium*, &c., as being red. I should unhesitatingly class them among blue or purple flowers. They give me no suggestion of red, but I observe that when they are coloured in botanical works, such as Sowerby's "Botany," &c., they are painted of a decidedly reddish colour, and not as they appear in Nature. I used to attribute this to carelessness, but it is now evident to me that two colours which when placed side by side appear identical to normal vision do not appear at all identical to the colour-blind. Doubtless pigments could be found which would produce similar impressions on both orders of vision, but this is only a matter of chance. An investigation on these lines might give useful information.

On the question of flag signals, I would observe that though I can make nothing out of the ordinary dull greens, reds, and browns, and am ready to believe anything that is told me about them, my impressions of scarlet and orange are intensely distinct and vivid. Scarlet (and especially orange scarlet) is the most vivid and beautiful colour which I know, and utterly unlike any other (it turns to nearly black in very faint twilight). I could recognize a flag of scarlet or orange under any possible circumstances and at almost any distance. If danger flags had this colour they would perfectly suit the colour-blind, and could never be mistaken for green.

On the subject of night signals I cannot make a useful suggestion. Green lights are very distinct, but they appear to me as a poor blue with very little power in them. Red lights are distinct enough compared side by side with ordinary yellow ones, but seen alone under unfavourable circumstances there is nothing to catch the eye or the imagination, and they might easily be mistaken for yellow or ordinary lights.

London, June 9. Latimer Clark.

Coral Reefs—Snail Burrows.

In regard to Dr. von Lendenfeld's letter (May 29, p. 100) it may suffice for me to say that I had not seen his reviews of Darwin's "Coral Reefs" in the periodicals which he names (for I find it impossible to keep level with the advancing flood of scientific literature), and that if his reply "considerably modifies

the meaning" of what I wrote, I must leave it to others to settle whether this be in a favourable or an unfavourable sense.

Turning to another matter: in reference to an interesting paragraph on p. 110 concerning the excavation of rock by snails, a subject on which I once wrote (*Geol. Mag.*, 1869, 1870), may I ask whether any of the readers of NATURE are acquainted with instances of these burrows occurring in non-calcareous rocks? All which I have seen were in limestone, and, as I believe, always in a pure variety. Hence, in the case of snails, one would suspect that the excavation was mainly due to chemical action.

T. G. BONNEY.

Coral Reefs, Fossil and Recent.

I SUPPOSE it will be expected of me that I should answer the two objections raised by Captain Wharton (May 22, p. 81), viz. (1) that he knows of no steep submarine reef-slopes exceeding 4000 feet in height; and (2) that the lagoons could not be so shallow as they are if we assumed any extensive positive shifting of the coast-line.

From the statements in the literature on the subject, concerning point (1), I select the following three:—

Captain Fitzroy found at the Keeling no bottom 2200 yards from the breakers with a line 7200 feet long (Darwin, "Coral Reefs").

Bourne says in his account of Diego Garcia (Proc. Roy. Soc., vol. xliii.), that the Maldives, Laccadives, and the Chagos rise from a bank 1000 fathoms below the surface very abruptly.

Heilprin ("Bermudas") states that the Bermudas rise abruptly out of a depth of 12,000 to 13,000 feet.

Concerning point (2), I cannot see why the gentle inward slopes of atolls should not be in harmony with the subsidence theory. It must be borne in mind that the shifting of the coast-line is both slow and oscillating. Positive and negative shiftings alternate. The latter predominates on the whole. Dr. Murray says that in shallow water the accumulation of material exceeds the removal by solution. I have professed my accordance with this view in my previous letter. Particularly in an inclosed or partially inclosed lagoon, sheltered from ocean currents, this filling-up process will be a rapid one. We can easily conceive that it will balance the subsidence until the lagoon becomes so shallow as to impede the life of those organisms whose skeletons form the raising-up deposit. If there is any oscillatory negative shifting of coast-line, the dry rim will rise, and extend horizontally, and afford to the atmospheric agencies a larger surface wherefrom material can be washed into the lagoon.

On the whole, if there is anything difficult to explain, it is that the lagoons are as deep as they are. Deep lagoons are, however, not common, and are generally only met with in large and interrupted atolls. Perfectly dry central depressions (with deposits of gypsum and the like) are by no means infrequent in very small atolls. The general proportionality of the depth and the horizontal extent of the lagoons is perfectly in accordance with the subsidence theory. It supports no other theory better than this one.

R. VON LENDENFELD.

Photographs of Water Drops.

IN NATURE of May 22 (p. 95) there is an account given of the discussion following Mr. C. V. Boys's demonstration of his photographs of falling water drops at the meeting of the Physical Society. In the course of this discussion, Lord Rayleigh, who was naturally much interested in the subject, remarked that it had never occurred to him that it would be possible to get enough light from a single spark to photograph the drops as Mr. Boys had done. And Lord Rayleigh believed Mr. Boys's success was owing to the fact of his using no lenses, which would absorb the ultra-violet rays.

With reference to this, it might, perhaps, be interesting to mention that I succeeded very well, some years ago, in photographing water drops, falling through air, with single sparks, the light of the spark passing two glass lenses and the objective of a camera which gave magnified images. My photographs (copies of which appeared in the *Annalen der Physik und Chemie*, vol. xxx., 1887) show all the forms obtained so very beautifully by Mr. Boys. From photographs taken at different depths below the orifice of the tube I could measure the periodic time of the elliptical vibrations and of the vibrations according to the next higher spherical harmonic, and show that the ratio of these two periodic times agreed very closely with the formulæ given by Lord Rayleigh in the Proc. Roy. Soc., 1879. The amplitudes had no influence upon the periodic times.

Richmond, Surrey, June 6.

P. LENARD.

THE CLIMATES OF PAST AGES.[1]

I.

IT happens sometimes in the history of science that a few striking facts lead to the building up of a far-reaching theory, which at first satisfies us, and with which, without being rigorously critical, we endeavour to bring the further results of experience into conformity. But contradictions and difficulties gradually manifest themselves, and go on accumulating, until at last we are convinced that we have built on an unsure foundation, and that the edifice that we have raised upon it must be utterly pulled down. Then follows a period of discussion and collection of further evidence, during which we abstain from any attempt to substitute new and more correct explanation for that which we have abandoned, until by assiduous labour we shall have prepared a broader and more stable basis for the superstructure.

In such a stage of transition, the old ground abandoned, the new not yet won, is our knowledge of the climatic conditions of our earth in bygone ages. In the far north a rich mass of fossilized plants and coal-beds had been found in the Carboniferous formation. Reef-building corals, such as to-day live only in tropical seas, were yielded by the Carboniferous limestone and the Silurian formation up to 80° of northern latitude; and many of the species were found to range, without any essential change of form, from arctic to temperate, nay in some cases even to equatorial regions. From a small number of data such as these it was hastily concluded that, under the influence of the internal heat of the earth, a warm uniform climate must have prevailed generally from the pole to the equator, while a sultry atmosphere, heavily charged with water vapour and carbonic acid, prevented the sun's rays from reaching the earth or in any case from exercising any considerable influence on it. As a consequence, the existence of climatic zones or of a distribution of the fauna and flora in such zones was denied. It was held that with the beginning of the Tertiary era a polar cooling first set in, and that it increased during its passage, until the present distribution of heat was brought about as the final result of this long-continued process.

The falsity of these assumptions is now pretty generally recognized, and the number of their adherents diminishes daily. It would lead us too far afield were we to follow out the hypothesis into all the details of its ofttimes fantastic errors, and to note their individual failure. It will be more to the purpose if, in the first place, we test the methods by which we arrive at conclusions on the temperature conditions of past ages, in order that we may thus gain a knowledge of what these really were and of the better-grounded attempts to explain them.

Among the more important data for judging of the climate of a past epoch, is the character of its plants and animals, on the assumption that these various organisms must have lived under nearly the same conditions of temperature as their nearest relatives now existing. This kind of reasoning has been very extensively applied, and within certain limits its validity cannot be gainsaid. If, for instance, in a comparatively recent deposit of the Pleistocene period in Central Europe, we find remains of the arctic willow, the dwarf birch, the white dryas, together with such mammals as the lemming, the musk-ox, the

[1] Translation of a Lecture delivered by the late Dr. M. Neumayr before the Society for the Dissemination of Natural Science, at Vienna, on January 2, 1889.

glutton, the arctic fox, and also certain snails which, at the present day, live in Lapland or the higher Alps, we may safely conclude that a severe climate formerly prevailed there. An example of the opposite kind is afforded us by the later Tertiaries, which belong, indeed, to a considerably earlier but still not very remote period. Here we find, in our own neighbourhood, a flora of plants with evergreen coriaceous leaves, such as now grow in the warmer parts of the Mediterranean area, and we are quite justified in concluding that a higher temperature was once here prevalent. But, although in many cases such conclusions are well founded, a universal extension of this kind of reasoning leads to deceptive results, and the whole method must be applied with the greatest caution.

In the first place, we must bear in mind that, even at the present day, some forms that are nearly related to each other live under very diverse conditions. Antelopes, for instance, are for the most part animals characteristic of warm regions, and yet a kind of antelope, the chamois, lives in a very severe climate in the high mountains of the temperate zone. The arctic fox lives in the far north beyond the polar limit of trees, the Fennec in the burning African desert, and yet the two are nearly related to each other. The elephant and rhinoceros are at the present time peculiar to hot countries, and yet we know from unmistakable evidence that species of both these genera prevailed in Europe and Northern Asia in the cold Pleistocene climate. We have similar instances among marine animals, and we may adduce a whole series of cases in which a group of forms is predominantly peculiar to a certain kind of climate, but have individual representatives living under totally different conditions. The molluscous genera *Voluta* and *Terebra*, for example, are among the most characteristic inhabitants of warm seas, but each of them has a representative in the icy waters of the Magellan's Straits. And among land plants we have the remarkable fact that many forms of the north temperate zone, when they have been transplanted or have escaped to far warmer regions, have extended in an extraordinary manner, and locally to such an extent that they have overpowered and displaced the indigenous flora, as has occurred with the most diverse species of European weeds when transported to foreign countries.

On the whole, we are inclined to infer that, with the exception of the Pleistocene fauna and flora, the animal and plant remains of past ages, in their generality, point to a warmer climate than that which we now experience; and in point of fact, several very striking items of evidence lead to that conclusion. The most important is the very great extension of reef-building corals in the older deposits, while their modern representatives are restricted to the warmer seas.

Many other instances of the same kind may be quoted, while in other cases similar conclusions have been somewhat uncritically based on insufficient evidence. Thus, some have inferred the prevalence of a high temperature from the abundance and occasional great size of the chambered-shelled Cephalopoda, solely because the last existing representative of this once widely distributed group, the well-known Nautilus, happens to live in a warm sea. This conclusion is quite unjustified, for it is obvious that the many thousands of extinct species must have lived under very varied conditions: and if we are to infer, from the great size of these creatures, that they lived in warm seas, we ignore the fact that the largest Cephalopoda of the present day, the cuttle-fishes, are most prevalent in the northern part of the temperate zone.

But even when we have excluded all such evidently erroneous cases, the number of those in which fossil forms do really present the characters of types highly characteristic of warm regions is very considerable. It is true, that the opposite case sometimes presents itself, though less frequently. Thus, in all the older formations, a group of Bryozoa, the Cyclostomata, is extensively distributed, but it is now especially preponderant in the circumpolar seas. The molluscous genus *Astarte*, so common and widely distributed in Mesozoic deposits, is at the present day entirely restricted to cold seas; and there also occurs the last representative of the once widely spread genus *Cyprina*. The Brachiopodous genus *Rhynchonella*, common in the Silurian formation, and especially abundant in the Jurassic and Cretaceous formations, is now a form of high northern latitudes, and the Squaloid genus *Selache*, now restricted to the seas of Greenland, occurs in Cretaceous deposits in much more southerly latitudes.

Such instances, and they are far from singular, teach us, unmistakably and assuredly, that animal and vegetable types are not unchangeable in respect of the external conditions of their existence, and especially of temperature, but that they are capable of accommodating themselves to changed circumstances. Whether, then, we infer that reef-building corals formerly lived in cooler waters, or that Cyclostomoid Bryozoa frequented warmer waters than at the present day, or finally that both have changed their habit of life, the conclusion is overwhelmingly forced upon us that organisms continually adapt themselves to changed temperatures, and in a far higher degree than has generally been supposed.

In connection with this, we may notice a very remarkable circumstance, viz. the great vitality, adaptability, and toughness of the organisms of the temperate and especially the north temperate zone, when transported to other parts of the globe. Just as European man carries on a successful struggle with the children of all other zones of the earth, so also do the animals and plants indigenous to Europe, and especially those of its central and northern parts. As already remarked, when they are transplanted to foreign countries, they extend rapidly, and often drive out the indigenous forms; English naturalists who have had most opportunities of observing these relations in their colonies, speak expressly of the great aggressiveness of North European organisms.

At the present time, when the dissemination of the most diverse forms is brought about in the highest degree by world-wide human intercourse, such displacements present themselves in a particularly striking manner, and yet similar processes must have gone on, more slowly indeed, but on a far greater scale, for many millions of years. At some given epoch, a certain assemblage of organic forms appears in moderately cold regions, from which colonists wander away southwards; these gradually adapt themselves to the new local conditions, and spread still further, until, at last, their further progress is stayed by some natural barrier. They become acclimatized under the new conditions and the higher temperature, and become enfeebled; but in the meantime new forms have been developed in their former home, which in their turn pursue the same course and suffer the same fate; and thus the southern types always display a certain relationship to the older forms of the northern region, without any change having supervened in the temperatures of their respective stations.

We now see with how great caution we must proceed, when we attempt to draw any inference as to the temperature of former ages from the relationship of species, stratigraphically remote, with those of the existing organic world. The danger of error is here very great, and it is the greater and more menacing, the older the deposit the climatic conditions of which are in question; obviously, the probability of a change having taken place in organic constitution with regard to temperature is the greater, the more remote the epoch with which we are dealing. While, therefore, we may deduce conclusions having some claim to probability, on the climatic conditions of Pleistocene and Tertiary times, even in the Mesozoic

deposits such conclusions become doubtful, and quite untrustworthy when we are concerned with Palæozoic times. We must, indeed, admit that as the result of more searching criticism, and the increased knowledge of the facts which the labours of many years have now amassed for us, we are not in a position to answer that most important and fundamental question whether a continuous and universal cooling of terrestrial climate has or has not been progressing from the time of the earliest stratified deposits down to the present day.

The difficulties with which we have to contend in dealing with this problem may be illustrated by a very significant example. The fact has already been noticed that reef-building corals occur only in warm seas, in which, throughout the year, the surface-temperature never sinks below 20° C. If now we compare the geographical distribution of the reef-building corals of the older formations, we find in very early times, in Silurian and Carboniferous deposits, the remains of such corals beyond the Arctic circle; at a much later period in the Jurassic formation, we find that they reach only to North Germany and to Southern England; during the second half of the Cretaceous formation, they do not pass the northern limit of the Alps and the mountains of Southern France, and their northern limit in the first half of the Tertiary era is nearly the same. At the beginning of the second half of the Tertiary era, we find them but scantily represented on the northern boundary of the Alps, and abundant only in Southern Europe; and in the latest subdivisions of the Tertiaries, in Pliocene times, they have almost disappeared from Europe.

From these facts it might seem almost a manifest conclusion that there has been a continuous fall of temperature since Silurian times, in consequence of which reef-building corals have retrograded through some fifty degrees of latitude; nevertheless, on closer examination, we find that such an inference would be altogether premature. In the first place, the Palæozoic corals differ very essentially from those that now exist, and therefore their requirements in respect of warmth may have been totally different; further, we have no knowledge of any coral reefs in the far north in all the older formations; and between the Carboniferous and Jurassic formations which we have cited, there intervene the Permian and the Trias in which we know of no reef-building corals so far north; the most northerly representatives of the group in Permian times appear in North-Western India, those of Triassic times in the Alps. We are therefore absolutely ignorant whether these changes of distribution, supposing them to have depended on the temperature, are not to be ascribed to alterations in the distribution of temperature, while there may have been no continuous cooling. Lastly, it is by no means definitely ascertained that the position of the earth's axis has always been the same as at present; indeed, there are in the course of geological time certain definite epochs pointing to such a displacement of the poles, of which we have yet to gather the meaning. It may therefore be the case that those parts of the earth at which we find Silurian and Carboniferous corals in the neighbourhood of the pole, were much nearer the equator in those early times than they now are.

Similar difficulties present themselves in all our attempts to arrive at far-reaching conclusions by this method, and thus we are admonished how great caution must be exercised in the face of so many sources of error. Another method, by means of which it has been sought to attain some holding ground for determining the climatic characteristics of early times, is that, leaving out of consideration the conditions under which nearly related organic types exist at the present time, we should simply regard the extent of the geographic distribution of extinct organisms, and from their wide distribution conclude the existence of a uniform climate over very great areas, nay even over the entire globe. But in such an attempt the risk of over-estimating the facts is imminent, and especially is this true in the case of marine organisms; in a former state of our knowledge, we might well have believed that ancient forms of life had a wider distribution than such as now exist, since our knowledge of the tenants of the present seas related almost exclusively to those of shallow water and the coasts, many of which have a restricted range. But from the epoch-making deep-sea soundings of the last decennia, we have learned much of the inhabitants of the depths of the ocean, and have become aware that they possess much the same characters in all parts of the world; so that, in this respect, there is no essential difference between the present and former ages. As a fact, we have ascertained, from the distribution of organic life, that climatic zones existed in most of the early periods, and that this has not been done in some cases may be simply ascribed to the fact that they have not yet been rightly investigated.

Side by side with the diverse indications afforded us by the animal and vegetable worlds, regard must be had to the petrographic characters of the old deposits. We have rocks which have issued from the interior of the earth and have solidified from the fluid state, others have been deposited from water, and in the formation of others, again, ice has played an important part, and this mode of formation is generally recognizable by well-marked characters. For our present purpose, only such masses are important as have been transported by ice and thus brought to their present position, for these alone furnish us with conclusions as to temperature conditions; they inform us that, whenever they occur, the cold has been, at least at times, sufficiently great to freeze large masses of water.

The marks of ice action are well known. A moving glacier polishes and scores or scratches its rock floor, and carries with it fine silt, sand, great and small stones, and even mighty boulders, and deposits these materials in its moraine, without sifting them according to magnitude, as in the case of transport by water. Polished and grooved rock surfaces, scratched pebbles, and deposition without stratification in a confused mixture of silt, sand, coarse pebbles, and enormous blocks, are the indications of glacier deposits; in the identification of which, nevertheless, great caution is necessary. If a glacier reaches the sea or a great lake, under certain conditions, masses of ice may be floated away to great distances, carrying with them the enclosed stones and boulders, and often deposit them when thawed out of the iceberg, on the sea-bottom at great distances from their place of origin. Thus the deep-sea investigations of the *Challenger* show that, in high southern latitudes, in the deep sea and far from any coast, numerous stones lie scattered on the fine silt of the ocean-floor, and these can have reached their present position only by such means of transport. Such indications are, however, not quite undeceptive, since it sometimes happens that stones are transported in the roots of trees which are carried by rivers into the sea; but this kind of transport is operative only to a very small extent. When, however, we find in old formations water-formed deposits of fine clay or sand extending over great areas, in which numerous great stones and boulders are promiscuously intermingled, we may infer that they have been transported by floating ice, especially when the stones moreover are scored.

We have now learned what are the most important indications from which we may draw inferences as to the climatal conditions of past ages, and we have endeavoured to ascertain how far, and within what limits, such inferences are legitimate. Our next task will naturally be to apply the conclusions thus established to the phenomena which we meet with at different epochs of the past, and to form a conception of the climatic relations of those times, and of the conditions depending on them. It would, however, lead us too far afield were we to discuss each period in detail, and we must restrict ourselves to a

hasty sketch of a few especially important formations that have been closely studied.

We pass over the oldest deposits, for the interpretation of which but few points of vantage present themselves, and we shall fix our attention on the upper half of the Carboniferous formation, the so-called Coal-measures. It has received this name because in many countries it contains those thick beds of fossil fuel which have become an indispensable factor of modern industries, and without which the actual status of our social and political condition could not have been attained. So great is the quantity of the fuel herein stored, that all that is furnished by other geological formations, taken together, falls far short of it. There is much difference of opinion as to the mode in which coal has been formed; but whatever disagreement there may be in matters of detail, this much is certain, that we have in coal the altered remains of a land-vegetation, which, partly at least, flourished in swamps. Of course the formation is not all or even chiefly coal; even where it is richest in coal, by far the greater part of the formation consists of shale, sandstone, and conglomerate, and the coal-beds are here and there interstratified, forming but a fraction of the total thickness. We may picture to ourselves the building up of the formation by supposing that a plain or depression was sometimes covered with water, sometimes dried up. When flooded, chiefly with fresh and but rarely with sea-water, beds of shale or sandstone were deposited; when dry, land or swamp plants sprang up, and their decayed remains furnished the material of coal. Then followed another period of inundation, and thick beds of shale, sandstone, and conglomerate were again deposited. . . . .

The vegetation that in its decay formed our beds of coal was of a peculiar character. As yet there existed no trees (with true foliage) and no flowering plants. A monotonous growth of plants with stiff leaves then clothed our continents. A great part in it was played by *Calamites*, great plants which no longer exist, and whose nearest relatives are the mares' tails so often met with in marshy ground; another important type was that of the *Lepidodendra*, large trees whose forked stems were covered with leaf scars arranged in a regular geometrical pattern, and the branches of which were clothed with short, stiff, grass-like leaves; and most important of all, the *Sigillarias*, the unbranching and twigless stems of which were marked with leaf scars in perpendicular rows and scale-like leaves. Both of these are long since extinct; and only the insignificant club-mosses of our present flora recall to us the varied gigantic forms of that distant age.

H. F. B.

(*To be continued.*)

LIGHTNING AND THE ELECTRIC SPARK.[1]

AT a date at least as remote as 600 years B.C. the Greek philosophers were acquainted with a curious little fact to which the modern science of electricity owes its name. They knew that a piece of amber (ἤλεκτρον) when rubbed against some suitable substance acquired a temporary attractive power, in virtue of which it became capable of lifting and holding light objects, such as dry leaves or pieces of straw. But another remarkable effect which often attends the friction of amber was for many centuries altogether overlooked. In A.D. 1708 it was first noticed by Dr. Wall that a piece of strongly excited amber emitted sparks, which were accompanied by crackling sounds, and these he had the sagacity to compare to thunder and lightning.

It must be confessed that the recognition of any resemblance between the microscopic scintillations thus produced and the brilliant lightning flash imposed a somewhat severe strain upon even the scientific imagination, and a few years later Stephen Gray, in reference to the same comparison, expressed the hope that "there might be found out a way to collect a greater quantity of the electric fire" than was then possible. His hope was realized by the subsequent improvement of electrical apparatus, and especially by the invention of the Leyden jar; and the effects obtainable by the means now at our command amply justify the speculations of Wall and Gray. The essential identity of the artificial electric spark with the natural lightning flash was conclusively established by the experiments of Franklin, and in these days it has become a mere common-place, familiar to everyone.

There are generally said to be two kinds of lightning flash, which are known as forked lightning and sheet lightning, the former being dangerous and destructive, the latter harmless. To these is sometimes added a third class, called ball lightning. The lightning flash of artists which is familiar to us from innumerable pictures, and of which the venomous-looking zigzag now projected upon the screen (copied from an engraving) is a fair example, has no existence in nature. It is simply an artistic fiction or symbol, like the conventional representation of a galloping horse, which, in the severe language of Mr. Muybridge, resembles nothing to be found in the heavens above or in the earth beneath. The absurdities commonly perpetrated in depicting animals in motion have been fully exposed by Mr. Muybridge with the assistance of photography. So, too, it is photography that has given the *coup de grâce* to the traditional forked lightning. Within the last few years an immense number of photographs of lightning flashes have been made. The Meteorological Society has formed a collection of these, containing about 200 examples, which, by the kindness of Mr. Marriott, the Secretary of the Society, I have had an opportunity of examining carefully. Not a single instance of the artistic lightning flash is to be found among them. The great majority bear a close resemblance to the sparks of our electrical machines: a few are distinguished by peculiarities which, though at first sight a little difficult to account for, can generally be explained and even imitated artificially.

What may be called a typical lightning flash is a stream of light which follows a sinuous and wavering course, very like that of a river as shown upon a map. [Several photographs of this kind of flash were exhibited by means of the lantern.] The next slide is a photograph of a machine-spark, about 3½ inches in length. The two kinds of discharge are so much alike in their general character that if it were not for the surroundings it would be hard to tell which was the lightning and which the artificial spark.

The variations upon the normal type of flash, which the Meteorological Society's photographs show, have been classified as ramified or branched lightning, beaded lightning, meandering or knotted lightning, ribbon lightning, and, lastly, dark lightning.

Branched lightning is again strikingly suggestive of a river in a map; not a simple stream, however, but one into which a number of tributaries flow. [Photographs were shown.] Sparks having branches of just the same character are easily produced by a large electrical machine. To obtain the effect well, the negative terminal should be made much larger than the positive, and the two should be separated so far that a spark will only just pass between them. According to Faraday, a ramified, or as he sometimes calls it a "brushy," spark occurs when the whole of the electricity has not been discharged, but only portions of it, more or less, according to circumstances. It is a "dilute" spark, generally passing to air or other badly conducting matter ("Exp. Res.," § 1448). When therefore a ramified flash occurs we may reason-

[1] Extracted from a lecture on "Electrical Phenomena in Nature," delivered by Mr. Shelford Bidwell, F.R.S., at the London Institution on February 10, 1890.

ably conclude that the discharge is partial and incomplete.

In beaded lightning there occur a number of bright spots, giving the flash the appearance of an irregular string of lustrous beads. This phenomenon is sometimes well shown in photographs of the machine spark, especially when the quantity of electricity passing is increased by using very large Leyden jars. Under these circumstances the path of the discharge is often found to contain at irregular intervals certain small and abrupt V-shaped indentations, and these, especially when seen "end-on," appear to be more luminous than other portions of the flash. Probably, therefore, in a beaded flash the quantity of electricity passing is more than ordinarily great.

Sometimes a lightning flash appears to take a very circuitous and roundabout path, perhaps forming a nearly closed loop, or even a complete knot. Such is what the Thunderstorm Committee of the Meteorological Society have called "meandering" lightning. This remarkable effect is no doubt the result of an optical illusion, and occurs when the general direction of the flash (or of part of it) is either towards or away from the observer.

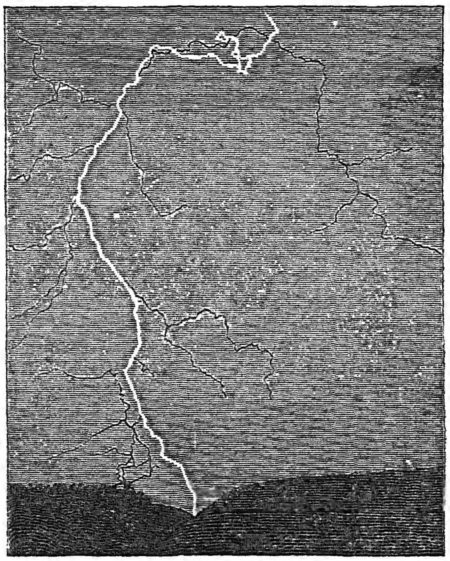

Fig. 1.

The different parts of the flash which seem to approach or to cross one another may in fact be miles apart. This explanation may be simply illustrated by means of the shadow of a properly bent wire. I have here a wire which is bent in such a form as to imitate a common type of flash or machine spark. When held transversely to the beam of the electric light its shadow is seen to represent fairly well the form of an ordinary sinuous flash; but if it is turned round so that its length is in the direction of the beam of light, the shadow presents an intricate appearance of loops and knots. Fig. 1 is from one of the most remarkable photographs of lightning flashes that I have seen. It was taken at Cambridge on June 6, 1889, by Mr. Rose, of Emmanuel College, and I am indebted for this copy to the kindness of Mr. W. N. Shaw, who described it at a recent meeting of the Physical Society. Among its many interesting features I will at present only direct your attention to the complicated knot which occurs in the upper part of the flash.

Many photographs of lightning have a curious flat and ribbon-like appearance. Such ribbons are sometimes broad and sometimes narrow. I have to thank Mr. Clayden for an excellent specimen of the broad kind, which was taken by himself last summer, and is reproduced in Fig. 2. The Thunderstorm Committee are of opinion that this peculiar structure may possibly not exist in nature at all, the effect being produced only in the photographic camera. It is noteworthy that, in nearly if not quite every case when broad ribbons have been obtained, the camera was held in the operator's hand, a fact which naturally suggests the idea that the widened image of the flash may be due to unsteadiness. It may be objected to this explanation that the duration of a lightning flash is so exceedingly brief as to preclude the possibility of any material movement during the time that its image is upon the sensitive plate. But such an objection is not unanswerable. It has often been observed that a lightning flash may be followed by one or more other flashes in rapid succession, all taking precisely the same path as the first. If then the camera were in motion a series of such flashes might impress themselves side by side upon the photographic plate, being so near together as to give the appearance of a single wide and flattened flash.[1] Moreover, though the true lightning flash is practically instantaneous it sometimes has a phosphorescent glow along its track, which lasts for at least a large fraction of a second. This phosphorescence would tend

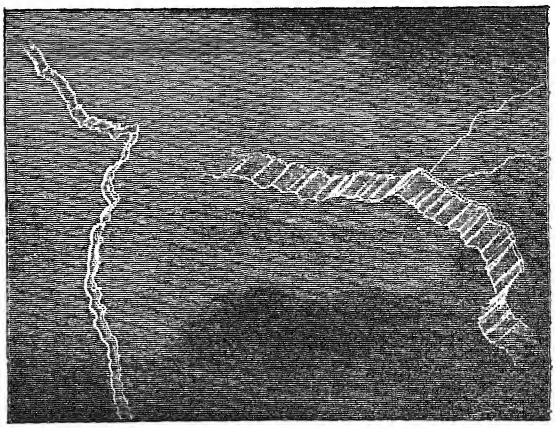

Fig. 2.

to connect the separate images into a uniform whole, and add to the ribbon-like appearance of the resulting picture. Dr. Hoffert has been kind enough to give me a copy of an exceptionally interesting photograph[2] which illustrates this explanation. The camera was held in the hand, and moved horizontally to and fro at the rate of about once in three-quarters of a second. The movement was continued until a flash was observed, when the lens was at once covered. The plate after development showed no less than two triple flashes and one double flash, eight in all, the whole of which, must have occurred, Dr. Hoffert thinks, within a little more than a second, forming a connected system of discharges which would appear to the eye as one. The several sets of flashes in the photograph are all joined together more or less perfectly by horizontal luminous streaks, which, though they may not impossibly represent a continuous brush-like discharge, are more probably due to phosphorescence of the oxygen of the air, oxygen, especially in the form of ozone, being a phosphorescent substance. If in taking Dr. Hoffert's photograph the camera had been moved

[1] It has been pointed out by Prof. S. P. Thompson that the path of the discharges might be shifted by the wind to a sufficient extent to produce the ribbon-like effect, even if the camera were perfectly steady.

[2] A good reproduction of this photograph is given in the *Phil. Mag.* (1889), and in the Proceedings of the Phys. Soc., vol. x. p. 176.

slowly instead of quickly, I think it is clear that the appearance of one or more ribbon-like flashes, like those in Fig. 2, would have been produced.

But the photograph of a flash may possibly assume a distinctly broadened form, perhaps more suggestive of a flattened wire than of a ribbon, when the camera is absolutely steady. In such cases it will generally (perhaps always) be seen that one edge of the image is sharp and clear, while the other is ill-defined and hazy. I have succeeded in imitating this effect very well in photographs of the machine spark: it is obtained when the light does not fall perpendicularly, or nearly so, upon the sensitive plate, and is no doubt due to successive reflections between the surfaces of the lens. [Exhibited.]

Lastly, we have to consider the so-called "dark flash." It occasionally happens that, on developing a photographic plate which has been exposed during a thunderstorm, the image of a lightning flash comes out black instead of white. Fig. 1 presents a striking instance of this phenomenon. Black ramifications are seen to proceed outwards on both sides of the main bright flash; there is also what appears to be an independent black flash which starts from the top of the picture and crosses the bright one near the knot. The origin of this strange appearance was for a long time a mystery. No one had ever seen a dark flash with the unassisted eye, and the question arose, whether the dark images in the photographs really represented a hitherto unobserved physical effect which occurred in the air itself, or whether, owing to some optical or chemical action taking place inside the camera or upon the sensitive plate, the impression of a luminous flash became converted into a dark one. There is no need to discuss the several ingenious hypotheses which were suggested in explanation of the anomaly; it is sufficient to say that the mystery was completely cleared up a few months ago by the experiments of Mr. Clayden. The fact, as demonstrated by him, is shortly this. If the lens of the camera be covered the moment after a flash has occurred, the developed image will always come out bright, feebly or strongly according to circumstances. If, however, the plate be exposed after a flash has acted upon it, either to the continued action of a feeble diffused light or to the powerful glare arising from one or more subsequent flashes, then on development the image of the original flash will probably come out black. The effect is therefore not a meteorological or physical one, but purely chemical. It can be obtained not only with a lightning flash, but also with a machine spark, or even with an ordinary flame. It is merely necessary that the plate should be exposed to the action of a certain amount of light after it has received the impression and before development.

Some photographs which I have made of machine sparks fully confirm this explanation of Mr. Clayden's. The room was illuminated by a single gas-jet, and the background was a white screen with a black post in the middle of it (see Fig. 3). Two series of sparks were passed between the ball terminals of an electrical machine and photographed. After the first series were taken, the lens was left uncovered for half a minute; then it was capped, the camera shifted slightly, and the second series taken; the lens was again left open for half a minute, and the plate afterwards removed from the camera and developed. It will be seen that while the second series of sparks come out bright in the natural way, the first series have been reversed and blackened by the action for one minute of the light reflected from the white screen upon the undeveloped image. Exposure to the diffused light for half a minute only was not in this case sufficient to cause reversal.

These experimental results make it almost certain that the flash in Fig. 1 was really a double one. The first flash was comparatively feeble, and possessed the lateral ramifications characteristic of an incomplete discharge. The second, which probably occurred immediately afterwards, was a powerful one without ramifications, and followed accurately the main path traced out by the other. The glare arising from this second discharge caused the photographic reversal of the ramifications belonging to the first.

Everyone must have noticed the proverbial quiver of a lightning flash. This peculiar effect is often due to the multiple discharge of which we have already spoken. Sometimes, however, I believe the phenomenon is a purely subjective one, depending upon a certain physiological reaction of the optic nerve. If we gaze at a bright flame which is suddenly uncovered and immediately extinguished, then after a very short interval of darkness a distinct but transient image of the flame will reappear; and it is even possible that after another brief interval a second after-image of the flame may be seen. It is, however, by no means easy to detect these appearances without considerable practice, because they belong to a class of impressions which we habitually train ourselves to disregard. But by means of a little device which I published a few years ago, the phenomenon may be easily demonstrated to almost anyone.

The beautiful effects produced by the rotation of a vacuum tube when illuminated by a series of discharges

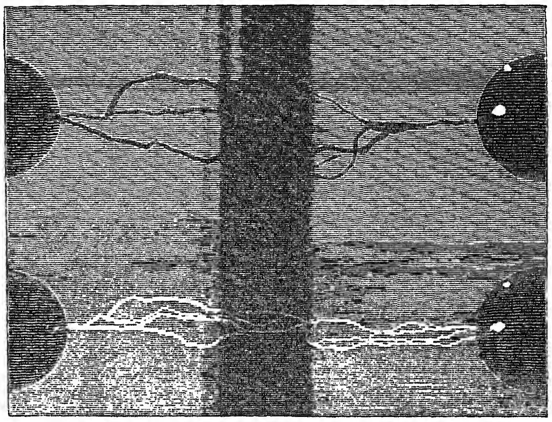

FIG. 3.

from an induction coil, are well known. The tube is generally attached to a horizontal axis, which is turned rapidly by means of a multiplying wheel; the images due to successive discharges, which, if the tube were at rest would be superposed, are thus caused to occupy different parts of the retina, and the result is the appearance of a gorgeous revolving star. But if the tube is caused to rotate very slowly, making about one turn in two or three seconds, there occurs a different and very curious phenomenon. The luminous images of the tube are almost superposed, forming a bunch which is slightly spread out at the ends. But about 40° behind the bunch, and separated from it by an interval of darkness, comes a ghost. This ghost is in shape and size an exact reproduction of the tube; it is very clearly defined, and is of a uniform bluish-grey tint. If the rotation is stopped, the ghost still moves slowly on, and after the lapse of about half a second disappears in coalescing with the luminous tube. The phenomenon of the ghost is clearly due to a succession of after-images, which are perceived a short time after the retina has been impressed by the flashes from the vacuum tube; and a similar physiological action, I think, explains—at least in some cases—the apparent reduplication of a flash of lightning.

Within the last year or two there has been a great deal of rather lively controversy concerning the protection of

buildings from the destructive effects of lightning. The controversy originated in some lectures on lightning conductors, delivered by Prof. Oliver Lodge at the Society of Arts in 1888; it was continued at the Bath meeting of the British Association, and it culminated in a paper, also by Dr. Lodge, read last year at the Institution of Electrical Engineers, in which, after stating that "the old views on the subject of electrical conduction are hopelessly and absurdly and dangerously inadequate," the author expressed the opinion that it was "time that the prophets of the old superstition were slaughtered by the brook Kishon." In the animated discussion which followed, Dr. Lodge's views were ably opposed by Mr. Preece and others, and the question can hardly yet be considered as definitely settled. Time will not admit of an adequate review of the arguments which were employed on the two sides, but, considering its great practical importance, I think it will be of interest to give a very short statement of the matter in dispute, which I will illustrate by copies of Dr. Lodge's diagrams and apparatus.

Ever since the time of Franklin it has been customary to make use of long pointed metallic rods for the purpose of protecting important buildings from damage by lightning; and the "older electricians," as Dr. Lodge calls them, have always taught that, if the rod were well made, of sufficient size and height, and properly connected to earth, it afforded practically perfect security over a certain limited area. The function of the rod was supposed to be not so much to receive the shock of a lightning flash as to prevent a flash from occurring at all in the neighbourhood of the protected building: this it did by promoting the silent discharge of electricity between the cloud and the earth through the point of the rod.

The lower of these two tinfoil-covered boards represents the earth, and the upper one a cloud; the upright metal rod with a ball at the top of it is supposed to be a church, or other building, erected upon the earth. Charging the apparatus by means of the electrical machine, we get a series of strong flashes between the cloud and the church, every one of which might do terrible damage. If now we place near the church another rod, with a needle-point at its end, to serve as a lightning-conductor, the flashes at once cease: however vigorously we work the machine, there is no longer any visible effect. The fact is, that the electricity is silently and harmlessly discharged as quickly as it is generated. In such a case as is at present represented by the model, the efficacy of a lightning conductor would be complete. This is what Dr. Lodge calls the case of "steady strain," and is that indicated in his first diagram [exhibited], where the charged cloud above the church spire is supposed to have moved into its present threatening position from a distance. According to Dr. Lodge, this is the only kind of lightning discharge which was ever contemplated by the older electricians.

But suppose that a harmless uncharged cloud which might be hovering over the church were suddenly to receive an overflowing charge of electricity by a flash from another more distant cloud. There would then be no time for any gradual relief of the strain by a silent discharge through the lightning conductor, and either the conductor itself or the church would infallibly be struck by a flash from the overflowing cloud.

By altering the connections between the model and the electrical machine,[1] we can easily imitate this condition of things. The tinfoil-covered boards now remain absolutely uncharged until the moment when there is a spark between the terminals of the machine: then they are suddenly charged, and a flash instantly passes between the cloud and the church. Placing the needle-pointed lightning conductor beside the church, we now find that it is powerless to prevent the flashes: they go on just as rapidly as before, striking either the conductor or the church, or sometimes both at once. This case, which, I think, Dr. Lodge was undoubtedly the first to call attention to explicitly in connection with thunderstorms, is called by him the case of "impulsive rush." The occurrence of an "impulsive-rush" flash, then, cannot be warded off by a lightning conductor. The most that a conductor can do is to divert the main shock of the discharge from the building to itself. But even so the lightning may do considerable damage, for, as Dr. Lodge says, "it is hopeless to pretend to be able to make the lightning conductor so much the easiest path that all others are protected. All possible paths will share the discharge between them, and lots of apparently impossible ones." Moreover, not only is the lightning conductor itself, when struck, liable to spit off sparks laterally, however good its earth connection may be, but other metallic bodies in the neighbourhood may do the same, whether such bodies are insulated or not.

The moral appears to be this. In all cases of steady strain in which a charged cloud descending from the upper regions of the air, or approaching from a distance, might inflict serious injury upon an unprotected building, a well-designed and properly earth-connected lightning-rod is an absolute safeguard. In a case of "impulsive rush," the rod may often be of use in bearing the brunt of the discharge, though sometimes the lightning will take no notice whatever of it, striking the building and altogether neglecting the rod; and it is even possible that a high rod might attract a destructive discharge which otherwise would not have occurred at all. Although, therefore, a lightning-rod is in many cases, probably in a very large majority, of the greatest service, it cannot be depended upon as affording perfect immunity from risk; and the assumption which has universally been made by the "older electricians," that damage by lightning is in itself conclusive evidence of some imperfection in the conductor, is an unfounded one.

In conclusion, it may not be out of place to say a word or two on the subject of personal danger from lightning. The spectacle of a severe thunderstorm, magnificent as it is, is no doubt calculated to inspire a certain amount of alarm. But statistics show clearly enough that, at least in this country, its bark is worse than its bite. It appears, from a paper published last year by Inspector-General Lawson, that the number of deaths caused by lightning in England and Wales from 1852 to 1880, as recorded in the returns of the Registrar-General, were 546, or rather less than 19 per annum. The average population during that period may be taken as 22 millions; it follows, therefore, that the average annual death-rate from lightning was considerably below 1 per million of the population. The risk of a fatal lightning stroke in any individual case is therefore exceedingly small.

[1] The tinfoil-covered boards were connected with the outer coatings of the Leyden jars, their inner coatings being in connection with the terminals of the machine.

SPORTS.[1]

It is highly desirable that we should attach a definite signification to this word. Among gardeners it may mean many things, whilst, among botanists, it is restricted to cases of bud-variation as distinguished from variation from seed. In this note we shall use the word in its botanical sense, as applying to a special illustration of that tendency to vary which is common to all living beings. We shall, however, gain a clearer idea of what true sports are by the elimination of certain things which are not sports, though often called so. In the first place they are not seedling variations. Out of a hundred seeds of Lawson's Cypress that are sown it is possible, I suppose,

[1] Reprinted from *Garden and Forest*. The article contains the substance of an unwritten address lately given to a society of gardeners.

to get ten more or less distinct varieties, besides others which are more or less indistinct. The great variability of this species is now well known, and the seedlings of *Abies subalpina*, Engelmann (*A. lasiocarpa* of Hooker), furnish another illustration of the same tendency. These seedlings may be the result of cross-fertilization between varieties, or they may be reversions to an earlier condition; at any rate, of whatever nature they are, they are not "sports" in the sense here intended.

Next, sports are not mere stages of growth. Most plants put on a different appearance at various periods or stages of their growth, and sometimes these changes are very remarkable. The Retinosporas of our gardens furnish us with excellent illustrations. *Retinospora* (or more strictly *Thuya*) *pisifera* exhibits during its growth very different appearances in its foliage. There is the squarrose form and the plumose form, the golden form, the silver form, the pendulous form, the thread-like form, the upright form, and perhaps others. All these, however, are not separate entities; they may all occur on the same bush. If cuttings or if grafts be taken from the sporting branches they may be reproduced almost indefinitely.

Barring the mere colour variation, these forms are but stages in the growth of the plant, occurring with more or less regularity and in greater or less degree of prominence in all the individuals of the species, as may be inferred from watching the growth of seedlings in a seed-bed.

Other illustrations of variations arising during growth are afforded by the differences often observable in the foliage on the flowering branches as contrasted with that on those branches which bear no flowers. The common Ivy furnishes an illustration. The short contracted shoots of the Laburnum, or the Apple, known as "fruit spurs," constitute other examples.

Another form of variation in flowers is that connected with difference of sex. A "pin-eyed" Primrose does not greatly differ in appearance from a "thrum-eyed" one, yet the difference between them is precisely of the same character as that between the variously formed flowers of some species of Catasetum and Mormodes. So utterly different are the male and female flowers of some of these species that they were at first placed by very competent botanists in different genera. It was only when the Protean plants produced all the forms of flowers on one and the same spike, that it was seen that, so far from belonging to different genera, they did not even belong to different species. It was left to Darwin to show what this paradoxical variation really means; and now, when we meet with a case of the kind, we say, "Ah! yes; only a sexual form," just as if we had known all about it from our earliest years, and very possibly, in our haste, mixing up, or, at least, not discriminating cases of a different nature. But this is not what we propose to discuss just now; we simply say that these cases, though often so designated, are not sports, at least in our acceptation of the term.

What, then, are sports? We have already characterized them as "bud-variations," but we must give some further indication of their peculiarities: First, as to the suddenness of their production. A tree or a shrub, all on a sudden and without any cause that is apparent to the eye, will put forth a bud, which, as it lengthens into a shoot, displays leaves of a different character from any that the plant has hitherto produced, which have no definite relation to any particular stage of growth; and which are quite different from any that under ordinary circumstances the plant in question has produced or is likely to produce in future. In short, the occurrence is sudden and unforeseen. Gardeners, of course, avail themselves of these variations. They remove them, bud them, graft them, strike them from cuttings, or, in some way or another, endeavour to perpetuate the variety, and thus have originated many of our cut-leaved Beeches, Maples, and Limes. Thus, too, may have originated some of our weeping trees and some of our pyramidal shrubs, though, for the most part, these have, as I believe, originated as seedling variations.

Not only do these variations occur suddenly, but they are very local in their manifestation. One particular shoot "sports," while all the rest remain in their normal condition. It is very different in the case of seedling varieties, where the whole system of branches and leaves is more or less affected.

Another and a most remarkable feature about these sports is, that they sometimes occur simultaneously in widely different localities; thus the same sport of a Chrysanthemum "turns up" about the same time, not only in different nurseries in this country, but also on the Continent. This may be because all the plants in question have originated from one and the same stock.

These, then, are the special characteristics of a true sport. Illustrations could be given by the hundred; but neither time nor space permit, nor, indeed, for our present purpose, is it requisite to do so. Whoever will investigate the cause of these sudden outbursts of local variation must, of course, sedulously examine each case for himself according to the measure of his ability and of his opportunity. The circumstances, the history, the progress, the anatomy of each particular sport must be investigated, both absolutely and in relation to similar outgrowths in other plants. Until this is done—and it has not been done yet—any explanation as to the cause of the phenomenon must be a matter of speculation. Still, we cannot help guessing, and though we may be wrong in our surmises, at least the process does good by setting us observing and thinking. Observing and thinking are processes valuable to all of us, but in a particular degree to those who practice the cultural arts. And so it happens—or, at least, we will hope so—that although the causes which have been assigned for these changes are various, some, perhaps, utterly wrong, others partially so, and all more or less inadequate to explain the whole of the phenomena, yet some advantage may accrue from the discussion. An indirect benefit is better than none at all, and anything which enforces us to take some measure of the extent of our own ignorance is likely to be beneficial. We should never be a bit the better if we simply acknowledged our ignorance, as, indeed, we needs must do in any case, but directly we attempt to find out in what particulars and in what degree we are ignorant, then there is some hope that some portion of our "nescience" may be dispelled. Under this impression we may allude to one or two of the assigned causes of sporting. External causes are those which the gardener most generally invokes. For him a sport is usually the consequence of some alteration in the nutrition of the plant. It gets too much or too little food, or the food is not of a suitable character—containing too much of one thing, too little of another, or the climate is charged with the results observed. It is very convenient to have the weather to blame; it may be too hot or too cold, too moist or too dry, too brilliant or too obscure; or the soil may be at fault, the drainage may be defective, the earth not sufficiently aërated, its temperature too high or too low. Combined action of some of these conditions is, of course, possible, intermittent action equally so, whilst we, in this country, are abundantly familiar, first with one thing in the way of the weather, and immediately afterward with another. It is, therefore, not surprising if some gardeners, without troubling themselves much to see how the explanation fits the facts, do attribute "sports" to such causes as we have mentioned. To our thinking, the objections to this kind of explanation are fatal. External circumstances are, no doubt, potent enough to effect very great changes indeed. We are daily witnesses of them; but they do not produce the kind of change which we know as "sports." On the contrary, sports occur some-

times when no alteration of external conditions is perceptible, and they do not occur when such alterations are very apparent. Or, again, they appear in one place under one set of circumstances, and at another place, simultaneously, under a different state of affairs; and although all the plants growing together have been exposed to the changed conditions of life, the sporting tendency shows itself in one particular plant only, and in one particular part of that plant, generally only in one bud. With all respect, then, for those who hold these views—and one at least of our most experienced and eminent plant-growers has lately publicly advocated them—we venture to think external causes, however adequate they may be in some cases, are inoperative in such cases as we are considering.

A better explanation is that offered by Darwin, by Naudin and others, according to which sports are due to a dissociation of mixed elements, a reversion to the character possessed by one or other of the ancestors of the plant, perhaps one or two, perhaps an indefinite number of generations ago. Let us recall for a moment what a very composite thing a plant is, even such a one as we call a simple plant. At first it is neutral and homogeneous, a mass of protoplasm, but the homogeneity of protoplasm is a thing of the past. We do not believe in it now. On the contrary, we believe in frameworks and interstitial fluid, in granules and fibres, in some parts that are alive, others that are dead; some that are stable and immutable, others that are mobile and changeable; in short, we have come to the conclusion that, physically and mechanically, as it was previously known to be chemically, protoplasm is very much "mixed."

Again, another of our old beliefs has been dissipated. Once we were taught that the cells of plants were closed bags without apertures, and that, while the fluid passed from cell to cell by osmosis, there were no visible pores, and no means of transmitting anything more solid than cell-sap. The passage of protoplasm from cell to cell was not then thought of as possible. But Mr. Walter Gardiner has changed all that. He and others who have followed in his steps have taught us how to see the pores in the cell-walls, how to see the passage of protoplasm through those pores from cell to cell, and how complacently to employ the phrase "continuity of protoplasm" in a manner that gives us, at present at least, great satisfaction. These modern discoveries of the composite nature of protoplasm, and of its passage, at certain times and under certain conditions, from cell to cell, seem to us to furnish a clue to the explanation of some of these cases of sporting, as they do also in the case of some of those curious cases in which the stock seems to influence the scion, or the scion the stock, in cases of grafting.

Again, in the life-history of a plant there are several stages. There is the neutral stage, when it is, at any rate, so far as sex is concerned, an epicene. Then there is the sperm stage, when our plant consists of a mass of neutral matter, a particular portion of which is developed into sperm-cells, or into what will ultimately produce them. At another time the neutral cells of one portion of the general plant-mass develop into germ or female cells, or it may happen that both sperm and germ cells may be developed at one and the same time, when the plant has, of course, a three-fold constitution.

All these modifications occur in the course of the life of each individual plant. But each individual plant is, necessarily, compounded of elements derived from its two parents, so that, for illustration sake, if we may consider the original stock to consist of three portions—neutral, male, and female, respectively—it is obvious that in the first generation there would be six component elements; in the second, twelve; in the third, twenty-four, and so on. Who can count the generations of plants? It is enough for our purpose if we succeed in showing clearly the composite nature of plants.

This being granted, it will not seem remarkable that occasionally a partial separation takes place, just as a scum may rise to the surface of some mixed fluid, or a sediment fall to the bottom of another. This illustration may, perhaps, serve to suggest the reason for the separation of mixed elements in plants; but that is too speculative a matter for us to enter upon here. It will be better for our present purpose to note one or two examples of dissociation of mixed characters wherein both the fact and its explanation are clear. One of the most interesting is that narrated by Mr. Noble, the originator of the white form of Jackman's Clematis. Noble's Clematis, as we may here shortly call it, is the result of a cross between Jackman's Clematis and *C. patens*. Soon after this Clematis was sent out, some dissatisfaction arose because, instead of producing flowers of good form and purity of colouring, more or less misshapen blooms of an unattractive appearance were formed. The matter was mysterious. The raiser was blamed by those who did not know that he is a highly competent man in his business, and one whose integrity is beyond question. The plant was condemned. Fortunately, however, the edict was not carried out in its entirety—some specimens were left. These were watched, and in due time afforded the explanation of the mystery. Jackman's Clematis flowers in the autumn on shoots formed during the spring and summer—on the new wood, as gardeners say, just as happens with a Rose. *Clematis patens* flowers in spring on shoots that were formed during the previous summer, on the "old wood," in gardening phrase. Now, when Noble's Clematis came to be scrutinized, it was found that it produced two kinds of flowers. Those which expand in spring are solitary, semi-double, never white, but bluish-gray, like those of *C. patens*. Those which unfold in autumn are produced in pairs and are single, like those of *C. Jackmanni*, but white. In the spring no flowers of the Jackman type are ever seen, and when the old wood is cut away, and only new wood thus suffered to produce flowers, no blooms of the *patens* character are seen, but only those of the Jackman type.

Another very interesting case of unmixing, or, if it be preferred, of partial mixture, is afforded by Neubert's Berberis. This is a hybrid between the evergreen pinnate-leaved Mahonia and the deciduous simple-leaved *Berberis vulgaris*, and it bears leaves some of which are intermediate in appearance, while others are like those of one or of the other of its parents.

The two illustrations above given are instances of the results of cross-fertilization, in which the whole process has, so to speak, taken place under our own eyes. But for how many centuries has the Chrysanthemum, we will say, been crossed and recrossed and crossed again? This process of crossing seems destined to come to an end, because the flowers, after a time, become sterile, owing to the fact that the stamens and pistils, one or both, are imperfectly or not at all developed. Seedling variations in such cases must become more and more rare as the process of sterilization becomes more and more marked. If new seedlings are desired, raisers will have to go back to less highly modified flowers—to flowers, that is, which are more nearly in their original condition. But although the production of varieties in the Chrysanthemum by fertilization is thus limited, the development of sports by bud-variation may, and probably will, still go on, to the delight of the grower and the interest of the student. It must, however, be said that at least in the case of the Chrysanthemum the change is sometimes very slight, depending solely on the presence of colouring matter in some cases and on its absence in others. The form of the flower and of the foliage in many of these Chrysanthemum sports is in no wise different from that of the parent plant. This is only an illustration of the fact that all degrees of combination or of dissociation, as the case may be, may be expected to occur.

Is there any commingling of the elements of stock and of scion in the case of grafts? Botanists and gardeners, almost without exception, have asserted that there is none. Place on a sheet of wet blotting-paper, which may represent the stock, a drier piece of the same substance, which may represent the graft, and there will be a passage of the fluid from the lower to the upper paper, but there will be no mixture of the constituents of the two.

We have always wondered, if there were no reciprocal influence of stock on scion, why grafting is practiced at all, because we cannot understand the acknowledged advantages of the practice except upon the supposition of some modification being exerted. Gardeners triumphantly, as they were quite justified in doing, pointed to the millions upon millions of cases where no such modifications are visible. Botanists pointed to the closed cells from whose cavities only the thinnest of liquids could exude and permeate through the walls of adjoining cells. This was before the days of "continuity of protoplasm," as above mentioned. Now that we know that not only water, but protoplasm itself, may, under certain circumstances, pass from cell to cell, the difficulties in the way of conceiving that any influence could be exerted on the scion by the stock, or *vice versâ*, are very materially lessened, if not entirely removed.

But before the time we speak of, there were some alleged facts which, provided the history given were true, could only be explained on the supposition of the commingling of elements by grafting and subsequent separation. In other words, the possibility of graft-hybridization must be assumed. Whether it has been proved is another matter.

One of the strongest cases in its favour that we know of is that of the famous Adams's Laburnum (*Cytisus Adami*). We cannot go into detail as to the history of this extraordinary tree. It must suffice to say, that it is stated to have originated from the implantation of a bud of the dwarf, shrubby, lilac-flowered *Cytisus purpureus* on to the common Laburnum. Be this as it may, we have in our gardens on this side of the Atlantic trees which every year astonish the beholder by producing, together with the foliage and flowers of the Laburnum, tufts of *Cytisus purpureus* and all sorts of intermediate conditions between the two. If the stock exerted no influence on the scion, the buds should be pure *Cytisus purpureus* and pure *C. Laburnum*, without any intermediate forms. It would lead me too far to give other illustrations of the production of shoots of an intermediate character between stock and scion. Many such are on record, and many have come under my own notice. It must suffice for me to show that whilst we may, with a very great amount of probability, attribute the existence of some sports to the "un-mixing" of elements blended by means of cross-fertilization, whether between species (hybrids) or between varieties (cross-breds), we may, likewise, but with a less degree of probability, attribute the existence of others to a similar dissociation in the case of grafted plants.

Obviously the latter cases must be much less numerous than the former, and are purely artificial productions, not likely to occur in Nature.

Other assigned causes appear to me to pertain rather to variation in general than to that limited, localized form of it which is here considered as bud-variation, and may be here passed with the mere mention.

MAXWELL T. MASTERS.

A NEW SCIENTIFIC SERIAL.[1]

THE imposing series of four octavo volumes before us is the embodiment of the first five years' work in the new Museum of the Austrian capital. Of the nature and plan of the building itself our readers have already been made aware; the collections housed within it are rich in types and specimens of priceless value, and its affairs are administered by a large and efficient staff of specialists, many of whom have attained a world-wide reputation. The directorship lies in the hands of Dr. Franz Ritter von Hauer.

Each of these volumes consists of four parts, and embraces one year's work. The parts are issued quarterly, their limitation in size being determined by the progress of work in hand. The first part of the first volume, issued early in the year 1886, is exclusively a "Jahresbericht" for the preceding year. It has already received notice in our pages (NATURE, vol. xxxiii. p. 424). While for the most part a report of work done, it contains information concerning the Museum itself, together with a list of names of the officers and staff, and of the various donors, correspondents, and persons who studied in the Museum during the year, as of those to whom material had been lent, together with references to published works in the production of which the resources of the Museum had been utilized. Of the remaining fifteen parts, each contains one or more special treatises, together with "notices" of a miscellaneous character, correspondence, personalia, and administrative detail, with acknowledgments of acquisitions. The four volumes make up a total of over 1900 pages of closely printed matter, with 80 plates and numerous woodcuts. The illustrations are, for the most part, highly satisfactory; we would, however, have preferred the substitution of ordinary lithographs for the photographs of Ophiurids described in vol. ii.; the latter are too indefinite and unsatisfactory. Excluding the notices and miscellanea, which monopolize collectively 22 per cent. (415 pp.) of the printed sheets, there remain 1532 pages of a more solid nature, which make up the bulk of the collective volumes. These bear, in all, 55 treatises; some of them, as our pages have already borne testimony (NATURE, vol. xxxv. p. 204), are lists of types and specimens in the Museum, others are elaborate monographs dealing with highly involved structural detail. The Museum is divided into five departments, each having its own working staff, and the published works bear the following ratio: zoology, 23; mineralogy with petrography, 13; geology with palæontology, 9; botany, 7; anthropology and ethnology, 3. As might be expected from this list, many new species of organic beings have been described. We find much to admire in some of the monographs; and especial attention is demanded by those devoted to the ethnology of the South Sea Islanders, by Dr. Otto Finsch, and to the artistic products of the Dyaks, by Prof. Alois Raimond Hein. These memoirs extend over the greater portion (240 pp.) of an average volume, and they are amply illustrated; the information contained in them is of inestimable value, the illustrations are of rare merit, and it would be difficult indeed to surpass the coloured representations of Papuan handiwork which adorn the pages of Dr. Finsch's important communication. These monographs are based upon the collections in the Vienna Museum, and upon perusal of them we know not upon which of their acquisitions most to congratulate our Austrian *confrères*—those of types of Nature's productions, or those of objects of human artifice. Moreover, the appearance of the memoirs cited, now that the South Sea Islanders are receiving renewed attention, is most timely; and their value is greatly increased by the fact that the peoples to whom they relate are becoming demoralized and demolished by the advance of "civilization."

The Museum whence these *Annalen* emanate was opened to the public in August 1889 by "His Apostolic Majesty the Emperor"; and an account of the ceremony, with its attendant honours, is to be found in vol. iv. The pages of the journal show the custodians of the institution to be fully alive to the value of their charge. The

[1] "Annalen der k.k. Naturhistorischen Hofmuseums, Wien." Bd. I.-IV. 1886-89. (Wien, Alf. Hölder.)

journal itself not only serves them as a catalogue, but as a medium for publication of investigations into structure, such as the officers of our own National Museum are in the habit of contributing to the Proceedings of our Learned Societies and to other private journals. The authorities of the Austrian Museum might, at first sight, appear to be ahead of us in the possession of their recognized official *Annalen;* and there are those among us who would desire the founding of a similar official journal with its attendant restrictions for our own National Museum. We are very doubtful of the advisability of such a step, supposing the trustees were willing to undertake it. As matters stand, the excellent official catalogues which emanate from the building in Cromwell Road fulfil the public demands, and suffice for all purposes of nomenclature which it is a leading function of its authorities to control. The supplementary work, with the publication of which the members of its staff have so long honoured outside bodies, is voluntary. The progress of science in Britain is unique in the extraordinary degree to which it has been furthered by private enterprise; in contributing to the work of our Learned Societies and of those self-supporting institutions to which we have alluded, our Museum officials are encouraging an essentially national system, and fostering a love of science for its own sake. For these if for no other reasons, we would not desire the extension of the Austrian system to our own land.

We cannot close this notice without commenting upon the growing desire to found journals in connection with departments of our native Universities and Colleges. From what we have said, we could hardly be expected to approve of this movement, especially as the interests of such journals are apt to centre in individual aggrandizement, and as the necessity for their continuity may lead to the publication of that which the literature of the sciences might well be spared. We have journals ample for our needs, provided sufficient care be exercised in the selection of their contents. Better far to improve and to extend these, than to tolerate that which in them may be least desirable, adding thereto a "literature" which can only ill compare with that of the last generation of British naturalists.

We note that the Viennese have as yet succeeded in effecting an interchange of publications with but few of our leading Societies, and that their *Annalen* are not yet to be found in a large number of our University and other leading libraries. With respect to this, comparison with foreign countries does not redound to our credit. We can strongly recommend the journal on its merits; and, if the standard of its early volumes be maintained, no working scientific library will be ere long complete without it.

G. B. H.

NOTES.

The programme for the Leeds meeting of the British Association has been issued. The first general meeting will be held on Wednesday, September 3, at 8 p.m., when Prof. W. H. Flower will resign the chair, and Sir Frederick Abel, President-Elect, will assume the Presidency and deliver an address. On Thursday evening, September 4, at 8 p.m., there will be a *soirée;* on Friday evening, September 5, at 8.30 p.m., a discourse on "Mimicry," by Mr. E. B. Poulton, F.R.S.; on Monday evening, September 8, at 8.30 p.m., a discourse on "Quartz Fibres and their Applications," by Prof. C. Vernon Boys, F.R.S.; on Tuesday evening, September 9, at 8 p.m. a *soirée;* and on Wednesday, September 10, the concluding general meeting will be held at 2.30 p.m. The Vice-Presidents are the Duke of Devonshire, the Marquis of Ripon, the Earl Fitzwilliam, the Lord Bishop of Ripon, Sir Lyon Playfair, the Right Hon. W. L. Jackson, M.P., the Mayor of Leeds, Sir James Kitson, and Sir Andrew Fairbairn. The following are the Presidents of the various Sections:—A.—Mathematical and Physical Science, Mr. J. W. L. Glaisher, F.R.S. B.—Chemical Science, Prof. T. E. Thorpe, F.R.S. C.—Geology, Prof. A. H. Green, F.R.S. D.—Biology, Prof. A. Milnes Marshall, F.R.S. E.—Geography, Lieut.-Colonel Sir R. Lambert Playfair. F.—Economic Science and Statistics, Prof. Alfred Marshall. G.—Mechanical Science, Captain A. Noble, F.R.S. H.—Anthropology, Mr. John Evans, V.P.R.S. The local secretaries are Mr. J. Rawlinson Ford, Mr. Sydney Lupton, Prof. L. C. Miall, and Prof. A. Smithells, and the local treasurer, Mr. E. Beckett Faber.

The annual meeting for the election of Fellows of the Royal Society was held at the Society's rooms in Burlington House, on June 5, when the following gentlemen were elected:—Sir Benjamin Baker, Robert Holford Macdowall Bosanquet, Samuel Hawkesley Burbury, Walter Gardiner, John Kerr, LL.D., Arthur Sheridan Lea, D.Sc., Major Percy Alexander MacMahon, R.A., Rev. Alfred Merle Norman, Prof. William Henry Perkin, Prof. Spencer Umfreville Pickering, Isaac Roberts, David Sharp, M.B., J. J. Harris Teall, Richard Thorne Thorne, M.B., Walter Frank Raphael Weldon.

Last Saturday the Royal Observatory was inspected by the Board of Visitors. By invitation of Sir G. G. Stokes, the chairman, about 250 ladies and gentlemen interested in astronomy attended to see the instruments and methods employed in the Observatory.

In the House of Commons, on Tuesday, Mr. A. Acland moved that the sum of £350,000, which the Government propose to use for the extinction of the licenses of public-houses, should be applied in England for the encouragement of agricultural, commercial, and technical instruction, and in Wales for like objects. This ingenious scheme did not commend itself to the Chancellor of the Exchequer. The Government, he said, "admired the enthusiasm of the hon. gentleman, but could not assent to his proposal."

It is announced that the Committee of Council on Education have decided, with the sanction of the Treasury, to allocate a fixed sum every year, in the vote for the Science and Art Department, for grants in aid of technical instruction given under the Technical Instruction Act. The sum allocated for the financial year 1891-92 will be £5000. A grant in aid will not necessarily be equal to, and in no case will it exceed, the amount contributed by the local authority out of the rates. Each grant will be computed, as far as possible, on the basis of the amount of the rate spent on subjects of technical instruction other than those for which the Science and Art Department gives aid under the Science and Art Directory. The application from the local authority, which must be sent in before the end of April in each year, should therefore give a certified statement, with the necessary extracts from the accounts of the preceding year, showing how the rate raised has been expended, and especially how any portion may have been applied to instruction in subjects for which grants are not made under the Science and Art Directory.

In the course of the discussion on Mr. Acland's proposal, Mr. Mundella commented severely on the fact that the sum to be allocated under the Technical Instruction Act for the financial year 1891-92 would be only £5000. There was not a canton in Switzerland, he declared, that would not be ashamed of such a paltry provision for technical education. Mr. Goschen replied that he had himself been struck by the smallness of the sum, "but it was the result of the comparatively small demand made by the local authorities. There was every disposition on the part of the Government to meet to the full the requirements under the Act."

THE Science and Art Department announces that it will make grants for the encouragement of instruction in drawing and of manual training in classes connected with elementary schools and in organized science schools. The instruction must be (*a*) in the use of the ordinary tools used in handicrafts in wood or iron; (*b*) given out of school hours in a properly fitted workshop; and (*c*) connected with the instruction in drawing—that is to say, the work must be from drawings to scale previously made by the students. The instruction may be given by one of the regular teachers of the school if he is sufficiently qualified; if not, he must be assisted by a skilled artisan. The work of the class will be examined by the local inspector of the Department, accompanied, if necessary, by an artisan expert, on the occasion of his visit to examine in drawing. If it appears that the school is properly provided with plant for instruction, and that the teaching is fairly good, a grant of 6*s*., or, if excellent, of 7*s*., will be made for every scholar instructed, provided (*a*) that he has passed the fourth standard; (*b*) that he has received manual instruction for at least two hours a week for twenty-two weeks during the school year; (*c*) that a special register of attendance is kept; and (*d*) that each scholar on whom payment is claimed is a scholar of the day school and has attended with reasonable regularity. The grant may be reduced or wholly withheld at the discretion of the Department, if it appears that the plant is insufficient or that the instruction is not good.

AUSTRALIAN educational legislators appear to be reconsidering the policy of the payment by results system, and in some instances, at least, to have come to the conclusion that it must be abolished. The Minister of Education in Victoria is said to have a measure drafted with the object of substituting fixed salaries for school teachers for the system of payment by results.

AT the annual general meeting of corporate members of the Institution of Civil Engineers, on June 3, it was pointed out in the Report that the meeting was held on the sixty-second anniversary of the incorporation of the Institution by Royal charter. At that time the number of members was 156, and the gross annual receipts were £447. At the close of the past financial year the number of members was 5872, and the gross receipts for the twelve months amounted to £22,478. This increase—thirty-seven-fold in numbers and fifty-fold in revenue—sufficiently indicated the position which the Institution had taken in connection with the profession it was designed to promote. At the same time the members were reminded that a large rate of increase was by no means desirable.

AT the beginning of May it was found at Howietoun that the supply of water from Loch Coulter had been interfered with on ten successive nights. On an examination being made each morning, a number of eels were discovered in the sluice; where the water is 10 feet deep. Thirty altogether were obtained, all of them proving to be females. One of these, 32 inches in length, and weighing about 2 pounds, was examined. The ovary, which was about 12 inches long *in situ*, and about 30 inches long when unravelled, was calculated to contain 10,077,000 eggs in various stages of development, some, 0·25 mm. in diameter, being nearly ripe. There is little doubt that these eels formed part of a band migrating to the sea (the smaller specimens escaping and the larger being caught); and judging from the condition of the ovary, it would appear that they were impelled by the instinct of reproduction.

THE Medical Section of the French Association for the Advancement of Science proposes to discuss thoroughly, at the approaching meeting at Limoges, the various questions relating to influenza.

THE visit of the Iron and Steel Institute to the United States in the autumn is likely to be in every way most successful. There will be three different sets of meetings—the meetings of the American Institute of Mining Engineers, which take place in New York on September 29 and 30; the meetings of the Iron and Steel Institute, which take place in the same city on October 1, 2, and 3; and the international meeting promoted jointly by those two Societies, which will take place about the middle of October at Pittsburg. The excursions which have been planned by the American Reception Committee, of which Mr. Andrew Carnegie is chairman, provide for about 3000 miles of free transportation through the United States. The principal excursions will take place to the iron ore and copper regions of Lake Superior, to Philadelphia, Harrisburg, and Chicago, where there are large iron and steel engineering works to be inspected, and to the new iron-making district of Alabama. About 300 members of the Iron and Steel Institute and 100 German ironmasters have intimated their intention of taking part in the meetings; and already many have booked passages in the Hamburg-American Company's steamer *Normannia*, leaving Southampton on September 12. The meetings and excursions will last altogether over a month, and will practically embrace every point of interest in the United States within a distance of 1500 miles of New York. Papers have been promised for the meetings by Sir Lowthian Bell, Sir Nathaniel Barnaby, Sir Henry Roscoe, and others. Among those who have intimated their intention of being present at the meetings are Sir James Kitson (President of the Institute), Lord Edward Cavendish, Sir John Alleyne, Sir James Bain, Mr. Hingley, M.P. (President of the Iron Trade Association), Mr. Theodore Fry, M.P., Sir J. J. Jenkins, Sir Thomas Story, Mr. Windsor Richards, Mr. Snelus, F.R.S., and Mr. Edward P. Martin.

IN the House of Commons, on Monday, Mr. Norris and Sir Henry Roscoe put questions to Mr. Chaplin with regard to the change made by him in "the muzzling order." Mr. Chaplin explained that the collar had been substituted for the muzzle only in those districts in which rabies had, it was believed, ceased to exist. The number of cases of rabies during last year was 340. The muzzling order had never at any time been extended to the whole kingdom, and there were no statistics to show what the effect of the order would be if it were made universal. From the progress made already, he anticipated that rabies might be effectually dealt with without any necessity for so stringent a measure.

IN January of the present year two samples of compressed or tablet tea were presented to the Museum of the Royal Gardens, Kew, by Colonel Alexander Moncrieff. In the new number of the *Kew Bulletin* the letter with which these samples were accompanied is printed; and much interesting information as to the making of compressed tea is brought together. Repeated attempts have been made to introduce compressed tea into this country, but never with complete success. "A few years ago," says the *Kew Bulletin*, "two companies were formed for working it, and at the present time there is a company in London which deals exclusively in this article, a sample of which is in the Kew Museums. It is claimed for this tea that it has many advantages over loose tea, the chief of which is that the leaves being submitted to heavy hydraulic pressure all the cells are broken, and the constituents of the leaf more easily extracted by the boiling water, thus effecting a considerable saving in the quantity required for use. Its great advantages over loose tea, however, would seem to be its more portable character, and in the case of long sea voyages, or for use in expeditions, the reduction of its bulk to one-third. The compression of tea into blocks further, it is said, constitutes a real and important improvement in the treatment of tea. These blocks weigh a quarter of a pound each, and are subdivided into ounces, half ounces, and quarter ounces; this insures exactitude in measuring, and saves the trouble, waste, and uncertainty of measuring by spoonfuls. It

also insures uniformity in the strength of the infusion. By compression it is claimed that the aromatic properties of the leaf are retained for a much longer period, and that it is better preserved from damp and climatic changes."

Besides the paper on compressed tea, the *Kew Bulletin* for June contains a valuable catalogue of timber trees of the Straits Settlements. Among the late Dr. Maingay's botanical collections—which were acquired for Kew—was a herbarium of the woody plants of the Eastern Indian peninsula, a large proportion of which were new to science. These were accompanied with a series of careful note-books containing descriptions drawn up from fresh specimens, with the native names. The whole material has been worked up at Kew in the preparation by Sir Joseph Hooker of the "Flora of British India," and has proved, the *Bulletin* says, "of inestimable value." In the list now printed botanical identifications are given to the native names comprised in Dr. Maingay's catalogue. In the same number of the *Kew Bulletin* there is an interesting correspondence, in which attention is drawn to the growing of cotton in West Africa, and especially to an attempt which has lately been made to introduce and cultivate experimentally in that region the best forms of Egyptian cotton.

The *Manchester Guardian* says that many students of science in Lancashire will learn with satisfaction that the Council of the Manchester Literary and Philosophical Society have at last been able to make arrangements for the cataloguing of the Society's unique library. This includes, amongst much other rare and valuable material, the publications for a long series of years past of several hundred foreign Academies and learned Societies.

The "Association pour la Protection des Plantes" held an interesting exhibition at Montpellier during the recent centennial celebration. This Society, which is now seven years old, aims at the protection of Alpine plants, especially in Switzerland, where many species have been all but destroyed by the depredations of plant-dealers. Among its members are many well-known English men of science. It is doing good work in establishing Alpine botanical gardens, where rare species are preserved.

A botanical school-garden has recently been instituted in Breslau by the magistracy, for regular supply of plants to the schools of the place, and for enabling teachers to make observations on the spot with their pupils. The cost of the arrangement is about £300. Private schools share the advantages on payment of an annual subscription.

German papers announce the death of Dr. Anton Felix Schneider, Professor of Zoology and Director of the Zoological Museum at the University of Breslau.

The measurement of the Rhone glacier in a comprehensive and systematic way has been carried on since 1874 by the Swiss Alpine Club, and the abundant data obtained will shortly be published in separate form. It appears that the glacier was in recession till 1888, but since last year it has been advancing.

Two violent shocks of earthquake were felt at Sofia on June 7, at half-past 6 a.m. The seismic disturbance was accompanied by subterranean noises. Its direction was from south to north. No damage seems to have been done.

A large water-barometer is now in use in the Saint Jacques Tower, Paris. The glass tube—the longest that has yet been made—is 12 metres 69 centimetres long. The diameter is 2 centimetres. Special openings in the tower were required to allow it to be put in its place. It is connected with a registering apparatus, and it is proposed that a photographic apparatus shall be associated with it, in order that the thermometrical readings of the water in the barometer may also be obtained. The instrument is a very curious one, and may render many services in consequence of its considerable sensitiveness. During thunderstorms it is especially active.

Mr. R. H. Scott has contributed a note on thunderstorms to *Longman's Magazine* for June, showing various peculiarities in their behaviour in this country and abroad. These storms are generally divided into two groups: (1) heat thunderstorms (the summer type), and (2) cyclonic thunderstorms, which occur principally in autumn and winter. The frequency of the storms is much greater in low latitudes than in high, and their energy is materially moderated by the dampness of the climate, hence our own comparative immunity from them. Certain districts also appear more liable to storms than others; the damage by hail, which frequently accompanies electrical discharges, appears to be greater in Huntingdonshire and neighbouring counties than in other parts of England. From an extensive inquiry by the Berlin Statistical Office, published in 1866, it appears that houses with thatched roofs are struck by lightning much more frequently than slated houses, while houses in towns are less frequently affected than those in the country.

Föhn winds, it is now known, are due to the descent from a mountain region of locally heated air-currents, when minima are passing. The föhn phenomena of Greenland have been lately studied by Herren Paulsen and Hann (*Met. Zeitsch.*). Over the ice-covered interior in winter (they represent) lies a barometric maximum, and before minima approach from the west, the phenomenon of increase of temperature with height occurs, as in the Alps. The masses of air on the plateau, cooled by radiation, sink as local cold valley winds, by the fjords. But when an approaching depression from the west sets the air in more general motion, the milder air from higher portions of the anticyclone comes down into the fjords as a warm east wind—the föhn. The movement extends as the minimum comes nearer, and the warming effect is not confined to the föhn localities. On one side of the mountain precipitation occurs, causing diminished cooling of the rising air, and thereby a continuance of the föhn on the other side.

The new number of the Journal of the Bombay Natural History Society (vol. v. No. 1) contains a valuable paper, by Mr. G. W. Vidal, on the venomous snakes of North Kanara. It has been said that no case of the bite of the *Echis* having proved fatal is known. Mr. Vidal thinks that at the present day this statement can hardly need refutation. There is no doubt, he says, that the *Echis* is a far more potent factor in swelling the mortality of the Bombay Presidency than any other venomous species, and it seems to him important that this fact should be more generally known and recognized than it has been hitherto. In all those districts—such as Sind and Ratnagira—where the *Echis* is known to abound, the average mortality from snake-bite is markedly high, whereas the mortality is insignificant in districts where the *Echis* is either rare or absent.

At the meeting of the Linnean Society of New South Wales on April 30, Mr. R. Etheridge read a paper on the question, "Has man a geological history in Australia?" The general want of satisfactory evidence of man's existence in Australia during Post-Tertiary times was commented on, and the various opinions which have been given on the subject were passed in review. A portion of the human tooth found in the Wellington Breccia Cave by the late Mr. Gerard Krefft was described, and the question of its value as evidence, from what is known of its history, was discussed. After considering all the evidence at present forthcoming, the author arrived at the conclusion that the matter could hardly be summed up better than by the very

reasonable and often correctly applied Scotch verdict of "not proven."

IN the annual address, delivered lately by Colonel J. Waterhouse, President of the Asiatic Society of Bengal, and now printed, he speaks highly of the work done by Indian museums and kindred institutions. He says they are exerting "a great educational influence" on "the teeming masses" of India. Native visitors are beginning "to take a really intelligent interest in the collections." Colonel Waterhouse urges that the work of local museums should be confined to the illustration of local products. If objects from other districts are admitted, the name of their place of origin should, he thinks, be distinctly marked upon them, and they should be kept apart from the local collections.

THE U.S. Department of Agriculture has issued an elaborate Report on the English sparrow (*Passer domesticus*) in North America. The Report has been prepared, under the direction of Dr. C. Hart Merriam, ornithologist to the Department, by Mr. Walter B. Barrows, assistant ornithologist. Dr. Merriam claims that it is "the most systematic, comprehensive, and important treatise ever published upon the economic relations of any bird." The new immigrant into the United States is accused of an enormous number of offences; and no one who studies the evidence brought together in this Report will be disposed to say that his evil deeds have been exaggerated. The climatic and other conditions of America have suited the sparrow to perfection, and he has exercised freely all his powers of doing mischief. The evidence set forth relates to the importation, spread, increase, and checks on the increase of the bird; the injury done by him to birds, blossoms, and foliage; the injury to fruits, garden-seeds, and vegetables; the injury to grain; and the relations of the sparrow to other birds, and to insects. All sorts of suggestions for the destruction or abatement of the nuisance are carefully considered. There is also interesting evidence as to the sparrow in Europe and Australia.

A PAPER upon the atomic weight of magnesium and the properties of the pure metal obtained by distillation *in vacuo* is communicated to the current number of the *American Chemical Journal* by Messrs. Burton and Vorce, of Cleveland, U.S. When an attempt is made to distil magnesium in an ordinary hard potash glass tube it is found that the vapour of the metal attacks the glass in a remarkable manner, a black voluminous substance being formed which evolves a spontaneously inflammable gas on treatment with an acid. This black substance is, in fact, magnesium silicide, Mg_2Si, and the explosive gas silicon tetrahydride, SiH_4. When the silicide is brought in contact with dilute acid there remains, after the liberation of silicon hydride and conversion of the magnesium into a salt of the acid employed, a quantity of a yellow substance which possesses the properties of the lower oxide of silicon described by Mabery. Hence it is not possible to use tubes entirely of glass for the distillation of magnesium. But by lining the interior of the heated portion of the tube with an inner tube of thin sheet-iron, magnesium not alloying with iron, the distillation can be conducted with perfect safety. The magnesium was packed in the iron tube in the form of small pieces of ribbon, and the iron tube then placed in an outer glass tube closed at one end and about twice the length of the iron tube. The other end was afterwards drawn out and connected with a Sprengel pump, and the tube exhausted. The apparatus was then laid in a combustion furnace and the tube heated, the closed end near which the iron tube and its magnesium contents had been placed being heated much more strongly than the end nearest the pump. When the iron tube became heated to bright redness the magnesium commenced to volatilize and sublime into the relatively cooler portion, forming at first a black mirror of silicide upon the glass, which protected t from further corrosion. After continuing the heating for about an hour in the case of the distillation of about ten grams of metal, the gas was shut off, and the whole allowed to cool very slowly so as to prevent fracture of the glass, the vacuum being maintained as perfect as possible until quite cold. The distilled magnesium was similarly redistilled three times, the product of the fourth distillation alone, in which no traces of impurities could be detected by analysis, being employed in the atomic weight determinations. The magnesium was generally deposited in the form of a thin crystalline bar of pure white metal which readily separated from the coating of silicide, but in certain of the distillations beautiful isolated crystals of considerable size were formed. Weighed portions of the metal thus purified were converted to the nitrate by means of purified nitric acid diluted with water also specially purified and recently redistilled in a platinum apparatus. The nitrate was then ignited to oxide, first over a sand-bath, and finally to constant weight at the highest temperature of a muffle furnace. From the relation between the weights of metal taken and oxide produced in ten experiments, the mean value of the atomic weight of magnesium if $O = 16$ was found to be $24{\cdot}287$; if $O = 15{\cdot}95$, $Mg = 24{\cdot}211$. The highest value found when $O = 16$ was $24{\cdot}304$, and the lowest $24{\cdot}271$. The crystals of magnesium obtained during the distillation were very perfect hexagonal prisms showing no planes but those of the primary prism ∞P, primary pyramid P, and basal plane oP. From measurements of the angles the axial ratio $a : c = 1 : 1{\cdot}6202$, which agrees tolerably well with the ratio given by Des Cloizeaux from the measurement of crystals obtained by Dumas in 1880. Magnesium is therefore isomorphous with zinc and beryllium, which latter metal it very closely resembles in its angular measurements and the ratio of its axes. In case of Zn, $a : c = 1 : 1{\cdot}3564$, and for beryllium, $a : c = 1 : 1{\cdot}5802$.

THE additions to the Zoological Society's Gardens during the past week include two Oak Dormice (*Myoxus dryas*), Central European, presented by Lieut.-Colonel G. M. Cardew; a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented by Mrs. Waterson; a Silver-backed Fox (*Canis chama* ♂) from South Africa, presented by Captain H. D. Travers, R.M.S. *Tartar*; a Great Kangaroo (*Macropus giganteus* ♀) from Australia, presented by Mr. Henry Irving, F.Z.S.; a Ring-necked Parrakeet (*Palæornis torquatus* ♂) from India, presented by Mr. Arthur O. Cooke; a West African Love Bird (*Agapornis pullaria*) from West Africa, presented by Mrs. Fell; a Chinese Bulbul (*Pycnonotus sinensis*) from China, presented by Lieut.-General Sir H. B. Lumsden, K.C.S.I., F.Z.S.; three Common Peafowl (*Pavo cristatus* ♂ ♀ et juv.) from India presented by Mrs. Francis Leighton; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. C. Ashdown, F.Z.S.; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Miss Beatrice Fort; a Grey Monitor (*Varanus griseus*) from the Sahara Desert, presented by Dr. John Murray; a Hawk-headed Parrot (*Deroptyus accipitrinus*) from Brazil, deposited; a Vociferous Sea Eagle (*Haliaëtus vocifer*) from West Africa, a Red-crowned Pigeon (*Erythrœnas pulcherrima*) from the Seychelles, purchased; a Japanese Deer (*Cervus sika* ♂), two Bennett's Wallabys (*Halmaturus bennetti* ♂ ♂), a Vulpine Phalanger (*Phalangista vulpina* ♂), a Peacock Pheasant (*Polyplectron chinquis*), a Swinhoe's Pheasant (*Euplocamus swinhoii*), four Spanish Blue Magpies (*Cyanopolius cooki*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on June 12 = 15h. 24m. 22s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4087	—	White.	15 12 57	+56 43
(2) G.C. 4234	—	Bluish.	16 39 51	+24 0
(3) χ Serpentis	4	Reddish-yellow.	15 43 48	+18 29
(4) δ Boötis	3	Yellow.	15 11 6	+33 44
(5) α Coronæ	2	Bluish-white.	15 30 0	+27 5
(6) W Cygni	Var.	Reddish.	21 31 53	+44 53

Remarks.

(1) This is a long white nebula in Draco which was described by Sir John Herschel as "a superb ray nebula." The G.C. description is: "Considerably bright; very large; very much extended in the direction 155°; at first very gradually, then pretty suddenly brighter in the middle, where there is a nucleus." In Herschel's 20-foot reflector it was seen to be 7½′ long. The spectrum of the nebula has not been recorded.

(2) This is one of the planetary nebulæ, and according to Dr. Huggins its spectrum shows the three bright lines usually seen in nebulæ. He also noted that F was the faintest line, and that there was a faint continuous spectrum. The spectrum was re-observed by Vogel in 1872, and he observed two additional lines near wave-lengths 518 and 554. It is important that these lines should be confirmed, and comparisons made with the flutings of carbon and manganese at 517 and 558 respectively. The existence of these lines will further tend to prove the connection between comets and nebulæ, for two bands in these positions have frequently been observed in cometary spectra. It is not improbable that a third cometary band, near λ 468, may also appear in the nebula, as a line near that position (λ 470) has been recorded by Dr. Copeland and Mr. Taylor in other nebulæ. Unfortunately, a rather large aperture is required for this observation; with a 10-inch refractor I have not been able to more than glimpse the additional lines seen by Vogel. The G.C. description of the nebula is: "A planetary nebula; very bright; very small; round; disk and border." It is not advisable to employ a cylindrical lens in searching for faint lines, even though the nebula is a small one.

(3) Vogel describes this star as a fine one of Group II., but Dunér states that the bands are narrow, 4 and 5 being little more than lines. He also notes that the spectrum approaches Class II.*a* (Group III.). It is therefore probable that the spectrum is an intermediate one, and will show some of the lines characteristic of Group III. Any differences in these lines, either in positions or relative intensities, from those seen in stars like the sun, should be noted, as they will form valuable criteria for the subdivision of the Class II.*a* stars of Vogel into two groups—one of increasing temperatures (Group III.), and the other decreasing (Group V.).

(4 and 5) The first of these has a spectrum of the solar type, and the second one of Group IV. (Gothard). The usual observations are required in each case.

(6) The range of this variable is very small—5·8–6·2 at maximum to 6·7–7·3 at minimum—and it will be interesting to observe if any changes in spectrum take place at maximum similar to those which occur in stars of greater range with the same type of spectrum. The general spectrum is a "very fine" one of Group II., but so far no variations with change of magnitude have been noted. The period is given by Gore as 120–138 days, and there will be a maximum about June 21.

The Spectrum of Comet Brooks (*a* 1890).—I made further observations of this comet on June 6 and 7, and found that it had become considerably brighter since my last observation (Nature, vol. xlii. p. 112). The tail was also slightly extended. The principal spectroscopic change noted was a diminution in the brightness of the continuous spectrum relatively to the carbon flutings, making the latter more distinct. There was no change in the positions of the bands, and as the comet has now passed perihelion, it is not likely that it will go through any of the higher-temperature stages. As its distance from the sun increases, it should be observed for the cooler stages. The first decided change, according to Mr. Lockyer's investigations, should be the replacing of the present "hot carbon" spectrum for that of "cool carbon," the criterion for which is a fluting near λ 483. This, again, should be replaced by a spectrum consisting mainly of a line in the position of the chief nebula line (λ 500).

In connection with the observations of the comet, I have also made observations of the spectrum of the nebula G.C. 4058 (see notes for June 5). I found that the spectrum of the nebula was irregularly continuous, with a very decided maximum of brightness coincident with the carbon fluting near λ 517. There were also other brightnesses, the positions of which are not yet determined. The whole spectrum is strikingly similar to that of the comet, and as the two objects are not far removed from each other, this is a good opportunity for observers to satisfy themselves that comets and nebulæ are intimately connected.

A. Fowler.

The Planet Uranus.—M. Perrotin, of Nice Observatory, has made some observations of dusky bands on Uranus, similar to those that are seen on Jupiter (*Vierteljahrsschrift des Astronomische Gesellschaft*). The following are some values found for the position-angle:—

1889 31 May	... ...	13°
,,	... ...	35
1 June	... ...	20
7 ,,	... ...	30

The mean value is 24°·5, or about 10° from the plane of the orbit of the satellites, from which it would appear that the plane of the Uranian equator differs little from the trend of the satellites. M. Perrotin also found that the direction of the bands, according to repeated measures, coincided with the longest diameter. The bands do not appear always to have the same aspect, but vary in number and in size in different parts of the surface. This unequal distribution will, it is hoped, afford a means of accurately determining the time of rotation. The oblateness deduced from the measures is said to be not less than $\frac{1}{20}$.

Mr. Tebbutt's Observatory.—We have received the Report of this Observatory for the year 1889. A considerable amount of extra-meridian work has been done during the year, observations having been made of some minor planets, phenomena of Jupiter's satellites, and occultations of stars by the moon. Barnard's comet (*a* 1889) and Davidson's comet (*d* 1889) were observed on eight occasions, and Brooks's comet (*e* 1889) on two occasions. The comparison observations that were made have been reduced, and sent to *Astronomische Nachrichten*. Brorsen's periodical comet was carefully searched for, with the help of Dr. Lamp's ephemeris, on December 21 and 25, 1889, and again on January 18, 20, and 22, but without success. Comparisons have been made, both of η Argûs and R Carinæ, with the neighbouring stars, and it is noted that the former star has not sensibly varied in its lustre since the announcement of its sudden increase of magnitude between April 1887 and May 1888. A satisfactory determination of a maximum of the latter star was made in June 1889, and its period determined as 312 days.

New Asteroid.—A minor planet of the 13th magnitude was discovered by M. Charlois at Nice on May 20. This brings the number up to (293).

CORAL REEFS AND OTHER CARBONATE OF LIME FORMATIONS IN MODERN SEAS.[1]

The vast organic accumulations known as coral reefs are, undoubtedly, among the most striking phenomena of tropical oceanic waters. The picturesque beauty of coral atolls and barrier reefs, with their shallow placid lagoons, and their wonderful submarine zoological and botanical gardens, fixed at once the attention of the early voyagers into the seas of equatorial regions of the ocean. Questions connected with the peculiar form, the structure, the origin, and the distribution of these great natural productions have, from the very outset, puzzled and interested all those who delight in the study of natural things. In this communication we propose to point out and discuss some of the more general phenomena of oceanic deposits, with special reference to the functions of corals and other lime-secreting organisms, and the accumulation of their dead shells and skeletons on the floor of the great oceans.

Coral reefs are developed in greatest perfection in those ocean waters where the temperature is highest and the annual range is least. It may be said that reefs are never met with where the temperature of the surface water, at any time of the year, sinks below 70° F., and where the annual range of temperature is greater than 12° F. Bermuda, which is the coral island the farthest removed from the equator (lat. 32° N.), and one or two other outlying reefs, may be, in a sense, exceptions to this

[1] Paper read on December 2, 1889, before the Royal Society of Edinburgh, by John Murray, LL.D., Ph.D., and Robert Irvine, F.C.S.

statement, for in these exceptional cases the temperature of the ocean water appears occasionally to fall to 66° or 64° F., and there is a wider annual range than 12° F. This condition of high temperature with small range in the temperature of the water is only to be met with in the middle and western portions of the Atlantic and Pacific Oceans and the central parts of the Indian Ocean; consequently, coral reefs flourish along the eastern shores of the continents, where the coasts are bathed by currents of pure oceanic water coming directly from the open sea; while, on the other hand, they are absent along the western shores of the continents, where the water is colder and the annual range is very much greater—for instance, off the western coasts of America and Africa. The *Challenger* observations have also shown that the layers of warm surface waters are much thicker towards the western parts of the great oceans; consequently, reef-forming organisms flourish at a greater depth along the eastern shores of the continents than in positions further to the eastward in the open ocean, where the warm layer of water—over 70° F.—is much thinner. Throughout the temperate and polar regions there are no coral reefs. This is all the more remarkable, seeing that organisms belonging to the same orders, families, and even genera as those which build up coral reefs flourish throughout colder, and even in polar, seas. In these colder seas the representatives of the reef-builders either do not secrete carbonate of lime in their body-walls, or, if they do so, the shells or skeletons are much less massive than in tropical waters. An attentive examination of the animals procured by the dredge and trawl from all depths shows that in descending into deeper water in equatorial regions the amount of carbonate of lime secreted by the animals living on the sea bottom becomes less with increasing depth, and all the calcareous structures of the organisms become less massive with the descent into the deeper and colder water of the abysmal regions. This remark does not, of course, apply to the shells and skeletons of surface organisms which have fallen to the bottom from the surface waters.

Still another illustration of the same fact is furnished by the study of the pelagic organisms collected in the surface and subsurface waters by means of the tow-nets. In the warmest tropical waters there are numerous species of Pteropoda, Heteropoda, Gasteropoda, Foraminifera, and Coccospheres and Rhabdospheres (calcareous Algæ), which lead a purely pelagic existence, and secrete carbonate of lime shells. Mr. Murray estimates from his tow-net experiments that at least 15 tons of carbonate of lime exists in this form at any moment of time in a mass of tropical oceanic water 1 square mile in extent by 100 fathoms in depth.[1] The number of species and individuals of these lime-secreting organisms decreases and the shells become less massive with a wider removal from the equator and an approach to the colder water of the poles, till we find in the surface waters of the polar regions only one or two thin-shelled Pteropods, and one, or at most two, dwarfed species of pelagic Foraminifera. It would appear then that organisms, as a whole or individually, are able to, and actually do, secrete more lime in regions where there is a uniformly high temperature of the ocean water than in those regions where there are great seasonal fluctuations of temperature, or where there is a uniformly low temperature of the water, as in the polar regions and in the deep sea. In temperate seas more carbonate of lime is secreted in the warm summer months than during winter months. Indeed, a high temperature of the sea water is more favourable to abundant secretion of carbonate of lime than high salinity.

An examination of the deep-sea deposits collected by the *Challenger* and other expeditions in all oceans shows that, after the death of the pelagic organisms above referred to, their calcareous shells are rained down on the ocean's bed, and there make up the larger part of the deposits known as Pteropod and Globigerina oozes, as well as a very considerable part of nearly all other marine deposits. If we take the samples of deep-sea deposits collected by the *Challenger* as a guide, then the average percentage of carbonate of lime in the whole of the deposits covering the floor of the ocean is 36·83, and of this carbonate of lime, it is estimated that fully 90 per cent. is derived from the remains of pelagic organisms that have fallen from the surface waters, the remainder of the carbonate of lime having been secreted by organisms that live on or attached to the bottom. If coral muds and sands, together with Pteropod and Globigerina oozes, be considered, then it is estimated that these contain an average percentage of 76·44 of carbonate of lime, and cover about 51,859,400 square miles of the sea bottom. We have little knowledge as to the thickness of these deposits; still such as we have goes to show that in these organic calcareous oozes and muds, we have a vast formation greatly exceeding in bulk and extent the coral reefs of tropical seas; they are most widely distributed in equatorial regions, but some patches of Globigerina ooze are to be found even within the Arctic circle in the course of the Gulf Stream. The following table shows the estimated area of the various kinds of deposits, with the average depth, and average percentage of carbonate of lime in each:—

Table showing the Estimated Area, Mean Depth, and Mean Percentage of $CaCO_3$ *of the different Deposits.*

Deposits.		Area, square miles.	Mean depth in fathoms.	Mean per cent. of $CaCO_3$.
Oceanic Oozes and Clay.	Red clay.	50,289,600	2727	6·70
	Radiolarian ooze.	2,790,400	2894	4·01
	Diatom ooze.	10,420,600	1477	22·96
	Globigerina ooze.	47,752,500	1996	64·53
	Pteropod ooze.	887,100	1118	79·26
Terrigenous Deposits.	Coral sands and muds.	3,219,800	710	86·41
	Other terrigenous deposits, blue muds, &c.	27,899,300	1016	19·20

One of the most remarkable facts discovered by the *Challenger* Expedition is that, although the dead shells of these pelagic organisms are rained down on the sea bottom, and in shallower depths accumulate so as to form calcareous deposits of immense extent, still, in other contiguous but deeper areas, these shells do not accumulate on the bottom, being wholly removed either while falling through the water, or shortly after reaching the ocean's floor. The pelagic organisms are as abundant in the surface waters over the one area as over the other, the only apparent difference in the conditions being one of depth. In the shallowest deposits of the open sea, shells, representative of nearly all the lime-secreting surface organisms, are to be found in the deposits. With increasing depth the more delicate ones disappear from the bottom, till, in 1800 or 2000 fathoms, it is rare to find more than traces of Heteropod, Pteropod, or the more delicate pelagic Foraminifera shells in the deposits, while these same delicate shells occasionally make up fully one-half of the carbonate of lime that is present in depths of 700 or 1000 fathoms. Again, in the still greater depths of 3000 and 4000 fathoms and deeper, the Foraminifera, Coccoliths, and Rhabdoliths are either wholly removed, or are represented only by the broken fragments of the thickest and most compact shells, like *Pulvinulina menardii*, *Sphæroidina dehiscens*, or *Globigerina conglobata*. This gradual decrease in the quantity of carbonate of lime in the deposits with increasing depth is well illustrated in the following table, showing the percentage of lime in the samples of deep-sea deposits collected by the *Challenger* towards the central parts of the ocean basins, away from the immediate influence of the *débris* from continental land or volcanic islands.

The organic oozes, including the red clays and the coral deposits, make up a total of 231 samples, and are arranged as follows, showing the percentage of carbonate of lime in relation to depth:—

14	cases under	500	fathoms,	m. p.c.	86·04.
7	,, from	500 to 1000	,,	,,	66·86.
24	,, ,,	1000 to 1500	,,	,,	70·87.
42	,, ,,	1500 to 2000	,,	,,	69·55.
68	,, ,,	2000 to 2500	,,	,,	46·73.
65	,, ,,	2500 to 3000	,,	,,	17·36.
8	,, ,,	3000 to 3500	,,	,,	0·88.
2	,, ,,	3500 to 4000	,,	,,	0·00.
1	,, over	4000	,,	,,	trace.

The fourteen samples under 500 fathoms are chiefly coral muds and sands, and the seven samples from 500 to 1000 fathoms contain a considerable quantity of mineral particles from continents or volcanic islands. In all the depths greater than 1000 fathoms

[1] Murray, "Structure and Origin of Coral Reefs," Proc. Roy. Soc. Edin., 1880, p. 508.

the carbonate of lime is mostly derived from the shells of pelagic organisms that have fallen from the surface waters, and it will be noticed that these wholly disappear from the greater depths. These figures are derived from a study of the *Challenger* deposits alone, but they are confirmed, as to the general result, by an examination of the deposits collected by the U.S.SS. *Tuscarora* and *Blake*, by H.M.SS. *Egeria* and *Investigator*, the ships of the Telegraph Construction and Silvertown Companies, and other ships. One other peculiarity as to the distribution of carbonate of lime organisms on the ocean's floor may be noted. Where these calcareous shells are most abundant on the surface, as in the tropics, the remains of the dead shells are as a rule found at greater depths on the bottom than in temperate or polar regions, where they are relatively much less abundant in the surface waters.

In his paper on the origin of coral reefs, published many years ago, Mr. Murray pointed out that sea-water, rushing in and out of the lagoon twice in the twenty-four hours, would take up and carry away large quantities of the carbonate of lime which, in the form of coral sand and mud, covers the bottom of these shallow basins. Just as the surface shells are dissolved by falling through the layers of ocean water, so in this case the dead coral fragments are dissolved by the sea-water that continually passes over them; in this way, chiefly, he accounted for the formation of lagoons in atolls and barrier reefs.

During the past few years a large number of experiments have been carried on at the Scottish Marine Station for Scientific Research, with the view of throwing some additional light on the oceanic phenomena referred to in the preceding paragraphs, in so far as these relate to the secretion and solution of carbonate of lime under varying conditions. Those dealing with the secretion of carbonate of lime by organisms will be considered in the first place, and afterwards those treating of the solution of the dead carbonate of lime shells and skeletons will be discussed.

A brief account of some of the experiments will show the nature of the investigations, and indicate the results which have been obtained in so far as they bear on the subject with which we are dealing.

Experiment 1. A number of laying hens were shut up in a wooden building, all ordinary sources of lime being withheld. In a few days the eggs, in place of a calcareous shell, had only a membranous covering. Thereafter sulphate, phosphate, nitrate, and silicate of lime were successively added to their otherwise limeless food, and from all these salts they were enabled to form normal shells for their eggs consisting of carbonate of lime.

From the investigations of Irvine and Woodhead it is believed that the lime salts in passing through the blood assume the form of phosphate, which is carried to the point of secretion, where it is decomposed and deposited as carbonate. When magnesium and strontium salts were added to the hens' food the eggs became membranous and shelless.

Ex. 2. Artificial sea-water was prepared, from which carbonate of lime was rigidly excluded. In this water crabs after ecdysis produced the usual exo-skeleton of carbonate of lime from the lime salts, other than carbonate, present in the water.

Ex. 3. The artificial sea-water of Ex. 2, which was perfectly neutral before the introduction of living crabs, in the course of a short time became distinctly alkaline in character. This was found to be due to the decomposition of their effete nitrogenous products, and the formation of carbonate of ammonia, and ultimately of carbonate of lime.

Ex. 4 and 5. Sea-water was mixed with urine and kept at a temperature ranging from 60° to 80° F. After a time the whole of the lime present in the sea-water was thrown down as carbonate and phosphate.

Ex. 6. A number of small crabs were placed in two litres of ordinary sea-water, and were fed with mussel flesh. This water was not renewed, the effete matters from the crabs passing into it. After a few days the crabs died; the water being then in a putrid condition was set aside at a temperature of from 70° to 80° F., when it was found that practically the whole calcium in the sea-water had been thrown down as carbonate of lime.

Ex. 7. We obtained absolutely fresh "liquor" from a number of living oysters, and examined it before decomposition had begun. It appeared to be a mixture of lymph with unchanged sea-water. The specific gravity was 1·023, indicating a considerable admixture of fresh or river water. This liquor contained 0·1889 grammes per litre of total lime in excess of that present in sea-water of the same specific gravity, and its alkalinity was equal to 0·2581 grammes per litre in excess of sea-water of the same specific gravity.

Thus we had in this liquid an accumulation of total lime (in excess of that present in sea-water) amounting to 0·1889 grammes per litre, the greater part of which was in the form of carbonate in solution, presumably in an amorphous or hydrated condition. Apparently this is due to the direct secretion of carbonate of ammonia by the cells of the living animals, which, reacting on the sulphate of lime in the sea-water, is capable of throwing out nine-tenths of the soluble calcium salts present, in the insoluble condition of carbonate. The oyster liquor was found to contain saline ammoniacal salts in enormous excess over that which is present in ordinary sea-water.

Ex. 8. A similar experiment was made with the liquor taken from living mussels. The results coincided with those obtained in Ex. 7.

Theoretically urea plus two molecules of water will give carbonate of ammonia. If, therefore, this substance be a stage in the formation of urea, it is not unnatural to suppose that in shell-forming animals the shell-formation may take place at this stage without the formation of urea at all. In these experiments the usual method for the estimation of saline and albuminoid ammonia could not be followed, and we made use of the following simple adaptation by which we obtained concordant results.

Absolutely pure potash was added to a measured and carefully filtered portion of the sea-water under examination, and the precipitate formed removed by filtration. The clear filtrate was then Nesslerized in the usual manner. We had thus an accurate means of determining between the actual ammoniacal salts and the albuminoid matter, both of which are, as a rule, present in sea-water according to the amount it carries of living and dead organisms. To satisfy ourselves that the addition of pure potash to a fluid containing albuminoids alone does not give rise (immediately) to the production of saline ammonia, we treated pure albumen taken from a newly laid egg in this manner, as also urea, without obtaining any trace of ammoniacal reaction.

These experiments show the alteration in the constitution of the lime salts in sea-water, both by the decomposition of effete matters thrown into the sea by animals, as also by the secretion of carbonate of ammonia by the cell action of the animals.

Sea-water collected among the coral atolls of the Louisiade Archipelago, received from Captain Wharton, F.R.S., Hydrographer to the Admiralty, contained per million parts—

Saline ammonia	0·48
Albuminoid ammonia	0·18
	0·66

whilst water collected by the *Challenger* in the North Atlantic (lat. 30° 20′ N. long. 36° 6′ W.) contained—

Saline ammonia	0·26
Albuminoid ammonia	0·16
	0·42

and water from the German Ocean near land contained—

Saline ammonia	0·13
Albuminoid ammonia	0·13
	0·26

This is exactly what we were led to expect from the experiments enumerated—the greatest amount of saline ammonia being present where the greatest animal life activity existed, as in the waters from the coral sea; and least in the German Ocean winter water where it was at its minimum.

Thus the whole of the lime salts in sea-water may, under these circumstances, be changed into carbonate, and in this way may be presented to the coral and shell builders in the form suitable for their requirements.

The temperature of the water is of great importance in this reaction. In cold water, of which the great bulk of the ocean consists, the decomposition of nitrogenous organic matter is retarded, whereas in tropical surface waters it proceeds with great rapidity. Thus coral-reef builders and pelagic organisms may not only benefit by the decomposition arising from their own effete matter, but also from the undecomposed nitrogenous matter carried to equatorial regions from the cold water of the deep sea, or from polar regions.

The quantity of carbonate of lime normally present in sea-water is exceedingly small; and the opinion hitherto held seems to have been that lime-secreting organisms must pump enormous quantities of sea-water through their bodies in order to be able to separate out a sufficient quantity to form their shells and skeletons.

Bischoff, in his "Chemical and Physical Geology," vol. i. p. 180, estimates that oysters in this way have to deal with an amount of sea-water equal to from 30,000 to 75,000 times the weight of their shells. It seems more probable that the reactions indicated by our experiments render the whole lime salts in sea-water available for coral polyps to build up their structures. In polyps, which unlike the higher animals have no true circulatory system, and where the animal is immersed in sea-water, it is hardly possible to account for the enormous secretion of carbonate of lime in the manner indicated by Bischoff; but if the conclusion we have arrived at be correct, and such animals, in place of secreting urea, secrete carbonate of ammonia, then we have a perfectly reasonable explanation of the phenomenon of coral formation.

As a laboratory experiment, when carbonate of ammonia is added to sea-water, the greater proportion of the calcium in solution is after a time thrown down as carbonate of lime; whilst the magnesium salts remain in solution. So that if the reaction above indicated be that which takes place in sea-water, then to this circumstance may be due the fact that carbonate of magnesia is almost wholly absent from coral reefs and deep sea calcareous formations.

That the amount of nitrogenous organic matter in a state of suspension and solution must be enormous will appear evident when it is remembered that the floor of the ocean, almost throughout its whole extent, is covered with living animals; that the surface of the sea and shallow waters off the coasts are crowded with plants and animals down to a depth of several hundred fathoms. (The *Challenger* experiments have shown that some species of animals flourish in the intermediate depths of ocean water from the surface to the bottom.) The waste products arising from the functional activity of these organisms, and the nitrogenous products arising from the decomposition of their dead bodies, must work continual changes on the internal constitution of sea-water salts, varying according to their amount, the temperature, the sunlight, and other conditions. It has been shown that ammoniacal salts are to be found everywhere in the ocean, but much more abundantly in warm tropical waters than in colder seas—a result no doubt due to the rapid decomposition of the nitrogenous organic matter present at a high temperature, and its retardation in colder water. The ammonia of the air, and all nitrogenous substances carried from the land to the sea, must also effect changes in the internal constitution of sea-water. Indeed, the peculiar pelagic fauna and flora met with in all regions of the ocean, where it is affected by river and coast waters, are as different in relation with the internal constitution of the sea-water salts as with the lower salinity which prevails in these circumstances.

It is well known that organic substances in the presence of alkaline and earthy sulphates become oxidized at the expense of the oxygen of these salts, with the production of carbonic and hydrosulphuric acids, the latter on oxidizing producing sulphuric acid. The greater part of the organic carbon, which it has been pointed out is of enormous amount, must apparently be thus oxidized, producing an equivalent amount of carbonic and sulphuric acids. The effects of this reaction are likely to be more marked in the deeper parts of the ocean, where the motion of the sea-water must be extremely slow, and where consequently the effete products accumulate; in this way the larger amount of lime and carbonic acid and the less amount of oxygen in solution in such waters is to be accounted for. Not only so, but the very existence of such a relatively large quantity of sulphate of lime in sea-water goes far to prove that this reaction must continually take place, seeing that sulphuric acid cannot exist in a free state in the presence of carbonate of lime. Thus it is probable that the quantity of sulphate of lime in solution in the ocean is only limited by the amount of organic decomposition which takes place in its waters. On the other hand, if marine organisms procure the whole of their carbonate of lime from the sulphate of lime by the reaction of ammoniacal salts, then the amount of lime that may be secreted from ocean waters is likewise limited by the amount of organic matter undergoing this oxidation process in the ocean.

Gmelin, in his "Chemistry," vol. ii. p. 191, refers to this decomposition as follows:—

"In hot climates, as on the west coast of Africa, where the water of rivers charged with organic matter mixes with sea-water, hydrosulphuric acid, sometimes to the extent of 6 cubic inches to the gallon, is found in sea-water, even at a distance of 27 miles from the mouth of the rivers."

This is also confirmed from samples of water which we have received, taken from the roadstead of Monte Video by the telegraph ship *Seine*.

If now we turn our attention to the solution of dead carbonate of lime in shells and coral skeletons by the action of sea-water, it will be found that the rate of this solution varies greatly according to the conditions in which these remains are exposed to the solvent power of the water. A large number of experiments have been conducted with the view of determining the solubility of carbonate of lime under its different conditions. It may be pointed out that the normal amount of carbonate of lime dissolved in sea-water is very small, strikingly so (0·1200 grammes per litre) when compared with the vast amount of this substance continually being secreted from the sea by organisms. Sea-water can, however, take up 0·6490 grammes per litre of carbonate of lime in an amorphous (or hydrated) condition, forming a clear supersaturated solution, but after a time not only the excess so added is thrown down, but also sometimes a portion of that normally present in the water itself. It would thus appear that it is unable permanently to retain in solution more of this substance than is usually found present in sea-water. This peculiarity of sea-water, after taking up a large amount of amorphous carbonate of lime, and throwing it out in a crystalline form, accounts for the filling up of the interstices of massive corals with crystalline carbonate in coral islands and other calcareous formations, so that all trace may ultimately be lost of their original organic structure. These experiments show a great diversity as to the amount of carbonate of lime which will pass into solution in sea-water from various calcareous structures in a given time. As a rule, the more definitely crystalline the substance is, the less it is soluble. Calcspar is less soluble than massive varieties of coral, and these again less than the more porous varieties. We have already indicated that amorphous or hydrated carbonate of lime is (in that condition) much more soluble than any other form of the substance. The rate of solution is also much greater when the water is constantly renewed, than when the same water remains in contact with carbonate of lime. The water quickly becomes saturated and unable to exert further solvent action. In this connection we found that different samples of sea-water from different localities possessed very different solvent powers. Especially was this the case between summer and winter waters, the former having distinct solvent action on coral skeletons, whilst with the latter there was hardly any. The lower specific gravity of winter waters may be regarded as to some extent reducing their solvent power, but this is more probably to be attributed to the absence of free carbonic acid—that is, carbonic acid in excess of what is required to saturate the free base in the sea-water as normal carbonate. To test this point, carbonic acid was added to one of these winter waters (which had no solvent action on coral), the quantity added not being sufficient to destroy its alkaline character. It was found that in these circumstances an appreciable amount had been dissolved.

This appears to indicate that there is more carbonic acid in summer than in winter waters in our latitudes, due probably to the increased activity of animal life. Mr. Buchanan's observations on board the *Challenger* show that the carbonic acid present in sea-water, over and above that necessary to form normal carbonate of lime, was subject to great variations. It appears that this is a much more effective agent in the removal of carbonate of lime shells, &c., than the solvent power of sea-water itself (although artificial sea-water quite free from carbonic acid dissolves carbonate of lime). Buchanan's observations have also shown that carbonic acid as a rule is more abundant in bottom than in surface waters; and Reid's experiments show that carbonated sea-waters under high pressure take up more carbonate of lime than that at a normal atmospheric pressure. The fact that carbonic acid is more abundant in deep waters is evidently connected with the respiration, and also the decay, of the animals which live and die on the ocean floor; and also with the decay of those which fall from the surface. The water filling the deeper hollows has also in its passage to the equator passed over thousands of square miles of this floor covered with living animals, and as this water has a very slow motion, and is but slowly renewed, we would expect an accumulation of carbonic acid and deficiency of oxygen in these abysmal depths. When, therefore, carbonate of lime

secreting animals die at the surface of the water and their bodies fall to the bottom, the shell is exposed to solution from the action of the sea-water through which it passes, and it may be to that of carbonic acid produced by the decomposition of its own organic matter. If the shell be thin, as in the case of Heteropods and Pteropods, it may be wholly removed before reaching the bottom, but the thicker shelled varieties tend to accumulate even in depths of 2000 fathoms, where they are soon covered up by other shells; and being surrounded by sea-water already saturated with carbonate of lime, are preserved from solution, and form vast beds of calcareous ooze. It is found that the amount of carbonate of lime present in such ooze is greater or less according to the depth of water through which the shells pass from the surface to the bottom, and also to the slow renewal of the water in contact with these great lime deposits. In the red clay area the carbonate of lime is almost entirely absent. The deeper waters which cover such areas are more active in the removal of carbonate of lime, not only because of the larger amount of carbonic acid they contain, but doubtless to the deoxidation of alkaline sulphates by organic matter, which, we have already pointed out, gives rise to sulphuric acid, &c. At the same time account must be taken of the great pressure at such abysmal depths, and the fact that the substance of the shells being less compressible than sea-water, they would fall more slowly, and hence would be longer exposed to the action of the deeper layer of water than those near the surface.

What calcareous remains do reach the ocean floor at such abysmal depths represent the hardest and crystalline varieties of carbonate of lime which resist the solvent action of sea-water to the greatest extent.

In this way we appear to have a perfectly rational explanation of the partial disappearance of carbonate of lime shells from the shallower depths, and their total disappearance from all the greater depths of the ocean. It is to be observed that all those shells in which a considerable quantity of organic tissue is associated with the carbonate of lime disappear in solution more rapidly than the shells of the Foraminifera, which contain little organic matter. (During the whole of the *Challenger* cruise only two bones of fishes, other than the otoliths and the teeth, were dredged from the deposits, and all traces of the cetacean bones were removed, except the dense ear-bones and dense Ziphioid beaks.) The remains of crustacean animals were almost wholly absent from deep-sea deposits, with the exception of Ostracode shells and the hard tips of some claws of crabs.

Turning now to the lagoons and lagoon channels of coral islands, it is believed that large quantities of carbonate of lime are in the same way being dissolved from these shallow basins as well as from the deposits of the deep sea, but under somewhat different circumstances. In the case of a shell falling to the bottom of the sea, it is continually brought in contact with new layers of water, which has the same effect as if a continuous stream of water were passing over the shell. In the case of the lagoons this last is what takes place. The water which flows in and out of the lagoons twice in twenty-four hours passes over great beds of growing coral, and from all the observations we have is largely charged with carbonic acid, owing probably to the large number of living animals on the outer reef over which the water passes on its way to the lagoon. This water passes continually over the dead coral and sand of the lagoon, and takes up and removes large quantities of carbonate of lime in solution (as well as suspension), for in these lagoons the spaces covered by dead coral *débris* always greatly exceed the patches of growing coral. Owing to the fact that the water of the lagoon is continually in motion, and constantly renewed, the layer in contact with the bottom of coral sand can never become saturated or unable to take up more lime, as is apparently the case in the layers of water in contact with the Globigerina ooze and other calcareous deep-water deposits.

From the foregoing discussion and observations it is evident that a very large quantity of carbonate of lime is in a continual state of flux in the ocean; now existing in the form of shells and corals, but after the death of the animals passing slowly into solution, to go again through the same cycle.

On the whole, however, the quantity of carbonate of lime that is secreted by animals must exceed what is re-dissolved by the action of sea-water, and at the present time there is a vast accumulation of carbonate of lime going on in the ocean. It has been the same in the past, for with a few insignificant exceptions all the carbonate of lime in the geological series of rocks has been secreted from sea-water, and owes its origin to organisms in the same way as the carbon of the carboniferous formations; the extent of these deposits appears to have increased from the earliest down to the present geological period.

At the present time most of the carbonate of lime carried to the ocean by rivers has been directly derived from calcareous stratified rocks formed by organic agency in the sea in earlier geological ages, but the calcium in these formations was in the first instance derived from the decomposition of the lime-bearing silicates of the earth's original crust, and this decomposition, which is still going on in the sea and on the land surfaces, is a continuous additional source of carbonate of lime.

In considering the analyses showing the average composition of sea salts, one is struck with the relatively small quantity of those very substances which are extracted so largely from sea-water by plants and animals, viz. carbonate of lime and silica. Siliceous deposits are of vast extent, yet silica occurs merely in traces in sea-water; carbonate of lime deposits are of vastly greater magnitude, yet carbonate of lime makes up only $\frac{1}{100}$th part of the saline constituents of sea-water, and only $\frac{1}{3300}$th part of the whole bulk of sea-water. Sulphate of lime is ten times more abundant than the carbonate in sea-water; on the other hand, the river water that is poured into the ocean contains about ten times as much carbonate as it does of sulphate of lime.[1]

The total amount of calcium in a cubic mile of sea-water is estimated from analyses to be 1,941,000 tons, and the total amount of calcium in the whole ocean is calculated at 628,340,000,000,000 tons. The total amount of calcium in a cubic mile of river water is estimated at 141,917 tons, and the total amount of this element carried into the ocean from all the rivers of the globe annually is estimated at 925,866,500 tons. At this rate it would take 680,000 years for the river drainage from the land to carry down an amount of calcium equal to that at present existing in solution in the whole ocean. Again, taking the *Challenger* deposits as a guide, the amount of calcium in these deposits, if they be 22 feet thick, is equal to the total amount of calcium in solution in the whole ocean at the present time. It follows from this that if the salinity of the ocean has remained the same as at present during the whole of this period, then it has taken about 680,000 years for the deposits of the above thickness, or containing calcium in amount equal to that at present in solution in the ocean, to have accumulated on the floor of the ocean. From the data here furnished a number of other interesting speculations might be indulged in, relating to the amount of carbonic acid that has been abstracted from the atmosphere and fixed in carbonate of lime deposits; the total amount of disintegration of lime-bearing siliceous rocks measured in terms of the calcium at present existing in solution in water and fixed in calcareous deposits; the relative proportions of substances secreted from the ocean as compared with other materials derived from the direct disintegration of the land-forming deep-sea deposits; and the apparent accumulation of carbonate of lime formations towards the equatorial regions of the globe. These various matters will, however, be discussed in another place.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The event of the week has been the achievement of Miss Philippa Garrett Fawcett, of Newnham College, who, in Part I. of the Mathematical Tripos, is declared to be "above the Senior Wrangler," Mr. Bennett, of St. John's.

Mr. Sedley Taylor, the delegate from Cambridge to the Sexcentenary Festival of the Montpellier University, in a letter to the Vice-Chancellor on the subject of his mission, writes:—"We had the great satisfaction of seeing Prof. von Helmholtz, Delegate of the University of Berlin, publicly received with much cordiality, and of learning that, on account of his optical researches, which have given such a beneficent impulse to modern ophthalmology, he was subsequently made the object of a special ovation by the Medical Faculty for which the University of Montpellier has long been famous."

Dr. Butler, Master of Trinity College, was on June 2 again elected to the office of Vice-Chancellor for the ensuing academical year.

The John Lucas Walker Research Studentship in Pathology is vacant by the resignation of Dr. William Hunter, of St. John's College, recently elected to a Research Scholarship in

[1] Murray, "Total Rainfall of the Globe," *Scot. Geogr. Mag.*, 1887.

Sanitary Science by the Grocers' Company. The election will take place about August 26. Candidates are requested to apply to Prof. Roy, 2 Wollaston Road, Cambridge, for information. The Studentship is of the annual value of £200, or of such larger sum, not exceeding £300, as the managers shall from time to time determine; and is tenable for three years. The Student is required to devote himself during the tenure of the Studentship to original pathological research. Dr. Hunter's tenure has been marked by his elaborate and valuable researches on pernicious anæmia.

The Professor of Mineralogy (Prof. Lewis) proposes to give a course of elementary lectures on crystallography in the long vacation, beginning on Tuesday, July 8, at 9 a.m. There will also be a practical course on crystallography given by the Demonstrator, beginning on the same day. Fees for lectures £1 1*s.*; for demonstrations £2 2*s.*

The Special Board for Biology and Geology have nominated Miss L. Ackroyd (Newnham College) to occupy the University table at the Laboratory of the Marine Biological Association for one month during the year 1890.

The Mechanical Workshops Enquiry Syndicate were on Thursday, June 5, empowered by a large majority of the Senate to inquire into the conditions and expense of establishing a definite school of engineering in the University.

The number of persons matriculated during the current academical year was on May 29 brought up to 1027, the largest number on record.

At a meeting of the Council of the Cambridge Philosophical Society on Monday, June 2, it was decided, in accordance with the Reports of the adjudicators, Sir W. Thomson, Lord Rayleigh, and Prof. G. H. Darwin, to award the Hopkins Prize for the period 1883-85 to W. M. Hicks, F.R.S., for his memoir upon the "Theory of Vortex Rings" (Phil. Trans., 1885) and for his earlier memoirs upon related subjects; also to award the Hopkins Prize for the period 1886-88 to Horace Lamb, F.R.S., for his paper on "Ellipsoidal Current Sheets" (Phil. Trans., 1887) and for his numerous other papers on mathematical physics.

Prof. J. J. Thomson announces that a course of demonstrations in practical physics, suitable for students who intend taking the Natural Sciences Tripos after passing Part I. of the Mathematical Tripos, will be given during the Long Vacation in the Cavendish Laboratory on Mondays, Wednesdays, and Fridays, at 10 a.m., commencing July 9. Students wishing to attend the course are requested to send in their names to Prof. Thomson before the end of the term.

The Observatory Syndicate publish in the *Reporter* (June 10, 1890) their record of proceedings for May 27, 1889, to May 26, 1890. The astronomical work of observation and reduction has been steadily carrried out, and the report is not marked by any eventful feature.

Dr. D. MacAlister and Prof. Roy have been appointed to represent the University at the Tenth International Medical Congress at Berlin.

The General Board of Studies, with a view to recruiting the finances of the University, especially in the scientific departments, propose to raise the examination and other fees payable by students. As a commencement they propose that the aggregate fees to be paid for the six M.B. examinations be raised from eight guineas to twelve.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 5.—"Account of Recent Pendulum Operations for determining the Relative Force of Gravity at the Kew and Greenwich Observatories." By General Walker, C.B., F.R.S., LL.D.

It is well known that a series of pendulum observations was carried on in India, during the years 1865 to 1873, with two invariable pendulums, the property of the Royal Society. The Observatory of the Royal Society at Kew was chosen as the base station of the operations, and the pendulums were swung there before being sent out to India, and again on their return from India. With a view to connecting the observations with those which had already been taken with other pendulums in other parts of the world, it was intended, on the return of the pendulums from India, to swing them at the Royal Observatory at Greenwich, which was a well-established pendulum station, observed at by General Sir Edward Sabine, the Russian Admiral Lütke, and others. But when the time arrived for making the observations at the Greenwich Observatory, such extensive preparations were being made there for the equipment of expeditions for the observation of the approaching transit of Venus that no room was available for the pendulum operations. It was, therefore, decided to make the connection with Kew by swinging at Kew Kater's convertible pendulum, for determining the absolute length of the seconds' pendulum, which had been swung 40 years previously at Greenwich by General Sabine. This being done, the length of the seconds' pendulum at Kew was found to be 0·0027 of an inch greater than the length which had been previously determined at Greenwich, and consequently that the daily vibration number was three vibrations greater at Kew than at Greenwich. The difference, however, was far too large to be admissible, as the Observatories are nearly in the same latitude, and differ very slightly in height.

In 1881, Colonel Herschel, R.E., was deputed by the Secretary of State for India to take pendulum observations at the two Observatories, and at the old pendulum station in London, and also at some stations in America, with a view to improving and strengthening the connection between the observations in India and those in other parts of the world. On completing his work in America, he handed over the three pendulums which he had employed to officers of the United States Coast and Geodetic Survey, by whom they were taken round the world, and swung at Auckland, Sydney, Singapore, Tokio, San Francisco, and finally at Colonel Herschel's terminal station at Washington.

But when the observations came to be finally reduced, it was found that the difference between Colonel Herschel's results at Kew and Greenwich, as shown independently by the three pendulums, had an extreme range of about seven vibrations in the daily vibration number. The cause of these differences was mysterious and inexplicable, and there was no alternative but to swing the pendulums a second time at the two Observatories.

The revisionary work was undertaken by the Observatory staff at each place, in such intervals of leisure as they could obtain from their regular operations. The final results, by the three pendulums, make the vibration number at Kew in excess of that at Greenwich by 1·56, 1·50, and 0·59, giving an average excess of 1·22.

The correction to this quantity for the excess of height of the Greenwich over the Kew Observatory is − 0·58. Thus, the revisionary operations, reduced to the mean sea-level, make the excess of Kew over Greenwich = 0·64 of a vibration, which may be accepted as very fairly probable.

Royal Microscopical Society, May 21.—Mr. James Glaisher, F.R.S., Vice-President, in the chair.—Mr. Mayall referred to the donation, by the Messrs. Trainini Bros., opticians of Brescia, of an early form of achromatic microscope objective, constructed by the late Bernardini Marzoli, Curator of the Physical Laboratory of the Lyceum of Brescia. The objective was a cemented combination, and was described and figured in the "Commentari della Accademia di Scienze" of Brescia in 1808. This and other works and documents in proof of its authenticity were exhibited.—Mr. Mayall exhibited on behalf of Mr. F. Vallance an eye-piece similar to that shown at the previous meeting by Mr. Goodwin. It was one of two constructed by Mr. Murrell nearly forty years ago, and was provided with a screw which enabled the compound eye lens to be adjusted with reference to the field lens through a space of nearly ½ inch.—Mr. E. M. Nelson read a paper on micrometers, in the course of which he described a new micrometer made for him by Messrs. Powell and Lealand. The subject was illustrated by a drawing upon the board, and the micrometer attached to a microscope and lamp was handed round.—Mr. Thomas Comber's paper on a simple form of heliostat, and its application to photomicrography, was read. Apart from the question of the extreme simplicity of the heliostat, which was mainly due to limiting the reflection of the mirror to the polar direction and deflecting the pencil in the horizontal direction in the axis of the microscope, by means of a fixed mirror placed at half the angle of the latitude above the heliostat mirror, Mr. Comber had rendered important service to photomicrography by showing how the heliostat might be placed close to the microscope so that the error due to slight inaccuracy of the adjustment of the heliostat might escape the optical leverage which took place when the reflected beam was made to travel through a considerable space.

PARIS.

Academy of Sciences, June 2.—M. Hermite in the chair.—On the application of a double plane mirror to the precise

measurement of stellar distances, by MM. Lœwy and Puiseux. In previous communications the authors have developed the theory of the optical system formed by a double plane mirror cut out of a single block of glass in the form of a prism, and placed in front of the object-glass of an equatorial. The properties of the apparatus are now demonstrated, and a practical method of observation deduced.—On the reduction of sulphates of the alkalies by hydrogen and by carbon, by M. Berthelot.—The author discusses in detail the mechanism of the reactions taking place in these reductions, with especial reference to the conditions obtaining during the process of manufacturing sodium carbonate. The equation $K_2SO_4 + 4H_2 = K_2S + 4H_2O$ expresses approximately the final state of the system, but does not at all represent the course of the reaction, which is probably as follows :—

$$(1)\ K_2SO_4 + 4H_2 = KHS + KOH + 3H_2O\ ;$$

the KHS then decomposes.

$$(2)\ 2KHS = K_2S + H_2S,$$

and the H_2S reacts with the KOH.

$$(3)\ KOH + H_2S = KHS + H_2O.$$

Equations (1) and (3) represent exothermic reactions, (2) is the expression of an endothermic dissociation which takes place at the temperature of reduction. In addition to the above an exothermic reaction takes place between the alkaline sulphide and water vapour, thus—

$$(4)\ K_2S + H_2O = KHS + KHO.$$

The reduction by hydrogen takes place at a comparatively low temperature. With respect to the action of carbon upon the alkaline sulphates, it is shown that solid carbon even at a very bright temperature fails to react with the sulphate, but that carbonic oxide at a bright red heat reduces the salt according to the equation—

$$(5)\ K_2SO_4 + 4CO = K_2S + 4CO_2,$$

the reaction being markedly exothermic.—Note by M. Blanchard accompanying the presentation of a work on the "Actions of the Products secreted by Pathogenous Microbes."—On the fossil Hippopotami of Algeria, by M. A. Pomel. The genus Hippopotamus has been represented in Algeria at different times during the Quaternary period, and the author describes the order in which the types succeeded each other. Of four species, two are said to be certainly special, and probably also a third, whilst the last is almost unknown.—Observations of Brooks's comet (a 1890) made with the Brunner equatorial at Toulouse Observatory, by M. E. Cosserat. Observations of the position of the comet, extending from April 28 to May 14, are given.—On the curve representing diffraction phenomena, by M. Ernest Cesaro.—On the characteristic equation of nitrogen, by M. Ch. Antoine. Some experiments by M. Amagat on the compression of nitrogen between 39·5 and 421·1 atmospheres are used to calculate the value of $\frac{pv}{D(\beta + t)}$, where p is the pressure, and v the volume of a gas. Taking $D = 2{\cdot}830 + 0{\cdot}00191p^{1{\cdot}1}$, which, however, can only be taken as a first approximation, the mean value found is 3·10.—On the ballistic electrometer, by M. Gouy.—The month of May 1890 at the Observatory of the Parc de Saint-Maur ; the cold of June 1, by M. E. Renou. The month of May was remarkable for low mean pressure, viz. 753 mm. at an altitude of 49·38 m. The mean temperature was 14°·0, or 0°·7 above the average of other years. On June 1 the minimum thermometer 2 metres above the ground registered 2°·7, and the ground thermometer registered 3°·3 below zero at sunset.—On the determination of the molecular weight at the critical point, by M. Philippe A. Guye. M being the molecular weight of any body, k the critical coefficient (the relation of the absolute critical temperature to the critical pressure), and R the specific refractive power, given by the formula of Lorentz and Lorenz, we have $M = 1{\cdot}8\,\frac{k}{R}$. The author shows the agreement of the results obtained by calculation with those experimentally determined, and claims that his method should rank with the vapour-density and cryoscopic methods of determining molecular weights.—On the chloro-salts of iridium, and the atomic weight of this element, by M. A. Joly. The double chlorides of iridium and potassium and iridium and ammonium are described, and from the results of their analyses the atomic weight of Ir is found to be 192·75 (H = 1) ; Seubert's value is Ir = 192·744.—On the oxides of manganese obtained in the wet way ; second part—manganous acid, by M. A. Gorgeu.—On some new double iodides of bismuth and potassium, by M. Ch. Astre. There are now five of these double iodides known—namely, $(BiI_3)_2, KI$; $(BiI_3)_2, 2KI, 2H_2O$; $(BiI_3)_2, 3KI, 2H_2O$; $(BiI_3)_2, 4KI$; and $(BiI_3)_2, 6KI$; of which the three latter are new, and form the subject of the present paper.—On soda-alum, by M. E. Augé. The properties of this body are incorrectly described in text-books. The author contrasts the observed properties with the properties attributed to the compound by most authors.—The *bouquet* of fermented drinks, by M. Georges Jacquemin.—New researches on the origin of omphalocephlaic monsters, and on the primitive duality of the heart in the embryos of Vertebrata, by M. Dareste.—On the arrangement of the collections of molluscs at the Natural History Museum, by M. Edmond Perrier.—On the development of blastodermic layers in *Géphyriea tubicola* (*Phoronis Sabatieri*, nov. sp.), by M. Louis Roule.—On the androgynous castration of the *Muscari comosum*, Mill., by the *Ustilago Vaillantii*, Tul., and some remarkable phenomena accompanying the parasitic castration of the *Euphorbia*, by M. Ant. Magnin.—On the æleolithic syenite of Montreal, and on the endomorphous and exomorphous contact modifications of this rock, by M. A. Lacroix.—Action of soluble substances produced by microbes on inflammation, by MM. Charrin and Gamaleia.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Characteristics of Volcanoes : J. D. Dana (S. Low).—A Contribution to the Natural History of Scarlatina : Dr. D. A Gresswell (Oxford, Clarendon Press).—A Manual of Pharmaceutical Testing : B. S. Proctor (Office of the *Chemist and Druggist*).—Aluminium, 2nd edition : J. W. Richards (S. Low).—Die Gesetze und Elemente des Wissenschaftlichen Denkens, Erster Band : Dr. G. Heymans (Leiden, van Doesburgh).—British Cage Birds, Part 2 : R. L. Wallace (Gill).—The Canary Book, Part 2 : R. L. Wallace (Gill).—Elementary Algebra, 2nd edition : C. Smith (Macmillan).—Induction and Deduction : C. C. W. Nadens (Bickers).—The Philosophy of Clothing : W. M. Williams (Laurie).—Madagascar ; or, Robert Drury's Journal : edited by Captain Oliver (Unwin).—Blackie's Modern Cyclopedia, vol. 6 (Blackie).—Fifty Years of Science, 4th edition : Sir J. Lubbock (Macmillan).—Sanity and Insanity : C. Mercier (Scott).—Nature and Woodcraft : J. Watson (Smith and Innes).—Den Norske Nordhavs-Expedition 1876-78, xix. Zoologi—Actinida : D. C. Danielssen (Christiania, Grondahl).—Observations of the New England Meteorological Society in the year 1888 (Cambridge, Mass., Wheeler).—Meteorological Observations made at the Summit of Pike's Peak, Colorado, January 1874 to June 1888 (Cambridge, Mass., Wheeler).

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THURSDAY, JUNE 19, 1890.

BRITISH AND ORIENTAL CICADIDÆ.

Monograph of the British Cicadæ, or Tettigidæ. By G. B. Buckton, F.R.S. Illustrated by more than Four Hundred Coloured Drawings. (London: Macmillan and Co., 1890.)

A Monograph of Oriental Cicadidæ. By W. L. Distant. (Calcutta: Indian Museum. London: H. S. King and Co. 1890.)

THE insects forming the family of the Cicadidæ of Westwood are among the largest of the Homoptera, and by far the largest number of the known species are to be met with in the warm regions of the world. Some fifty years ago but one species of this family seems to have been recorded from Great Britain—it was found in the New Forest, and figured by Curtis as *Cicada anglica.* Curtis thought it did not sing, because a specimen kept in confinement by Mr. Dale for two or three days was mute. Kirby and Spence, however, were informed that it was very noisy, and, adds Prof. Westwood, "analogy would lead to the belief that it does sing, the drums of *C. orni* not being comparatively larger." Weaver found the pupa-case of this insect attached by the legs to the stem of a fern.

Great have been the changes within the last half-century, during which all the above-mentioned well-known names, but that of the respected Professor of Zoology at Oxford, have been numbered among those of the dead; and now the number of the species of the "British Cicadæ"—using this word, however, in a wider sense—is about 230. Mr. G. B. Buckton, F.R.S., so well known for his excellent monograph of the British Aphidæ, has published the first two parts of an illustrated monograph of our native "froghoppers and grassflies."

Although not of large size, like their tropical brethren, our native species are of great interest, and as to this date there has been no serious attempt to publish an adequately illustrated history of even the European forms, the appearance of this monograph is all the more welcome, and its publication will, no doubt, very greatly facilitate the study of these insects.

It is proposed that this monograph shall be published in eight quarterly parts, and these will be illustrated by about eighty coloured plates. Part 1 was issued in January, and Part 2 in April of this year.

The monograph opens with an introduction, in which the author tells us that he proposes to treat his subject under the following heads: Etymology, and the ancient notices of the Cicada or Tetix; classical allusions and poetic myths relating to them; a biographical sketch of the writings and investigations of authors who have considered the subject; a terminology and description of the parts available for classification; general remarks as to their life-history, reproduction, &c.; diagnosis of species, accompanied by coloured representations of the British species of these insects; notes on variation and distribution; remarks as to the probable antiquity of the group, as shown by their remains in the rocks, amber, and fossil resins; and, in addition to all this, in an appendix, there is to be a bibliographical list of the chief modern authors who have studied the Cicadæ; and a short list of ancient and modern quotations, for reference and for use by the curious. Certainly, we have here the programme of a very large and entertaining volume.

We would suggest that Mr. Buckton should not limit his bibliographical list to the "chief authors who have studied the group," but that he should, if even at the cost of cutting out some of the folk-lore, make this list a complete one. Indeed, if we are to judge of the promise by the present performance, it would perhaps be wiser for the author to dwell more on the descriptive and bibliographical portions of his work, than on those appertaining to the literature thereof, for no small research, of a peculiarly special character, would be necessary before one could successfully write the history of the ancient notices of the Cicadæ, recall all the classical allusions that have been made to them, or even give an account of the early scientific writings about them. We agree with the author that "the ordinary scope of a monograph is the description of the forms, life-history, distribution, &c., of the species contained in it," and hence we regret that he should have added so much to his labours by venturing, in this volume, on other fields of research, with which there seems to be some proof that he has not been so familiar. Thus, on p. iii. of the introduction we read that "the first English author who wrote on the Cicada was Dr. Thomas Moufat, or Mouffet, an English physician, who flourished in the reign of James I. In 1634 he wrote, in folio, a curious Latin treatise on zoology, having for its title, 'Insectorum sive minimorum Animalium Theatrum.'" On p. xx. we find a short account of this book, which is said to be "somewhat rare"; it is therefore reasonable to conclude that Moufet's volume was in our author's hands, but, if so, he could never have read over the dedicatory epistle, from which he, however, quotes, with any care. If we are able to judge by his spelling of Moufet's name, even the title-page was not carefully examined. A glance at Hagen's "Bibliotheca Entomologica," or at Burmeister's "Manual of Entomology," would have guarded the author from a great many mistakes.

Moufet was a physician living in London; he was born in 1550, and, according to Burmeister, he died in 1604. From the little known of him, it does not appear that he was an entomologist. His little volume, "Nosomantica," treating of the prognosis of disease, was published in 1588, and he died "in poverty." Conrad Gesner had laboured hard to complete his "Historia Animalium" by a history of insects, based on Wotton, but he died before he had made much progress with it. Pennius took up the subject, worked at it for 15 years, and then too died, leaving many drawings and fragments of descriptions in manuscript; whereupon Moufet tells us he arranged these descriptions in order, "added to them the light of oratory which Pennius wanted, and so constructed" the history referred to. Born during the reign of Mary, Moufet flourished in the days of Elizabeth, and died at or about the time King James ascended the throne. In 1634, when the work of Wotton (of Oxford), Gesner, and Pennius first saw the light, as "wove together" by Moufet, Charles I. was on the English throne; and, as

can be read in the "Epistola," not of Moufet, but of Sir Theodore de Mayerne, in which he dedicates the work to the illustrious Sir William Paddy, Moufet constructed the "History" hoping to acquire fame by dedicating his compilation to the Virgin Queen; but she, as Hume notes, was no great lover of literary or scientific men, and the dedication appears not to have been accepted, and shortly after her death Moufet also died. "Great poverty at home" then delayed the publication, so the book lay for a long time in obscurity, until it was offered to Sir T. de Mayerne by Moufet's apothecary, Darnello. It lay even then in de Mayerne's library for a long time, subject to the attacks of "moths and cockroaches," and that through no fault of his, "but the printers demanded too much money." In the month of May 1634, dedicated to no sovereign, but to the "ever illustrious Paddy," and having been approved of by "Guliel. Bray, of Lambeth Palace," it was published in London by Hope.

Remembering who wrote the "Epistola," it is funny to find Mr. Buckton writing, "Moufat was a true naturalist, and well loved the subject of which he treated. In his Epistola he dilates on the pleasure, &c., felt by the operator, &c." Instead of Moufet one should read Th. de Mayerne, and the operator was William Paddy, and whoever may have been the Frenchman referred to in this same paragraph, assuredly it was not Réaumur, for he was not born until half a century after the publication of Moufet's work. Perhaps enough in the way of criticism has been written about this subject, but it seems right to call attention to the numerous errors of translation from Moufet's Latin text, which errors are the more to be deplored as the translation given in the Rev. Edw. Topsel's "History of Animals," published in 1658, is fairly accurate, and could have been easily referred to. In referring to the text where Moufet writes about the song of the Cicadæ, Mr. Buckton deplores (p. xii.) its want of clearness, and even ventures to emend it, but Moufet's text is not correctly quoted, and in the translation the whole sense of the original is quite lost. Moufet, no doubt, omitted the word λαλιστέραν from his quotation, but the words cited by Athenæus mean, "I have never seen one more loquacious, no, neither a Cercopia, nor a jay, nor a nightingale, nor a Tettiga, nor a turtle-dove."

Moufet may or may not have been disgusted with the luxury of his day, and possibly partook of the Puritanic spirit of the age (p. xxi.), but he could not have intended that the word "magistræ" should have been translated (even with a ?) as "mistresses": the whole of the translation here is indeed curious; the "health-giving diet of one's forefathers," is interpreted to mean the "health-giving tables of the *better* sort"!

Leaving this portion of the subject, we pass on to briefly notice the diagnoses of the species and the coloured plates. We trust that we are not hypercritical if we suggest that the student should have had some clue to the classification adopted in this portion of the work; such may be given in the introduction, but, if so, we presume it will have to explain the sequence of the species and genera in the text, which surely ought to have explained itself. Thus, under the heading, British Cicadæ, comes the genus Cicadetta, and this is followed by "II. Membracidæ, Stål.," with two genera, Centrotus and Gargara, after which we find "Fulgorinæ, Stål.," with III. Tettigometridæ, IV. Issidæ, and V. Cixiidæ; but the next group, the Delphacidæ, is not numbered, so there is no help given one from the name-endings, or the numerals, or, we may add, the typography, as to what Mr. Buckton regards as a family, or a sub-family, nor can we be quite sure always even of the names that he would adopt for the forms described, as, for example, the species given on pp. 28 and 29.

We have thus pointed out a few of the blemishes that to some slight extent mar the early pages of this important and interesting work, and the literary student could easily point out many more in the already published pages of the introductory chapter, still this part is of but secondary value to the entomologist, and a little more attention to the part containing the diagnoses of the genera and species, and the recording the habitats of *all* the British forms, are points that can be easily attended to in the future parts. Of 41 species of the genus Liburnia recorded as British, twenty-seven have no British habitat quoted, unless, indeed, in some few cases, such phrases as "common on marshy lands," "uncommon in England," and "from Mr. Douglas's collection," are to be regarded as such.

The figures are drawn on stone by the author: those of the perfect insects enlarged are very characteristic and pretty, those of the anatomical details would be improved by a little more distinctness in their outlines.

While the British Cicadæ are thus being monographed by Mr. Buckton, those of the Orient are being monographed by Mr. W. L. Distant, the first two parts of a "Monograph of Oriental Cicadidæ" having been published by order of the trustees of the Indian Museum, Calcutta. Part I. is dated July 1889, and Part II. December 1889. They are in large quarto, each containing twenty-four pages and two plates. We have no words but those of praise for this splendid work, which the trustees of the Indian Museum are to be warmly congratulated on publishing. Mr. Distant leaves us in no doubt as to the forms of which he treats: they belong to the sub-order of the Homoptera, and to the family of the Cicadidæ; for this family he adopts two divisions—the sub-families Cicadinæ and the Tibiceninæ—while the diagnoses of the genera and the species leave nothing to be desired. The typography is excellent. A word of commendation is also due to Mr. Horace Knight, to whom the drawing of the figures from nature has been intrusted; one figure on each plate of a species of each genus is represented in colours.

Mr. Distant proposes in this work to fully describe and figure all the species known from continental India and Ceylon, the islands in the Bay of Bengal, in Burma, Tenasserim, the Malay Peninsula, the length and breadth of the Malayan Archipelago, including, but extending eastward of, New Guinea; and, lastly, Eastern Asia, including China and Japan. Thus this monograph will include all the Oriental species. We trust the author may bring this work, so auspiciously commenced, to a successful issue.

MACHINE DESIGN.

The Elements of Machine Design. By Prof. W. Cawthorne Unwin, F.R.S. (London and New York: Longmans, Green, and Co., 1890.)

THIS is the eleventh edition of an excellent and most useful book for engineers and students in the engineering departments in our technical colleges. Prof. Unwin is so well known in the profession that any work of his is sure to receive full attention and careful study; for even in the present day one unfortunately often sees machinery and engineers' tools, the design and construction of which give us cause to wonder how they manage to work at all. The author is one of those Professors whose books are eagerly sought after by practical men for guidance. To say this is to say very much indeed, for engineers have to make their machines "pay" and creditable to themselves; a bad machine tool in a shop is very soon found out by the repairs it requires, and the quality of the work it can produce.

In this, the new edition of the work, the author has found it necessary to divide the book into two parts, the first of which is now before us. It deals principally with the general principles of design, fastenings, and transmissive machinery.

The author, well knowing the conditions of every-day work in the drawing office and shops, has, we are glad to observe, used throughout the standard English units of weight and length. Another good point is that the mathematics used in the calculations are well within the range of the average engineer; at the same time accuracy is obtained in the results, although useless refinements are omitted.

In the chapters on rivetted joints, and the one on journals and the friction of the same in their bearings, the experimental results obtained from experiments inaugurated by the Institution of Mechanical Engineers are fully described and the results tabulated; and they are embodied in the chapters in many useful forms suitable for the guidance of engineers. Under the heading of rivetted joints, it may be interesting to observe that the question of punching *versus* drilling steel or iron plates has solved itself in, at any rate, one first class bridge works in the north, and in this particular works the invariable practice is to drill all the holes throughout the bridge work because it is cheaper, with suitable machinery, to do so. On p. 97 the author does not say whether his remarks apply to boilers as well as other constructions, but to punch an iron boiler-plate is considered bad practice, and a punched steel plate, even if it is annealed afterwards, certainly comes under the same head.

In most of the locomotive works in this country the boilers are drilled, finally, after all the plates are in position, the barrel being fitted to the fire-box casing after each portion has been drilled; and certainly no good locomotive builder would use a punched steel plate in a boiler, even after annealing. One eminent locomotive superintendent, we believe, uses punched steel boiler shell plates; this is probably the only exception in this country, and is generally considered risky and not sound practice. On p. 140 the system of applying the direct stays to the crown of locomotive fire-boxes might have been added and illustrated with advantage.

The illustrations are particularly good, and all represent good practice. The thanks of engineers are due to Prof. Unwin for placing within their reach a volume in which theory and practice are judiciously treated to their great advantage.

N. J. L.

OUR BOOK SHELF.

Investigation of the Fur-Seal and other Fisheries of Alaska. Report from the Committee on Merchant Marine and Fisheries of the House of Representatives. (Washington: Government Printing Office, 1889.)

THE fisheries of Alaska are among the great questions of the day, and those of our legislators who wish to take part in the inevitable debate on the subject will do well to possess themselves of the present volume, and digest the large amount of information that it contains. As is well known, the fur-seal fisheries of the Northern Pacific, which supply the ladies' jackets so much prized in Europe, are rented by the Alaska Commercial Company, and produce a considerable revenue to the United States. It is therefore a standing grievance among our American friends, that, as shown by the testimony collected in the present Report, the number of seals on the Prybiloff Islands, whence the principal supply is derived, "has materially diminished during the last two or three years." This is attributed to the fact that a large number of British vessels, "manned by expert Indian seal-hunters," have frequented Bering's Sea, and destroyed "hundreds of thousands of fur-seals." It is shown that, of the seals thus killed on the ocean, not more than one in seven is secured, because a wounded seal sinks so quickly. Thus, for every thousand seal-skins realized by the British sealing-vessels, some seven thousand seals are killed. Now, during the three years 1886–88, it appears that the number of what the Americans call "illicit skins" secured by the British traders was over 97,000, so that, if these calculations are correct, it follows that nearly three-quarters of a million of fur-seals were destroyed by British vessels during that period. American citizens, we are told, "have respected the law, and have made no attempt to take the seals."

While we fully sympathize with the Americans in their view that the fur-seal is a most useful animal, and deserves protection by special legislation, it seems to be doubtful whether they have any right, in their praiseworthy efforts in this direction, to turn a large tract of the Northern Pacific into a "*mare clausum*," without obtaining the consent of other nations. But the arguments by which they justify this somewhat strong proceeding are fully set forth in the present volume, and deserve special study. We may also commend Mr. Dunn's Report as containing a large amount of information on the history and habits of *Callorhinus ursinus*, and some excellently drawn illustrations of what the Americans consider to be the only legitimate method of obtaining this animal's skin.

Pond Life: Algæ and Allied Forms. By T. Spencer Smithson. (London: Swan Sonnenschein and Co., 1890.)

"THE Young Collector" series, to which this hand-book belongs, deals generally with classes of objects which can be permanently preserved. The present volume describes plants which, as the author says, "are not well adapted for preservation." His task, therefore, has been to give an account of the structure and habits of these plants, and to explain how they may be procured in the best form for observation. He begins with information about

the apparatus required, then treats of the Algæ as a class, and the main divisions into which they have been separated by botanists, and in most of the remaining part of the book describes species, "choosing as types of each genus such species as are most likely to be met with, and leaving out those which are either rare or possess few points of interest for the beginner." Mr. Smithson himself points out that the volume leaves much to be sought elsewhere; but, if used intelligently, it will do sound work by preparing the way for wider study.

Rambles and Reveries of a Naturalist. By the Rev. William Spiers, M.A., F.G.S., &c. (London: Charles H. Kelly, 1890.)

MR. SPIERS does not profess to give in this little book a full account of any one of the subjects with which he deals. His aim has been "to awaken or to stimulate a love for Nature in the minds of some who may not as yet have suspected what wondrous and ever-varying beauty lies everywhere about us, in ditch and pond, in rock and stone, in river and sea, on earth and in the skies." With this end in view, he describes, in a series of short sketches, various phenomena which he himself has had opportunities of observing; and he does his work so well that to a good many readers his book may be of considerable service. There is nothing new or brilliant in Mr. Spiers's descriptions; but they are fresh and clear, and display not only a genuine love for Nature, but a capacity for appreciating the scientific significance of many different orders of facts. Besides other essays, the volume includes papers on seaweeds, rambles in Cornwall, a visit to the Channel Tunnel, St. Hilda's snake-stones, tiny rock-builders, and an evening at the microscope.

Sketches of British Sporting Fishes. By John Watson. (London: Chapman and Hall, 1890.)

A PREFATORY note to the "Sketches" tells us that "the subject-matter has, for the most part, been gleaned directly from the waterside, and should be looked upon more as the notes of a naturalist than the jottings of an angler." Accordingly, it was with anticipation of interest that we turned to the opening chapter, on salmon.

So little is known of the natural history of the salmon, and so great is its value, both for sport and for food, that we eagerly scan the pages of a naturalist and an angler who may tell us what he has seen and knows. Mr. Watson has nothing to tell us. He disposes of the salmon in 12 pages, and the impression produced upon us is that his acquaintance with that noble fish is confined to the fishmonger's slab, and to the dinner-table.

The chapter on trout is little more satisfactory. That on grayling is by another hand.

LETTERS TO THE EDITOR.

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Coral Reefs, Fossil and Recent.

DR. VON LENDENFELD has (June 12, p. 148) quoted cases to contest my statement that there are no coral reefs whose slopes are known to descend steeply to greater depths than about 4000 feet. I must take these seriatim.

(1) "Fitzroy's no-bottom sounding of 7200 feet at a distance of 6600 feet from the breakers at Keeling Island."

I hope I shall not be misunderstood when I say that I cannot accept this as conclusive evidence. Experience daily shows us how little confidence can be placed in a single deep sounding, taken before the days of suitable apparatus, and with no description of the means employed, either to fix the position exactly, or to obtain the cast. It may be correct, but on the other hand it may not be.

(2) "Maldives, &c., rise from a bank of 1000 fathoms very abruptly."

I cannot find any deep soundings near these groups at all. One sounding of 1243 fathoms at a distance of 10 miles is the closest.

(3) "Bermudas rise abruptly out of a depth of 12,000 to 13,000 feet."

There is only one sounding of 12,000 feet anywhere near Bermuda, and as that is six miles from the nearest shallow water, the isolated Challenger Bank, it represents a slope of only 19°.

In point of fact, very few slopes of coral formations have yet been accurately measured. Among the most remarkable that I know are:—

Bougainville Reef in Coral Sea, which drops perpendicularly from the water-level to 360 feet; at a mean slope of 76° to 780 feet; and at 53° to 1500 feet.

Dart Reef, in same sea, has a mean slope of 64° to 1200 feet.

Macclesfield Bank, a so-called, "drowned" atoll, in China Sea, has a mean slope of 51° to 4200 feet, and possibly more.

The existing conditions of the steep outer slopes of atolls are sufficiently astonishing. All I wish to maintain is that we should argue upon proven facts, and not assumptions, which tend to exaggerate difficulties, and to lead us astray.

With regard to Dr. von Lendenfeld's explanation of the limitation of depths of lagoons, I must await a better before I am convinced. My point is that it is very remarkable that no matter how vast the lagoon, and how deep the steep outer slopes, no lagoon has more than a certain depth, and that such a limited depth that isolated coral heads can spring out of it; and I cannot make this general fact fit with a general theory of subsidence, even when varied by occasional elevations.

The "drowned" atolls are no deeper than others whose rims are at the surface; *vide* Great Chagos Bank, and Suadiva Atoll in Maldives. W. J. L. WHARTON.

June 14.

ELECTRO-MAGNETIC RADIATION.[1]

IN order to discover whether actions are propagated in time or instantaneously, we may employ the principle of interference to measure the wave-length of a periodic disturbance, and determine whether it is finite or no. This is the principle employed by Hertz to prove experimentally Maxwell's theory as to the rate of propagation of electro-magnetic waves. In order to confine the experiments within reasonable limits we require short waves, of a few metres' length at most. As the highest audible note gives waves of five or six miles long, and our eyes are sensitive only to unmanageably short waves, it is necessary to generate and observe waves whose frequency is intermediate between them, of some hundred million vibrations per second or so. For this purpose we may use a pair of conducting surfaces connected by a shorter or longer wire, in which is interposed a spark-gap of some few millimetres' length. When the conductors are charged by a coil or electrical machine to a sufficiently high difference of potential for a spark to be formed between them, they discharge in a series of oscillations, whose period for systems of similar shape is inversely proportional to the linear dimensions of the system so long as the surrounding medium is unaltered. When the surrounding non-conducting medium changes, the period depends on the electric and magnetic specific inductive capacities of this medium. Two such systems were shown: a large one, whose frequency was about 60 millions per second; and a small one, whose frequency was about 500 millions per second. The large one consisted of two flat plates, about 30 cm. square and 60 cm. apart, and arranged in the same way as is described by Prof. Hertz in *Wiedemann's Annalen*, April 1888. The

[1] Friday Evening Lecture delivered at the Royal Institution, on March 21, by Prof. G. F. Fitzgerald, F.R.S.

smaller vibrating system consisted of two short brass cylinders terminating in gilt brass balls of the same size, and arranged in the same way as the smaller system described by Prof. Hertz in *Wiedemann's Annalen*, March 1889. This latter system was placed in the focal line of a cylindrical parabolic mirror of thin zinc plate, such as that described by Prof. Hertz in this paper.

These generators of electro-magnetic oscillations may be called electric oscillators, as the electric charge oscillates from end to end. A circle of wire, or a coil in which an alternating current ran, or, if such a thing were attainable, a magnet alternating in polarity, might be called a magnetic oscillator. A ring magnet with a closed magnetic circuit is essentially an electric oscillator, while a ring of ring magnets would be essentially a magnetic oscillator again. The elementary theory of a magnetic oscillator can be derived from that of an electric oscillator by simply interchanging electric and magnetic force. Electricity and magnetism would be essentially interchangeable if such a thing existed as magnetic conduction. The only magnetic currents we know are magnetic displacement currents and convection currents, such as are used in unipolar and some other dynamos. It is in this difference that we must look for the difference between electricity and magnetism.

In order to observe the existence of these electro-magnetic oscillations we can employ the principle of resonance to generate oscillations in a system whose free period of oscillation is the same. A magnetic receiver may be employed, consisting of a single incomplete circle of wire broken by a very minute spark-gap, across which a spark leaps when the oscillations in the wire become sufficiently intense. In order that a large audience may observe the occurrence of sparks, the terminals of a galvanometer circuit were connected, one with one side of the spark-gap, and the other with a fine point which could be approached very close to the other side of the spark-gap. It was observed that, when a spark occurred in the gap, a spark could also be arranged to occur into the galvanometer circuit, and, with a delicate long-coil galvanometer (that used had 40,000 ohms resistance), a very marked deflection can be produced whenever a spark occurs. This arrangement we have only succeeded in working comparatively close to the generator, because the delicacy required in adjusting the two spark-gaps is so great. It can, however, be employed to show that the sparks produced in this magnetic resonant circuit are due to resonance by removing this receiver from the generator to such a distance that sparks only just occur, and then substituting for the single circuit a double circuit, which, except for resonance, should have a greater action than the single one, but which stops the sparking altogether. An electric receiver was also used, which was identical with the generator, and had a corresponding, only much smaller, spark-gap between the two plates. When the plates are connected with the terminals of the galvanometer, upon the occurrence of each spark the galvanometer is deflected. It is not so easy to obtain sparks when the plates are connected with the galvanometer as when they are insulated, and it is this that has limited the use of this method of observation. By making the first metre or so of the wires to the galvanometer of extremely fine wire, so as to reduce their capacity, we have found that the difficulty of getting sparks is less than with thick wires. We have not observed any effect due to the thickness of the wires after a short distance from the receiver.

In the case of the small oscillator, a receiver exactly like the one described by Prof. Hertz in his second paper already quoted was placed in the focal line of a cylindrical parabolic mirror, and its receiving wires were connected with the wires leading to the galvanometer by some very fine brass wire. With the large-sized generator and receiver, which were placed about 3 metres apart, it was shown that the sparking was stopped by placing a thin zinc sheet so as to reflect the radiations from a point close behind the receiver. By means of a long india-rubber tube hung from the ceiling, it was shown how, when waves are propagated to a point whence they are reflected, the direct and reflected waves interfering produce a system of loops and nodes, with a node at the reflecting point. It was explained that these nodes, though places of zero displacement, were places of maximum rotation, and that the axis of rotation was at right angles to the direction of displacement. It was explained that an analogous state of affairs existed in the electro-magnetic vibrations. If the electric force be taken as analogous to the displacement of the rope, the magnetic may be taken as analogous to its rotation, and the two are at right angles to one another. In the ether the electric node is a magnetic loop, and *vice versâ*. Though the two are separated in loops and nodes, they exist simultaneously in a simple wave propagation, just as in a rope when propagating waves in one direction the crest of maximum displacement is also that of maximum rotation. It was explained that by placing the reflector at a quarter of a wave-length from the receiver this would be at an electric loop, and have its sparking increased. It may thus be shown that there are a series of loops and nodes produced by reflection of these electromagnetic forces, like those produced in any other case of reflected wave-propagation. This was Hertz's fundamental experiment, by which he proved that electro-magnetic actions are propagated in time, and by some approximate calculations he verified Maxwell's theory that the rate of propagation is the same as that of light. It follows that the luminiferous ether is experimentally shown to be the medium to which electric and magnetic actions are due, and that the electro-magnetic waves we have been studying are really only very long light waves.

A rather interesting deduction from Maxwell's theory is that light incident on any body that absorbs or reflects it should press upon it and tend to move it away from the source of light. Illustrating this, an experiment was shown with an alternating current passing through an electro-magnet, in front of which a good conducting plate of silver was suspended. When the alternating current was turned on the silver was repelled. It was explained that as the silver could only be affected by what was going on in its own neighbourhood, and that if sufficiently powerful radiations from a distant source were falling on the silver, it would be acted on by alternating magnetic forces, this experiment was in effect an experiment on the repulsion of light, which was too small to have been yet observed, even in the case of concentrated sunshine. These slow vibrations are not stopped by a sheet of zinc, though much reduced by a magnetic sheet like tin-plate, though the rapid ones are quite stopped by either—thus showing that wave-propagation in a conductor is of the nature of a diffusion.

In all cases of diffusion where we consider the limits of the problem, terms involving the momentum of the parts of the body must be introduced. It appears from elementary theories of diffusion as if it were propagated instantaneously, but no action can be propagated from molecule to molecule, in air, for instance, faster than the molecules move, *i.e.* at a rate comparable with that of sound. In electro-magnetic theory corresponding terms come in by introducing displacement currents in conductors, and it seems impossible but that some such terms should be introduced, as otherwise electro-magnetic action would be propagated instantaneously in conductors. The propagation of light through electrolytes, and the too great transparency of gold leaf, point in the same direction.

The constitution of these waves was then considered, and it was explained that if magnetic forces are analogous to the rotation of the elements of a wave, then an ordinary

solid cannot be analogous to the ether because the latter may have a constant magnetic force existing in it for any length of time, while an elastic solid cannot have continuous rotation of its elements in one direction existing within it. The most satisfactory model, with properties quite analogous to those of the ether, is one consisting of wheels geared with elastic bands. The wheels can rotate continuously in one directionn, and their rotation is the analogue of magnetic force. The elastic bands are stretched by a difference of rotation of the wheels, and introduce stresses quite analogous to electric forces. By making the elastic bands of lines of governor balls, the whole model may have only kinetic energy, and so represent a fundamental theory. Such a model can represent media differing in electric and magnetic inductive capacity. If the elasticity of the bands be less in one region than another, such a region represents a body of higher electric inductive capacity, and waves would be propagated more slowly in it. A region in which the masses of the wheels was large would be one of high magnetic inductive capacity. A region where the bands slipped would be a conducting region. Such a model, unlike most others proposed, illustrates both electric and magnetic forces and their inter-relations, and consequently light propagation.

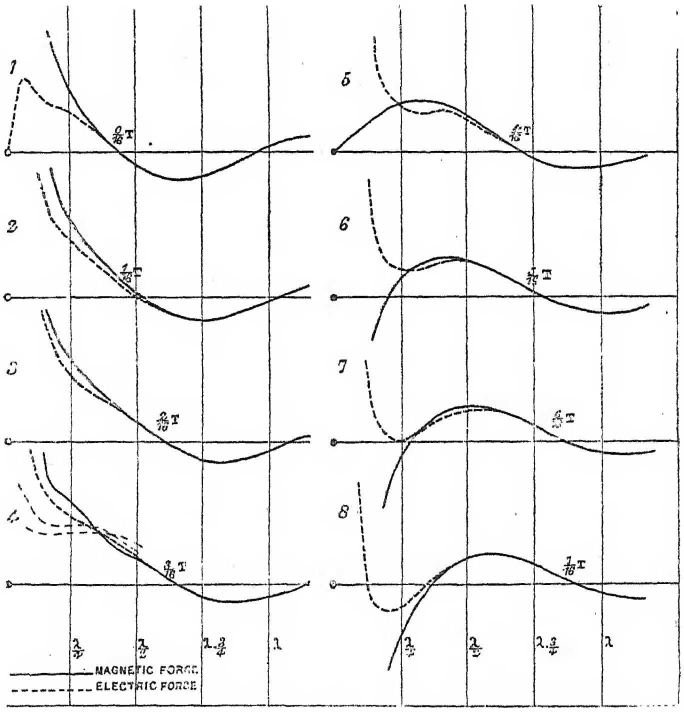
In the neighbourhood of an electric generator the general distribution of the electric and magnetic forces is easily seen. The electric lines of force must lie in planes passing through the axis of the generator, while the lines of magnetic force lie in circles round this axis and perpendicular to the lines of electric force. It is thus evident that the wave is, at least originally, polarized. To show this, the small-sized oscillators with parabolic mirrors were used, and a light square frame, on which wires parallel to one direction were strung, was interposed between the mirrors. It was shown that such a system of wires was opaque to the radiation when the wires were parallel to the electric force, but was quite transparent when the frame was turned so that the wires were parallel to the magnetic force. It behaved just like a tourmaline to polarized light. It is of great interest to verify experimentally Maxwell's theory that the plane of polarization of light is the plane of the magnetic force. This has been done by Mr. Trouton, who has shown that these radiations are not reflected at the polarizing angle by the surface of a non-conductor, when the plane of the magnetic force in the incident vibration is perpendicular to the plane of incidence, but the radiations are reflected at all angles of incidence when the plane of the magnetic force coincides with the plane of incidence. Thus the long-standing dispute as to the direction of vibration of light in a polarized ray has been at last experimentally determined. The electric and magnetic forces are not simultaneous near the oscillator. The electric force is greatest when the electrification is greatest, and the magnetic force when the current is greatest, which occurs when the electrification is zero: thus the two, when near the oscillator, differ in phase by a quarter of a period. In the waves, as existing far from the oscillator, they are always in the same phase. It is interesting to see how one gains on the other. It may be worth observing, again, that though what follows deals with electric oscillators, the theory of magnetic oscillators is just the same, only that the distribution of magnetic and electric forces must be interchanged. Diagrams drawn from Hertz's figures published in *Wiedemann's Annalen* for January 1889, and in NATURE, vol. xxxix. p. 451, and in the *Philosophical Magazine* for March 1890, were thrown on the screen in succession, and it was pointed out how the electric wave, which might be likened to a diverging whirl ring, was generated, not at the oscillator, but at a point about a quarter of a wave-length on each side of the oscillator, while it was explained that the magnetic force wave starts from the oscillator. It thus appears how one gains the quarter-period on the other. The outflow of the waves was exhibited by causing the images to succeed one another rapidly by means of a zoëtrope, in which all the light is used and the succession of images formed by having a separate lens for each picture and rotating the beam of light so as to illuminate the pictures in rapid succession.

As the direction of flow of energy in an electro-magnetic field depends on the directions of electric and magnetic force, being reversed when either of these is reversed, it follows that in the neighbourhood of the oscillator the energy of the field alternates between the electric and magnetic forms, and that it is only the energy beyond about a quarter of the wave-length from the oscilllator which is wholly radiated away during each vibration. It follows that in ordinary electro-magnetic alternating currents at from 100 to 200 alternations per second, it is only the energy which is some 3000 miles away which is lost. If an electro-magnetic wave, having magnetic force comparable to that near an ordinary electro-magnet, were producible, the power of the radiation would be stupendous. If we consider the possible radiating power of an atom by calculating it upon the hypothesis that the atomic charge oscillates across the diameter of the atom, we find that it may be millions of millions of times as great as Prof. Wiedemann has found to be the radiating power of a sodium atom in a Bunsen burner, so that, if there is reason to think that any greater oscillation might disintegrate the atom, it is evident that we are still a long way from doing so. It is to be observed that ordinary light-waves are very much longer than the period of the vibration above referred to. Dr. Lodge has pointed out that quite large oscillators in comparison to molecules—namely, about the size of the rods and cones in the retina—are of the size to resound to light-waves of the length we see, and so might be used to generate such waves. This seems to show that the electro-magnetic structure of an atom must be more complicated than a small sphere or other simple shape with an oscillating charge on it, for the period of vibration of a small system can be made long by making the system complex, *e.g.* a small Leyden jar of large capacity with a long wire wound many times round connecting its coats, could easily be constructed to produce electro-magnetic waves whose length would bear the same proportion to the size of the jar as ordinary light-waves do to an atom. The rate at which the energy of a Hertzian vibrator is transferred to the ether is so great that we would expect an atom to possess the great radiating power it has. This shows, on the other hand, how completely the vibrations of an atom must be forced by the vibrations of the ether in its neighbourhood, so that atoms, being close compared with a wave-length, are, in any given small space, probably in similar phases of vibration. It is interesting to consider this in connection with the action of molecules in collision as to how far the forces between molecules after collision is the same as before. In the same connection the existence of intra-atomic electro-magnetic oscillations is interesting in the theories of anomalous dispersion. An electro-magnetic model of a prism with anomalous dispersion might be constructed out of pitch, through which conductors, each with the same rate of electro-magnetic oscillation, were dispersed. In theories of dispersion a dissipation of energy is assumed, and it may be the radiation of the induced electro-magnetic vibrations. These can evidently never be greater than the incident electro-magnetic vibration, on account of this radiation of their own energy. In some theories a vibration of something much less than the whole molecule is assumed, and the possibility of intra-atomic electro-magnetic oscillations would account for this. Some such assumption seems also required, in order to explain such secondary, if not tertiary, actions as the Hall effect and the rotation of the plane of polariza-

tion of light, which are, apparently at least, secondary actions due to a reaction of the matter set in motion by the radiation on this radiation.

Some further diagrams were exhibited, plotted from Hertz's theory by Mr. Trouton, to whom much of the matter in this paper is due. They are here reproduced, and show eight simultaneous positions of the electric and magnetic waves during a semi-oscillation of an electric oscillator. The dotted line shows the electric force at various points, and the continuous line the magnetic force. In the first diagram the magnetic force is at its maximum near the origin, while the electric force there is zero. In the second the magnetic energy near the origin has partly turned into electric energy, and consequently electric force begins. The succeeding figures show how the magnetic force decreases near the origin, while the electric force grows, and the waves already thrown off spread away. The change of magnetic force between Figs. 4 and 5 is so rapid, that a few dashed lines, showing interpolated positions, are introduced to show how it proceeds. It will be observed how a hollow comes in the line showing electric force, which gradually increases, and, crossing the line of zero force at about a quarter of a wave-length from the origin, is the source of the electric wave, which, starting with this odds, picks up and remains thenceforward coincident with the magnetic wave. From this origin of electric waves they spread out along with the magnetic waves and in towards the origin, to be reproduced again from this point on the next vibration. These electric and magnetic forces here shown as coincident are, of course, in space in directions at right angles to one another, as already explained. The corresponding diagrams for a magnetic oscillator are got by interchanging the electric and magnetic forces.

A further experiment was shown to illustrate how waves of transverse vibration can be propagated along a straight hollow vortex in water. It was stated that what seemed a possible theory of ether and matter was that space was full of such infinite vortices in every direction, and that among them closed vortex rings represented



matter threading its way through the ether. This hypothesis explains the differences in Nature as differences of motion. If it be true, ether, matter, gold, air, wood, brains, are but different motions. Where alone we can know what motion in itself is—that is, in our own brains—we *know* nothing but thought. Can we resist the conclusion that all motion is thought? Not that contradiction in terms, unconscious thought, but living thought; that all Nature is the language of One in whom we live, and move, and have our being.

THE CLIMATES OF PAST AGES.[1]

II.

We need not enter on a detailed description of the other vegetable types of the Coal-measure formation; we can only note the abundant occurrence of tree-ferns, and the existence of not very numerous conifers, which amid this strange vegetation are the forms most nearly related to those of our present world.

The geographical extent of this typical flora was extraordinarily great; we trace it from the shores of the Atlantic through the northern half of the Old World to China, and it is also greatly developed in the eastern half of the United States. There, and in China, are the greatest developments of beds of coal. Besides these, we find similar deposits with nearly the same vegetation in the far north, in the American polar archipelago, in Spitzbergen, and Nova Zembla. It is these facts that have led to the conclusion, already mentioned, that in the Carboniferous period a uniform climate prevailed from the equator to the pole, together with a dense atmosphere rich in carbon-dioxide, and impenetrable to the solar rays. And yet a simple examination of the facts assures us that all these suppositions are groundless. In so far as regards the character of the flora, we really know nothing of the temperature requisite to the Calamites, Lepidodendra,

[1] Translation of a Lecture delivered by the late Dr. M. Neumayr before the Society for the Dissemination of Natural Science, at Vienna, on January 2, 1889. Continued from p. 151.

Sigillariæ, and other extinct types. Conifers grow now in very severe climates, and only the tree-ferns really indicate warm climatic conditions. At the present day their chief development is in the tropics, and they require, not indeed great heat, but the absence of frost. We do not, however, know that this was equally the case in former ages; in the Carboniferous period, the highest division of the vegetable kingdom, now so dominant, the flowering plants, were either non-existent, or were sparsely represented only by a few early forms, and it is by no means improbable that these types in their gradual extension have exterminated the tree-ferns in the colder regions to which they formerly extended, and that these latter have lost the power which they once possessed of withstanding frost.

Another fact that has been adduced to prove the former prevalence of a warm climate, is the great thickness of the beds of coal, which, it was assumed, could only have been formed by a luxuriant vegetation stimulated by a high temperature. But this also is incorrect; remarkably rich plant-growths are to be met with also in countries with very severe climates, and indeed few countries surpass, in this respect, the inhospitable Terra del Fuego, with its impenetrable beech forests. Moreover, there is no good ground for the assumption that a luxuriant growth of plants is necessary for the formation of thick beds of fossil fuel. At this present time we know of but one mode in which vegetable remains accumulate in thick beds, and thus exhibit to us the first step of the process of coal formation: this is the formation of peat, which, as is well known, is effected by the most inconspicuous and poorest of plants, viz. certain kinds of mosses. It is not in the towering primæval forests of India and Brazil, nor the mangrove swamps of tropical coasts, but in the moors of the sub-arctic zone, that plant-remains are now being stored up in a form that, in the course of geological ages, may become converted into beds of coal.

A closer examination of these conditions apprises us of certain important facts. The reason why great masses of vegetable remains do not accumulate in warm countries is that, in the presence of a high temperature the decaying plants decompose too rapidly, and speedily disappear; it is only in a cold climate that they are preserved; and we may therefore regard the existence of coal-beds as a proof that at the time of their formation a high temperature did *not* prevail.

Out of the mass of baseless assumptions, then, this tolerably well-founded fact remains, that an arborescent vegetation of the Carboniferous period presents itself in 76° of northern latitude, whereas, at the present day the northern limit of tree-growth nowhere exceeds 72°; and if we assume that there has been no displacement of the earth's axis of rotation, we must conclude that in these high latitudes the mean temperature of the year was formerly some degrees warmer than at this present time; in the temperate zone we may infer, with some probability, a cool climate with moderate heat in the summer and cold in the winter, and with but little frost: in fact, an insular climate, such as our knowledge of the distribution of land and sea in that age presupposes.

So far we have regarded only the conditions obtaining in the north temperate zone and the polar regions. These, however, show certain peculiarities of distribution. The greatest coal deposits are all in the temperate zone, and chiefly concentrated in its middle and northern regions. The most northerly of the great deposits of the productive Coal-measures are those of Scotland, the most southerly those on the border of the central plateau of France; such as lie further north or south are of little importance. In North America, it is true, they extend considerably further south, but none reach to the 30th parallel of latitude; while, in the north, they extend into British North America. The coal of China occurs in the northern provinces, in Shansi, Shensi, and Honan.

Thus we find that the greater deposits are restricted to a zone of variable width, the southern limits of which are between 30° and 45°, the northern between 50° and 60° N. lat.; beds of true coal of the same age are not indeed entirely wanting outside these limits, but they are rare; as a rule we meet with only the characteristic plants, and these gradually disappear as we proceed further south. In a few instances they may be traced as far as Northern Africa and the peninsula of Sinai; but between the tropics the typical flora of the coal formation seems to fail entirely; not a single instance of their occurrence can be cited; and their first reappearance seems to be in the southern temperate zone in the coal-fields of Southern Brazil.

For a long time it was very doubtful what explanation should be given of this phenomenon, whether plant-bearing deposits of this age were altogether wanting in the tropical zone, or whether their development was of so different a character that we had failed to identify them, or finally whether it were due to some other cause. We cannot notice at length the gradual development of our knowledge on this head; we can only sketch out the final results which have been yielded in the last few years. We know now, that in Southern Africa, in India, and Australia, there are extensive deposits of the same age as our productive Coal-measures, with abundant plant-remains, but that these differ very greatly from the contemporaneous growths of our own region. No trace is found of the forms characteristic of our Coal-measures, no Sigillariæ, no Lepidodendrons, no Calamites. Ferns and true Equisetaceæ furnish by far the greater part of the flora; and with these are associated a small number of conifers and Cycads. The commonest and most characteristic form of this flora is the fern genus *Glossopteris*, and accordingly the whole assemblage of associated plants has been termed the *Glossopteris* flora. When put in comparison with our European coal flora, so strange does this seem, that no one would venture to think of it as contemporary until it had been established, by evidence admitting of no question, that such is actually the case.

From this, however, the important result follows that the doctrine of a universal coal flora is altogether false. On the contrary, we find that we have to deal with two very different floral regions, which stand strongly contrasted. And what makes this contrast especially remarkable, and for a long time hindered its true interpretation, is that the *Glossopteris* flora of India, Australia, and South Africa is nearly related to the European flora of a much later period, viz. the Trias.

But the most striking fact connected with this flora is that its first appearance, whether in South Africa, India, or Australia, is associated with deposits of fine argillaceous sand, with numerous stony fragments varying in size from small pebbles to gigantic blocks of many hundredweight, irregularly embedded; they consist for the most part of rocks that do not occur anywhere in the neighbourhood, and must therefore have been transported from a distance, and moreover some among them are scored and scratched. These phenomena, which manifest themselves in three far-distant localities, and according to the latest intelligence seem to recur also in Brazil, bear such striking evidence of the agency of ice in the formation of these deposits, that any doubt on this head seems scarcely any longer admissible, however much it may startle us to find great ice-masses and floating icebergs at the time of the coal formation in regions so far from the poles.

From the facts we have recounted, bearing on the climate of the Coal-measure period, it is abundantly manifest that everything runs counter to the assumption of a uniform

and warm terrestrial climate from the equator to the poles. Geographically we have sharply contrasted floras, and we have moreover widely distributed deposits, in the formation of which great masses of ice must have played a part, and thus the old views are utterly overthrown. But when we go further, and seek to learn from the facts before us what the conditions really were, we are quickly admonished that our knowledge is as yet far too small to admit of any definite representation of these conditions. We may say with much probability that the differences of the floral regions must be ascribed to differences of climate, and that, locally, the temperature was so low as to allow of the formation of great masses of ice; but anything beyond this is quite uncertain, and no one of the assumptions that have been made to explain the conditions of that epoch has any claim to validity. Those early ages present us with so much that is strange to us, the unknown is so vast in comparison with what we know, that we dare not as yet attempt any generalization of our knowledge.

We pass over the formations which succeed the Coal-measures, viz. the Permian, the Trias, the Jura, and the Chalk, and after this enormous interval we turn our attention to Tertiary times. Here begin those modern developments that have resulted in our present world; the chief types of animals and plants are the same as those of our own day; and it is only since the beginning of Tertiary times that mammals predominate among the fauna of the land, whereas in the previous formations this leading part had been played by reptiles.

At that time Europe was far more cut up by inland seas than it now is, and formed a dismembered assemblage of islands and peninsulas. In the first division of the Tertiary age, the Eocene, the seas around its coasts were tenanted by animals of a tropical character. In the later subdivisions, this character was gradually lost. In the Oligocene, a marine fauna of a tropical character extends only to a line which about coincides with the northern limit of the Alps. In the Miocene, which next follows, the fauna even of this part of Europe is, at the utmost, sub-tropical; and, by degrees, the forms which give evidence of a warm climate gradually diminish, so that towards the end of the last division, the Pliocene, the conditions were almost the same as to-day.

What we know of the land organisms agrees entirely with these indications afforded us by the marine fauna, at least in their leading characteristics, since we equally find, at the beginning of Tertiary times in Europe, a predominance of sub-tropical and tropical types, which, later on, were replaced by a flora representative of a temperate climate. In detail, indeed, there are many and not unimportant deviations. Thus, for instance, the flora the remains of which are preserved in the calcareous tufa of Sezanne in Champagne, or in the marls of Gelinde, belongs to the Lower Eocene. The forms here represented are such as at the present time are peculiar to the southern part of the temperate or the sub-tropical zone; numerous evergreen oaks, laurels, cinnamon and camphor trees, various Myrtaceæ, Araliaceæ, figs, magnolias, &c.; many forms point decidedly to a tropical climate, but among them we find also, walnut trees, limes, alders, willows, ivy, and vines, which have an opposite character. Palms and cycads, the specially characteristic forms of hot climates, are absent, or at any rate have not been detected. On the whole, botanists are inclined to infer for that epoch in Central Europe such a climate as now obtains in Southern Japan in 33° N. latitude.

We meet first with truly tropical floral characters in somewhat later deposits, viz. in the Middle and Upper Eocene. At that time there flourished on the mainland and islands of Europe great palms and a number of other plants, whose nearest relatives now exist in tropical Africa, India, and Australia. To judge from the land flora, there was then a maximum of warmth in our neighbourhood (Vienna), from which up to the end of Tertiary times a continuous fall took place. In the Oligocene and Lower Miocene the prevailing character is still that of a tropical or sub-tropical region, but the number of forms that now live in temperate regions has considerably increased; such as now live in Australia occur in remarkable quantity. Then in the Upper Miocene of Central Europe we meet with a flora such as at the present day characterizes the warmer parts of the temperate zone, and in which forms allied to the present flora of North America are especially prominent. In the Pliocene, the latest subdivision of the Tertiaries, the change has progressed still further, and at its end we find in our neighbourhood an assemblage of plants nearly recalling that of the present day, with but a slight intermixture of those of warmer regions.

We may grant generally that these facts prove the existence in Tertiary times of a warmer climate than now prevails in Europe, even though there may be great differences of opinion as to the amount of the difference. Heer, to whom we are indebted for the most important investigations of this subject, has endeavoured to determine the mean annual temperature at certain definite geological epochs from the characters of their respective floras. He found that on the northern border of the Alps in Switzerland, at the epoch of the Upper Oligocene, there was a mean temperature of between 20° and 22° C. (68°–72° F.), such as at the present day is that of Cairo, Tunis, Canton, or New Orleans; at the time of the Upper Miocene, one of 18° or 19° C. (64°–66° F.), corresponding to that of Messina, Malaga, Madeira, and Nagasaki; whereas at the present time the mean annual temperature of Zurich is 8°·73 (47°·7 F.), that of Geneva 9°·67 C. (49°·4 F.). But whereas Geneva and Zurich now lie high above sea-level, we have proofs that in Tertiary times the sea-level was much higher in that neighbourhood than now, therefore that this flora grew at a small height above the sea, which would imply alone an increase of about 3° C. ($5\frac{1}{2}$° F.) of temperature. It follows, then, that at the time of the Upper Oligocene the temperature was about 9° C. (16° F.), in that of the Upper Miocene about 7° C. (12° F.) higher than at present.

With respect to these figures, we must, however, bear in mind that in such computations no allowance is made for the acclimatization of species and whole genera in the course of long geological periods, and therefore that the assigned variations of temperature are almost certainly oo high. Moreover, we must remember that, at that time, tEurope was far more than now interpenetrated by inland seas and straits, and therefore that its climate was more insular, the summers being cooler and the winters warmer than now. But whatever weight we give to these considerations, they are alone insufficient to account for the whole of the difference between the Eocene and the present floras. We must perforce admit that other and deeper-lying causes have co-operated in producing the observed differences.

The examination of the Tertiary floras of high northern latitudes leads us very decisively to a similar conclusion. The various English, American, Danish, and especially the Swedish expeditions have discovered in numerous localities the Tertiary plant-remains of the polar regions, the floras of which have been worked out by Heer. Places which are now among the coldest known spots of the earth have yielded the remains of a rich forest vegetation; nay, within the polar circle itself are found plants which at the present time find even our own latitudes too cold for them. The most northern point from which we have plant impressions is Grinnell Land in the North American archipelago, in 81°45′ N. lat. Its present mean annual temperature is about – 20° C. (4° F.). The flora consists chiefly of conifers, among which are our common pine, two species of fir, and the American swamp cypress (*Taxodium distichum*); with these are associated elms, limes, birches, poplars, hazel, and some others, the

temperature requisite for which is estimated at about 8° C. (46° F.).

Much richer is the fossil flora of Spitzbergen, between [illegible]d $78\frac{2}{3}$° N. lat. Here also conifers are dominant; among foliage trees are present several poplars, also willows, alders, beeches, birches, large-leaved oaks, elms, plane trees, walnuts, magnolias, maples, and others; accordingly the climate of Spitzbergen at that time must have been much the same as the present climate of Northern Germany. A still warmer climate is indicated by the fossil flora of Greenland, which may be compared with the present flora of the shores of the Lake of Geneva.

These are by no means the only instances of a similar kind; analogous discoveries have been made at many different points in high northern latitudes; for instance in Siberia on the lower Lena, on the New Siberian Islands, in Kamtschatka, Alaska, Sitka, Banks Land, and some other points. It is not yet certainly determined to what part of the Tertiary period these fossil remains belong. While some regard them as Miocene or Upper Oligocene, others consider them to be Eocene; and good reasons may be assigned for both these opinions. Whatever may be the final decision is for our present purpose a matter of minor importance. The point we have to insist on is that in the polar regions, the mean temperature of which is now below the freezing-point, and in which only some of the lowest plants exist, there was in Tertiary times a rich forest growth. The difference between those times and the present was so great that for Grinnell Land we cannot estimate it as less than 27° C. (49° F.).

Such a change is absolutely inconceivable so long as we continue to regard as unalterable the present position of the places in question with reference to the pole. We cannot imagine any change in the distribution of land and water, in marine currents, or in any other influential factor, which, at a time comparatively so little distant from the present, could have brought about a luxuriant forest growth in Grinnell Land. This has long been recognized, and in many quarters it has been contended that the only explanation possible is a displacement of the earth's axis of rotation. To this the answer has been that the stations that have yielded the Tertiary plant-remains form a circuit around the pole, a chain from which, as an English geologist has expressed it, the pole can no more escape than a rat from a trap in a ring of terriers.

In point of fact there is no need for assuming so considerable a displacement of the pole since the beginning of Tertiary times. There is, however, ample room within the circle of the northern Tertiary plant stations for such a change, and there are valid grounds for such an assumption. For nowhere do the Tertiary plants reach so far north, and yet nevertheless testify so strongly to the existence of a warm climate as in the quadrant in which lie Grinnell Land, Greenland, and Spitzbergen; when we pass over to the opposite quadrant we find precisely the opposite case, for the Tertiary plants of Alaska, in North-Western America have, in north latitude 60°, scarcely more the character of a southern flora than those of Spitzbergen in lat. 78°.

From these considerations, it seems not improbable that, at the time when these Tertiary plants lived, the pole really had not the same position as now, but was displaced from 10° to 20° in the direction of North-Eastern Asia. The circumstances of the Tertiary deposits in other places outside the polar regions agree very well with this view. In Europe, as we have seen, a very warm climate prevailed universally, but when we turn to other countries we meet with a different result. The flora of the Tertiary formations of the United States give no indication of any essential increase of temperature, and the fossil plants of the probably Miocene and Pliocene formations of Japan, according to the admirable investigations of Nathorst, point to a colder climate than that which now prevails. These facts are obviously eminently favourable to the idea of a displacement of the pole. Curiously enough, we find in the yet but little known Tertiary deposits of the southern hemisphere a somewhat striking confirmation of this view, inasmuch as the marine Tertiary Mollusca which occur in several parts of the Chili coast, do not contain a single species indicative of a warmer climate than that of the present day.

Thus, then, it seems very probable that the position of the pole in Tertiary times was different from that of to-day, and only became as at present at the close of that era. But on this assumption the extreme contrasts are only somewhat palliated, the greater divergences somewhat reduced: no complete explanation is afforded of the phenomena. Whatever position we may assign to the pole, those places in which Tertiary forest trees are found were in any case far nearer to it than is the present northern limit of tree-growth; and when we compare the fossil floras of Europe and Japan, we find that the first shows a much greater departure from the present state of things in the direction of a warmer climate than does the latter in the opposite direction. Thus we are led to the conclusion that the climate of Tertiary times in general was somewhat warmer than that of our own day, but by no means to such an extent as that of the lands specially favoured through the displacement of the pole, viz. Grinnell Land, Greenland, Spitzbergen, and Western and Central Europe.

When from the Tertiary age we take another step forward in time, and reach the Pleistocene, the immediate forerunner of our present age, we meet with quite another picture. The remarkable characteristics of this period have been set forth by a skilled hand in this place, and I need only refer to them in a few words and in so far as is specially important in connection with our present subject.

At the setting in of the Pleistocene, the climate seems to have been somewhat warmer than at present: figs, laurels, and vines grew wild in Central Europe, and among animals, we meet with certain fresh-water Mollusca (*Cyrena fluminalis*) which afford a similar indication. Then followed through the greater part of the Pleistocene that extension of enormous ice masses, which, issuing from Scandinavia, Finland, and the Russian Baltic provinces, covered a great part of Europe and advanced to England, Holland, the base of the mountains of Central Germany, the Carpathians, and in Russia as far as Kiew, Woronesch, and Nishni Novgorod. England, Scotland, and Ireland were almost completely glaciated, the ice-sheet covered nearly the whole Alpine region, a broad ice-girdle lay in front of its northern base, and even the small hill-ranges of Central Europe and some of the greater ranges of Southern Europe developed independent glaciers. On a still greater scale, similar phenomena present themselves to us in North America, and in Northern Asia the greater mountains were then glaciated. Also further south, in the Himalaya and the Karakorum were enormous glaciers, and the same in the neighbourhood of the equator in the Sierra di Santa Martha in the northern part of South America. In the southern hemisphere, traces of glaciers occur very extensively in the southern part of the same continent, and according to many accounts also in South Africa.

It was long doubtful whether the glaciation of the northern and southern hemisphere took place simultaneously; but there is now no longer any doubt that such was really the case. Attempts have been made to explain the formation of great ice-accumulations without any depression of temperature, nay even in warm climates, solely as the result of an excessive precipitation of rain and snow, and in consequence of the prevalence of warm winters and cool summers; but these views are wholly

untenable; a depression of temperature is testified to, not only by the extension of the glaciers, but also by the vegetable and animal denizens of the land and the sea. When in Pleistocene deposits of the Mediterranean basin we find Mollusca suddenly appear which now live only in the German Ocean, no other explanation is possible than that the temperature at that time was low.

We need not indeed conclude that an excessive degree of cold was necessary to produce the phenomena of the Glacial period; the height of the snow-line at that time has been computed for many of the mountains of Europe, and from this it has been deduced that the extreme reduction of temperature was at the utmost 6° C. (11° F.), and possibly considerably less. Much has been said and written of the causes which brought about the cold of the Glacial period. Very thoughtful and also very jejune hypotheses have been put forward, all of which have this one characteristic in common, that in some one particular or another they are strongly opposed to the actual facts, and have therefore no validity. With our present knowledge, any explanation is quite impossible. We must content ourselves with recognizing that the cooling was simultaneous, and, as far as research has yet gone, extended over the whole of the globe. It is, then, obviously impossible to attribute it to a displacement of the pole, for in that case a part of the earth must have experienced an increase of temperature; and, in addition to this, we certainly cannot suppose any considerable change in the position of the pole within so comparatively short an interval as separates us from the Glacial epoch. The uniform extension of the phenomenon excludes all those attempted explanations which appeal to geological or geographical changes of the earth's surface, a different distribution of land and sea, changes in the ocean currents, &c., and all points to some agency external to the earth, and therefore acting on it as a whole.

We must specially notice one other circumstance in connection with the Glacial period. It has been observed in many places that the glacial deposits with their scratched pebbles and irregular heaping of their materials do not form a continuous mass, but that, between a lower and upper deposit of glacial character, there is an intermediate bed showing no trace of ice action; at different places, the remains of animals and plants have been met with in this intermediate bed which indicate a somewhat warmer climate, though slightly colder than the present. Thus, in the slaty coal of Utznach and Dürnten in Switzerland, which belongs to this formation, have been found only the remains of plants still growing in the neighbourhood, with the single exception of the mountain pine, which no longer exists in the low plains of Switzerland, but has withdrawn to Alpine heights. These so-called inter-glacial deposits attain in places to a considerable thickness. They show us that during the great Glacial period there intervened a very decided recurrence of a warmer temperature, during which the great ice masses melted away; and from all the indications, this interval, according to human reckoning, must have lasted thousands of years. This page of the earth's history has for us this especial interest, that the oldest certain indications of man's existence in Europe are found in these inter-glacial deposits.

Similar evidence of an interruption of the Glacial period by one of greater warmth is met with in many other parts of the Alpine region, and also on the plains of Northern Germany, in Scandinavia, England, and in different parts of North America, and we must therefore conclude that it was of general occurrence, and that the changes of temperature which brought about the glaciation of an enormous extent of land, and subsequently set it free from its icy covering, were not regularly progressive, but consisted of many changes and oscillations. . . .

Thus we have sketched in a few hasty outlines what we know of the climatic conditions of three periods of the earth's history which are of especial importance for judging such questions. The first of these, of hoar antiquity, was that of the Coal-measures. We have ascertained the existence of distinct floral regions, which in all probability were determined by differences in the distribution of heat; moreover, we have found in deposits far distant from each other evidence of ice action. But in all other points the conditions are so far removed from any of which we have experience, that any further inference is hardly possible. At the utmost we may conclude from the limitation of the greater coal-beds to the temperate zone that the position of the earth's axis and of the pole did not differ very greatly from those of the present day.

When we turn to the much younger formations of the Tertiary age, the conditions are somewhat clearer. In them we recognize, in the first place, the operation of purely local agencies, the distribution of land and water, of ocean currents, &c., but we must also confess that these play but a subordinate part. We have also seen that in certain regions, viz., in Europe, Greenland, Grinnell Land, &c., there prevailed a much warmer climate, which, however, we do not recognize in America; while in Japan, as inferred from the vegetation, the temperature of Tertiary times seems to have been lower than it now is; and we have found in a displacement of the pole and the earth's axis the only probable explanation of these phenomena.

This cause does not, however, suffice to explain all anomalies, and we must assume for all parts of the earth the prevalence of a somewhat warmer climate, an increase perhaps of a few degrees only, which manifests itself particularly in the vegetation of the polar regions.

In the Pleistocene epoch, which is, comparatively speaking, so near to our own, the problem is so far simplified, that one of the two principal factors which determined the deviation from our present climatic conditions—the displacement of the earth's axis—was no longer present; or rather, having regard to the shortness of the time that has since elapsed, was so unimportant that its influence is not traceable. Apart from purely local circumstances, we have, as far as we can judge, only to deal with uniform oscillations of temperature over the whole earth anomalies of the same general character as brought about the general elevation of climatic temperature in the Tertiary age.

If we follow the march of these vicissitudes of temperature, evidently determined by some cosmical agency, we find at the beginning of Tertiary times a moderately warm climate; then a rise during the Eocene, and then a gradual cooling, interrupted possibly by some oscillations, down to a degree nearly corresponding to that now prevailing, at the beginning of the Pleistocene epoch. Then the cooling continued below the present temperature, to a minimum at the time of the greatest glaciation of the land; then a re-warming in the inter-glacial period nearly up to the present temperature; after which cold and glaciation regained the upper hand, finally to give way to the present conditions, which are about midway between the greatest warmth of the Tertiary age and the greatest cold of the Pleistocene.

One fact stands out conspicuously, viz. that these changes progressed very irregularly, and were subject to much oscillation, and the period during which we can approximately follow the course of the change is much too short to enable us to learn the law that regulated it. We cannot decide whether oscillations like those of the Pleistocene will be repeated, and we are now progressing towards another temporary Glacial period, or whether we have to expect the return of a warmer temperature such as prevailed in Tertiary times, or, finally, whether the outcome of all the deviations will be a lasting refrigeration of our climate.

Just as little can we determine at present by what agency all these vicissitudes are brought about; most

plausible and simple would it certainly be were the sun a variable star that at different periods emits different quantities of heat; but for this or any other assumption there is no proof forthcoming. This enigma, like so many others, will some day be solved by man's searching intelligence, but, like all other acquisitions of science, this goal can be won only by assiduous and patient labour. Haply the triumph may not be for our generation; but what we may certainly accomplish is to prepare the way to it, by an accurate and critical collection of the facts.

H. F. B.

NOTES.

It is expected that about fifty foreign men of science will be present at the Leeds meeting of the British Association. A good many manufacturing firms have promised to open their works during the time at which the meeting is being held; and a Guide to Leeds and the surrounding district, with accounts of the various industries, is being prepared. There will, of course, be excursions to the more interesting places within easy reach of Leeds. The first *soirée* will be given by the Mayor, the second by the Executive Committee. The Yorkshire College will give an afternoon reception.

The London Mathematical Society has awarded the De Morgan Memorial Medal (given triennially) to Lord Rayleigh, Sec.R.S., for his researches in mathematical physics. The previous awards have been to Profs. Cayley and Sylvester. The medal will be presented at the annual meeting in November next.

The *conversazione* of the Society of Arts, as we have already announced, will take place at the Natural History Museum, Cromwell Road, on Friday, June 27. The galleries will be lighted with electricity, so that the authorities of the Museum will have a good opportunity of judging how far the electric light is suitable for the building. If the experiment is successful, the system will no doubt soon be permanently established. It may be hoped that in that case the public will not be excluded during an interval between twilight and the lighting of the electric lamps. That plan has been tried at the British Museum, and the results are not encouraging. If the national collections are to have a fair chance of attracting visitors, they must be open continuously from morning until the hour when they are closed for the night.

The anniversary meeting of the Royal Geographical Society was held on Monday, Sir E. M. Grant Duff, the President, occupying the chair. Mr. Douglas W. Freshfield announced that the Patron's Medal had been awarded to Emin Pasha, and the Founders' Medal to Lieutenant F. E. Younghusband. The Murchison Grant was awarded to Signor Vittorio Sella, for his journey in the Caucasus; the Cuthbert Peek Grant to Mr. E. C. Hore, for observations on the physical geography of Tanganyika; and the Gill Memorial to Mr. C. M. Woodford, for three expeditions to the Solomon Islands. Scholarships and prizes were awarded to students in training colleges. Dr. R. W. Felkin attended, upon instructions by telegram from Zanzibar, to receive the medal for Emin Pasha. The President, in handing the medal to Dr. Felkin, congratulated him upon having done much to make the work of Emin known in England. The Society was not based upon politics, and they simply saw in Emin Pasha one who had from early life given a great deal of attention to botany, natural history, and other subjects. Dr. Felkin, in acknowledgment of the medal, referred to the great services rendered by Emin Pasha to science. Afterwards the Report of the Council was read, and Sir E. M. Grant Duff delivered his presidential address.

At the meeting of the Scientific Committee of the Royal Horticultural Society on June 10, Mr. Morris called attention to the fact that the Royal Society had assigned £100 "on the recommendation of the Government Grant Committee, for an inquiry into the composition of London fog, with special regard to the constituents of fog injurious to plant life." An informal conversation followed with reference to chemical investigations to be undertaken at the laboratory of University College, under the superintendence of Dr. Oliver.

A deputation from the Sanitary Institute lately visited Brighton, and met the Mayor and other members of the Committee for the purpose of further considering the Congress and Exhibition to be held in the Pavilion buildings at the end of August. The large dome of the Pavilion, the Corn Exchange, and the Picture Gallery, are all devoted to the Exhibition, but the applications for space are considerably in excess of previous years, and probably some difficulty will be found in accommodating exhibitors. Sir Thomas Crawford is the President. At one of the meetings of the Congress a lecture will be delivered by Mr. W. H. Preece, F.R.S. Dr. B. Ward Richardson, F.R.S., will address a meeting of the working classes.

The thirty-seventh Report of the Department of Science and Art has been issued.

A lecture on the use of alloys in art metal-work, delivered by Prof. Roberts-Austen at the Society of Arts on May 13, is printed in this week's number of the Society's Journal. It is a lecture of great value and interest, and all who read it will cordially agree with the author that "an effort should be made to induce British artificers to employ the materials and methods which their Japanese brethren have used for centuries with such remarkable effect."

In France much interest is being taken in the question whether a University shall be established in Paris. At a meeting of the General Council of the Paris Faculties, held last Saturday at the Sorbonne, it was agreed that a University with five faculties (Protestant theology, law, medicine, science, and literature), and an upper school of pharmacy, should be formed. "The principal effects of the constitution of the University," says the Paris correspondent of the *Times*, "will be to permit the faculties to make arrangements for the organization of instruction (under the form of schools or institutes) of which the elements are at present scattered in several faculties, and to facilitate a sort of general instruction of a philosophical character, to which the professors of all the faculties will contribute, and which will be addressed to the students. The University will grant, besides professional degrees, diplomas of purely scientific studies to native and foreign students."

M. Deflers has just returned to France from his extremely arduous exploration of Southern Arabia at the instance of the Minister of Public Instruction in France. He has brought back large collections of both living and dried plants for the Museum of Natural History.

The Museum of Natural History in Paris has also received a considerable collection of dried plants gathered in Madagascar by M. Catat.

M. Balansa is about to return to Tonkin for the purpose of continuing his botanical explorations there; and M. Thollon to the Congo, from which he has already sent interesting collections.

A Laboratory of Vegetable Biology was opened at Fontainebleau on May 15. It is under the control of M. G. Bonnier, Professor of Botany at the Sorbonne, Paris, to whom applications for leave to pursue researches in the Laboratory should be addressed.

THE Königsberg Physikalisch-Oekonomische Gesellschaft has recently invited a comprehensive discussion of the observations of ground-temperature at Königsberg, published in the Society's *Schriften*, as bearing on our knowledge of heat-movements in the earth and their causes. Attention is called to a previous work by O. Frölich, published at Königsberg in 1868. For the best treatment of the subject a prize of £15 is offered. Papers (in any language), with motto, to be sent in before February 1, 1891.

Science announces that Lieutenant J. P. Finley, of the U.S. Signal Corps, has gone to San Francisco to take charge of the Pacific Coast weather service.

WE learn from *Science* that a work of great importance to navigators is to be undertaken in connection with the report of the U.S. Eclipse Expedition to West Africa, under the direction of Prof. D. P. Todd. This is the preparation of a set of daily weather-maps of both oceans from October to May inclusive, the entire period of the cruise of the U.S. steamship *Pensacola*. The U.S. Hydrographic Office calls attention to the importance of the subject, and the exceptional opportunity presented for utilizing the data already at hand, together with such additional data as may be contributed for this purpose by various Government offices and individual navigators. The scheme determined upon consists in the preparation of a weather map for each day at noon, Greenwich mean time, from October 1, 1889, to May 31, 1890, inclusive, for the entire area between latitude 70° north and 60° south, longitude 20° east and 100° west. In addition to the Greenwich noon observations that are kept regularly for the Hydrographic Office by nearly two thousand voluntary observers, it is earnestly desired that other navigators of these waters, within the limits of time and place mentioned above, may forward to that office such data from their log-books as may be useful in this connection, selecting those observations that come nearest to noon, Greenwich mean time, and stating as many details as possible regarding wind, weather, state of the sea, and velocity and set of currents. Data from land stations are also very important, especially such as are not accessible in any published records. To make this great undertaking a success, however, there must be further and cordial co-operation among the nations interested in the meteorology of this vast area, and among navigators of every nationality. It has long been the desire of the U.S. Hydrographic Office to begin the publication of a pilot chart of the South Atlantic and west coast of South America, and the present undertaking will furnish an admirable basis for this work.

A PAPER on the Mannesmann weldless tubes was lately read before the Society of Arts by Mr. J. G. Gordon, the chair being occupied by Sir Frederick Bramwell, who referred to the importance and interest of the subject, and to the extraordinary means by which the desired result was attained. The process consists in the solution of a purely kinematical problem, viz. the arranging of the velocity ratio of a pair of aconoidal rolls so as to change a solid piece delivered to them at one end into a hollow tube passed out at the other. These rolls revolve at about 200 to 300 revolutions per minute, and by their action on the hot and therefore plastic steel stretch it and make a hollow in the centre. The substance of the metal must be sufficiently homogeneous and plastic, and, in passing through the rolls, it undergoes a violent twisting and stretching action. The bar, in fact, in its passage through the rolls, is twisted as a thread is twisted in a spinning-machine, the material being drawn from the interior. This action was illustrated by one of the exhibits, which consisted of a bar, the ends of which were slightly drawn down under the hammer, so that the rolls could not act on them. A hollow was thus produced in the solid bar of metal, the contents of which were tested by Prof. Finke, of Berlin, and found to contain 99 per cent. of hydrogen of its total volume; the remaining 1 per cent. he considered to be probably nitrogen. In the carrying out of the process, 2000 to 10,000 horse-power is required for from 30 to 45 seconds, according to the dimensions of the tube. Although this is all the time actually required to convert a bar 10 to 12 feet long and 4 inches in diameter into a tube, a certain amount of time is required to adjust the guides, to deliver the bar to the rolls, and to remove the finished tube. The time so spent is employed to accumulate energy in a fly-wheel 20 feet in diameter, weighing 70 tons, and revolving 240 times in a minute, the periphery of which therefore revolves at 2·85 miles per minute; by this means, a steam-engine of 1200 horse-power is quite sufficient to do the work. A peculiar feature of these rolls is that the resulting tube is a test of the material and process. If the metal is homogeneous throughout, and well melted, well rolled, and carefully heated, it makes a perfect tube; but if there is a flaw in the metal, or if it has not been properly heated, the rolls cannot make a tube out of it. The paper, which was illustrated by photographs of the mills and engines, led to a very interesting discussion, in which Sir Frederick Bramwell, Prof. A. B. W. Kennedy, Mr. Alexander Siemens, and others took part.

MESSRS. J. AND A. CHURCHILL have issued the fifth edition of Prof. F. Clowes's "Treatise on Practical Chemistry." The present edition contains several emendations and additions. The author explains that the work is intended to furnish a course of laboratory instruction in practical chemistry, which may precede the higher training of the professional and pharmaceutical chemist and the medical man, and the more special training of the technical chemist and the chemical engineer.

THE sixth volume of "Blackie's Modern Cyclopædia," edited by Dr. Charles Annandale, has been published. The volume begins with "Mona," and ends with "Postulate." The articles, like those of the previous volumes, are remarkably clear, concise, and accurate.

THE third number of the "Indian Museum Notes" consist-of a careful paper on silkworms in India, by Mr. E. C. Cotes. The author confines attention to those species which are actually utilized in India for the production of silk.

MR. W. F. KIRBY, author of "A Synonymic Catalogue of Diurnal Lepidoptera," will publish shortly with Messrs. Gurney and Jackson, Mr. Van Voorst's successors, "A Synonymic Catalogue of Neuroptera Odonata or Dragon-flies." He hopes to bring out afterwards the first volume of his "Catalogue of Lepidoptera Heterocera," a work which has engaged his attention for nearly twenty years.

THE editor of the *Naturalists' Gazette* has in the press an "Illustrated Hand-book of British Dragon-flies," which will contain a full description of all the species indigenous to the British Isles, in addition to a quantity of other information.

A NEW gas, methylene fluoride, CH_2F_2, has been obtained by M. Chabrié, and a preliminary account of it will be found in the current number of the *Comptes rendus*. Its chlorine, bromine, and iodine analogues, CH_2Cl_2, CH_2Br_2, and CH_2I_2, have long been known. Methylene iodide is a liquid at ordinary temperatures which solidifies about 4° C. to brilliant leafy crystals. The liquid boils at 182°. Methylene bromide is a liquid boiling at 81°, and the chloride is also a liquid boiling at 41°. The fluoride, completing the graduation of the series, is now shown to be a gas. It is obtained by heating the chloride with silver fluoride. Methylene chloride is generally prepared from methyl iodide, which is placed in a retort, covered with water, and a

stream of chlorine slowly allowed to pass through it. The very volatile methylene chloride distils over, as it is formed, into a condenser which is strongly cooled by a freezing mixture. In order to prepare the new gas, the methylene chloride is placed in a tube of Bohemian glass along with the proper quantity of pure silver fluoride, and the tube sealed before the blow-pipe. It is then heated for half an hour to 180°. In an actual experiment 1·7 gram of methylene chloride and 5·08 grams of anhydrous silver fluoride, specially purified by the method described by Gore, were employed. Upon opening the tube, a great rush of gas occurs, which on collection and analysis is found to consist of methylene fluoride. The density of the gas compared with air was found to be 1·82, which agrees very closely with the theoretical density, 1·81, required for the formula CH_2F_2. Alcoholic potash is found to absorb it completely. Hence, in order to obtain a measure of the amount of carbon contained in the gas, a measured volume was absorbed in alcoholic potash and then treated with acetic acid and potassium permanganate. The alcoholic potash appears to convert it into formaldehyde, $\begin{matrix} H \\ H \end{matrix}\!\!>C = O$; this is oxidized by the potassium permanganate to carbonic acid, $\begin{matrix} HO \\ HO \end{matrix}\!\!>C = O$, and the acetic acid consequently liberates a volume of carbon dioxide equal to the volume of methylene fluoride experimented upon. This affords a ready mode of demonstrating at the same time the principal properties of the gas and its composition as regards the amount of carbon contained in it. Experiments are now in progress from which it is hoped some knowledge will be gained concerning its physiological action, which it will be interesting to compare with that described by MM. Regnault and Villejean in case of methylene chloride. In a recent communication by M. Moissan upon carbon tetrafluoride, CF_4, an account of which was given in NATURE (May 15, p. 67), it was recommended that metallic tubes should always be employed in these reactions with silver fluoride, inasmuch as fluorides of carbon attack glass with production of carbon dioxide and silicon tetrafluoride; for instance, $CF_4 + SiO_2 = CO_2 + SiF_4$. But M. Chabrié finds that if hard Bohemian glass is used, the product contains only mere traces of the two gaseous impurities mentioned, and, as glass is so much more convenient to manipulate, considers it advisable to use it. The methylene fluoride prepared in the above manner was quite sufficiently pure for all practical purposes.

THE additions to the Zoological Society's Gardens during the past week include a Common Marmoset (*Hapale jacchus*) from South-east Brazil, presented by Mr. Percy Standish; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from the Upper Shire, two Grand Galagos (*Galago crassicaudata*) from Mandala, Shire Highland, East Africa, presented by Mr. John W. Moir; two Common Marmosets (*Hapale jacchus*) from South-east Brazil, presented by Mr. W. Norbury; a Common Fox (*Canis vulpes* ♂), British, presented by Mr. Atkins; a Great Crested Grebe (*Podiceps cristatus*), British, presented by Mr. T. E. Gunn; two Green Lizards (*Lacerta viridis*), three Wall Lizards (*Lacerta muralis*), a Dark-Green Snake (*Zamenis atrovirens*), four Common Snakes (*Tropidonotus natrix*), four Marbled Newts (*Molge marmorata*), an Edible Frog (*Rana esculenta*) from the South of France, presented by the Rev. F. W. Haines; eighteen Young Green Turtles (*Chelone viridis*) from Ascension Island, presented by Captain Robinson; a Silvery Gibbon (*Hylobates leuciscus*) from Java, deposited; a Philippine Paradoxure (*Paradoxurus philippensis*) from Zebu Island, Philippines, three Japanese Teal (*Querquedula formosa* ♂ ♀ ♀) from North-east Asia, purchased; an Angora Goat (*Capra hircus*, var., ♂), two Yellow-legged Herring Gulls (*Larus cachinnans*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on June 19 = 15h. 52m. 18s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4244	—	Bluish.	16 43 56	+47 48
(2) 367 Birm.	6	Red.	15 59 21	+47 32
(3) α Serpentis	2	Yellow.	15 38 54	+ 6 46
(4) α Coronæ	2	Bluish-white.	15 30 0	+27 5
(5) U Cassiopeiæ... ...	Var.	Red.	0 40 11	+47 39

Remarks.

(1) The spectrum of this nebula, according to an observation made by Dr. Huggins in 1866, is a continuous one, but this result does not appear to accord with Smyth's description of it as "a fine planetary nebula, . . . large, round, and of a lucid pale blue hue." The G.C. description of it is: "Very bright; large; round; disk with faint, possibly resolvable, border." I know of no later observation of the spectrum than that referred to, but it is important that it should be confirmed, as the colour alone would lead to the supposition that there is something in addition to continuous spectrum. It is indeed possible that we have here a case of a nebula intermediate in condensation between those which give a spectrum of bright lines, and those which give a so-called "continuous" spectrum. In any case the apparent discrepancy between colour and spectrum should be investigated, for it is generally understood that planetary nebulæ with a bluish colour give bright lines.

(2) Dunér describes the spectrum of this star as a magnificent one of Group II. "All the bands 2–10, 6 included, and possibly 1, are visible. They are of extraordinary width and entirely black. The spectrum is totally discontinuous." The usual more detailed observations should be made.

(3) This star has a spectrum generally described as similar to the solar spectrum. The usual more detailed observations, as to whether the star is increasing or decreasing in temperature, are required.

(4) The spectrum of this star is a well-marked one of Group IV., but so far we have no information as to the temperature of the star relatively to others with almost similar spectra.

(5) The spectrum of this variable has not been recorded. The range of variation is from 8·5 to 14 in about 260 days. There will be a maximum about June 20. A. FOWLER.

OBSERVATIONS OF METEORS.—The May number of the *Monthly Notices* of the Royal Astronomical Society contains a catalogue of 918 radiant-points of meteors observed by Mr. Denning at Bristol since 1873, together with a mass of information pertaining to their determination. The total number of meteors seen from 1873 to 1889 was 12,083, and the paths of 9177 of these were registered. The following table shows the horary rate of apparition of the meteors during the various months of the year:—

January ...	6·5	July	11·3
February ...	4·9	August	11·3
March	6 6	September ...	10·3
April	6·6	October... ...	11·8
May	5·2	November ...	11·3
June	4·9	December ...	8·9

The mean horary rate of apparition is therefore 8·3. This is less than would be observed from a place where there is no interference with the light and smoke of a large town, some observations made by Mr. Denning in a different locality increasing the mean horary number to 11·4.

The observations were almost equally distributed between the morning hours, and were usually made between the third and first quarter of the moon, because a bright sky is very effective in obliterating meteor-showers, and therefore moonlight meteors are commonly rare.

As to the relative numbers which appear during the night, the maximum appears to be attained between 2 and 3 a.m., when the rate is nearly double that observed in the early hours of the evening. Two or three meteors have frequently been noticed to appear at nearly the same time and from the same radiant, the probable explanation in such cases being that the

two objects originally formed one mass, which suffered disruption owing to the vicissitudes encountered in planetary space.

The average length of path of all the meteors registered is $10^\circ \cdot 9$. The average height of either fireballs or shooting-stars has been computed, from thirty-eight instances, to be—

Beginning height ... 71·1 miles.
End height ... ... 48·2 ,,

From a comparison of a large number of other similar results, the following general average has been deduced :—

Beginning height ... 76·4 miles (683 meteors).
End height ... ... 50·8 ,, (736 ,,).

If fireballs and shooting-stars are separated, the usual heights of disappearance are: fireballs, 30 miles; shooting-stars, 54 miles. A considerable amount of information as to the radiant-points, stationary and otherwise, has been brought together; and, with the catalogue, they render Mr. Denning's paper one of a very important character.

BROOKS'S COMET (a 1890).—The following ephemeris has been computed by Dr. Bidschof (*Astr. Nach.*, 2970), and is in continuation of that previously given (vol. xlii. p. 138). The elements have been found from observations at Cambridge, March 21, and Vienna, April 18 and May 24:—

$$T = 1890 \text{ June } 1 \cdot 5360 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned} \omega &= 68^\circ \ 54' \ 39''\cdot 9 \\ \Omega &= 320 \ \ 20 \ \ 32\cdot 2 \\ \iota &= 120 \ \ 33 \ \ \ 5\cdot 4 \end{aligned}\right\} \text{Mean Eq. } 1890\cdot 0.$$
$$\log q = 0\cdot 280524$$

Ephemeris for Berlin Midnight.

1890.	R.A.	Decl.	Brightness.	1890.	R.A.	Decl.	Brightness.
	h. m. s.	° ′			h. m. s.	° ′	
June 21	15 59 27	+65 19·5		July 12	13 48 18	+55 30·9	2·10
,, 22	49 35	65 5·3	3·08	,, 13	45 11	54 58·1	
,, 23	40 4	64 48·6		,, 14	42 15	54 25·6	
,, 24	30 56	64 29·4		,, 15	39 29	53 53·3	
,, 25	22 13	64 8·1		,, 16	36 53	53 21·2	1·93
,, 26	13 55	63 44·9	2·88	,, 17	34 27	52 49·5	
,, 27	6 0	63 20·0		,, 18	32 9	52 18·1	
,, 28	14 58 29	62 53·6		,, 19	29 59	51 47·0	
,, 29	51 22	62 25·9		,, 20	27 56	51 16·3	1·77
,, 30	44 38	61 57·1	2·68	,, 21	26 0	50 45·9	
July 1	38 16	61 27·3		,, 22	24 11	50 15·9	
,, 2	32 15	60 56·7		,, 23	22 29	49 46·3	
,, 3	26 35	60 25·4		,, 24	20 52	49 17·0	1·63
,, 4	21 14	59 53·6	2·48	,, 25	19 21	48 48·1	
,, 5	16 12	59 21·4		,, 26	17 56	48 19·7	
,, 6	11 27	58 48·8		,, 27	16 36	47 51·7	
,, 7	6 59	58 15·9		,, 28	15 21	47 24·1	1·50
,, 8	2 46	57 42·9	2·29	,, 29	14 10	46 56·9	
,, 9	13 58 48	57 9·9		,, 30	13 4	46 30·1	
,, 10	55 6	56 36·8		,, 31	12 1	46 3·8	
,, 11	51 36	56 3·8		Aug. 1	11 1	45 37·9	1·38

PHOTOGRAPH OF BROOKS'S COMET (a 1890).—A photograph of this comet was obtained at Algiers on May 22 by M. Ch. Trépied (*Comptes rendus*, June 9, No. 23). Two hours' exposure was found necessary.

ASTRONOMICAL TELESCOPES.[1]

BEFORE speaking of the enormous instruments of the present day, with their various forms and complicated machinery, it will be well to give some little time to a consideration of the principles involved in the construction of the telescope, the manner in which it assists the eye to perceive distant objects, and in a brief and general way to the construction and action of the eye as far it affects the use of the telescope, all as a help to consider in which way we may hope to still further increase our sense of vision.

[1] Discourse delivered at the Royal Institution on Friday, May 30, 1890, by Mr. A. A. Common.

I will ask you to bear with me when I mention some things that are very well known, but which if brought to mind may render the subject much more easy. Within pretty narrow limits the principles involved in the construction of the telescope are the same whatever form it ultimately assumes. I will take as an illustration the telescope before me, which has served for the finder to a large astronomical telescope, and of which it is really a model. On examination we find that it has, in common with all refracting telescopes, a large lens at one end and several smaller ones at the other; the number of these small lenses varies according to the purpose for which we use the telescope. Taking out this large lens we find that it is made of two pieces of glass; but as this has been done for a purpose to be presently explained which does not affect the principle, we will disregard this, and consider it only as a simple convex lens, to the more important properties of which I wish first of all particularly to draw your attention, leaving the construction of telescopes to be dealt with later on.

Stated shortly, such a lens has the power of refracting or bending the rays of light that fall upon it: after they have passed through the lens the course they take is altered; if we allow the light from a star to fall upon the lens, the whole of the parallel rays coming from the star on to the front surface are brought by this bending action to a point at some constant distance behind, and can be seen as a point of light by placing there a flat screen of any kind that will intercept the light. For all distant objects the distance at which the crossing of the rays takes place is the same. It entirely depends on the substance of the lens and the curvature we give to the surfaces, and not at all upon the aperture or width of the lens. The brightness only of the picture of the star, depends upon the size of the lens, as that determines the amount of light it gathers together. If, instead of one star we have three or four stars together, we will find that this lens will deal with the light from each star just as it did with the light of the first one, and just in proportion to the distance they are apart in the sky, so will the pictures we see of them be apart on our screen. So if we let the light from the moon fall on our lens, all the light from the various parts of the moon's surface will act like the separate stars, and produce a picture of the whole moon (in the photographic camera the lens produces in this manner a picture of objects in front of it which we see on the ground glass). When we attempt to get pictures of near objects that do not send rays of light that are parallel we find that as the rays of light from them do not fall on the lens at the same angle to the axis the picture is formed further away from the lens. The nearer the object whose picture we wish to throw upon the screen is to the lens, the further the screen must be moved. If we try this experiment we will find, when we have the object at the same distance as the screen, the picture is then of the same size as the object, and the distance of the screen from the lens is twice that which we have found as the focal length; on bringing the object still nearer the lens, we find we must move the screen further and further away, until when the object is at the focus the picture is formed at an infinite distance away, or, what is more to our purpose, the rays of light from an object at the focus of a convex lens go away through the lens parallel, exactly as we have seen such parallel rays falling on the glass come to a focus, so that our diagram answers equally well whatever the direction of the rays; and this holds good in other cases where we take the effect of reflection as well as refraction.

We can also produce pictures by means of bright concave surfaces acting by reflection on the light falling upon them. Such a mirror or concave reflecting surface as I have here will behave exactly as the lens, excepting, of course, that it will form the picture in front instead of behind. The bending of the rays in the case of the convex lens is convergent, or towards the axis, for all parallel rays; if we use the reverse form of lens—that is, one thicker at the edge than in the middle—we find the reverse effect on the parallel rays; they will now be divergent, or bend away from the axis; and so with reflecting surfaces if we make the concavity of our mirror less and less, till it ceases and we have a plane, we will get no effect on the parallel rays of light except a change of direction after reflection. If we go beyond this and make the surface convex we then will have practically the same effect on the reflected rays as that given to the refracted ray by the concave glass lens.

As regards the size of the picture produced by lenses or mirrors of different focal length, the picture is larger just as the focal length is greater, and the angular dimension is converted into a linear one on the screen in due proportion. Now, as we

shall assume that the eye sees all things best at the distance of about nine inches, we may say that the picture taken with a lens of this focal length gives at once the proper and most natural representation we can possibly have of anything at which we can look. Such a picture of a landscape, if placed before the eye at the distance of nine inches, would exactly cover the real landscape point for point all over. A picture taken with a lens of shorter focal length, say four inches, will give a picture as true in all the details as the larger one, but if this picture is looked at, at nine inches distance, it is not a true representation of what we see; in order to make it so, we must look at it with a lens or magnifier. With a larger picture one can look at this at the proper distance, which always is the focal distance of the lens with which it was obtained, when we will see everything in the natural angular position that we have in the first case.

But if, instead of looking at this larger picture, which we may consider taken with a lens of say ninety inches focal length, at a distance of ninety inches, we look at it at a distance of nine inches, we have practically destroyed it as a picture by reducing the distance at which we are viewing it, and we have converted it into what is for that particular landscape a telescopic picture; we see it, not from the point at which it was taken, but just as if we were at one-tenth of the distance from the particular part that we examine. A telescope with a magnifying power of ten, would enable us to see the landscape just as we see it in the photograph, when we examine it in the way I have mentioned.

Having thus seen how a lens or mirror acts, we will turn our attention to the eye. Here we find an optical combination of lenses that act together in the same way as the single convex lens of which we have been speaking. We will call this combination the lens of the eye. It produces a picture of distant objects which in the normal eye falls exactly in focus upon the retina. We are conscious that we do see clearly at all distances beyond about nine inches.

At less than this distance objects become more and more indistinct as they are brought nearer to the eye. From what we have seen of the action of the lens in producing pictures of near and distant objects, we know that some movement of the screen must be made in order to get such pictures sharply focussed, a state of things necessary to perfect vision. We might therefore suppose that the eye did so operate by increasing when necessary the distance between the lens and retina, but we know that the same effect is produced in another way; in fact, the only other way. The eye by a marvellous provision of nature, secures the distinctness of the picture on the retina of all objects beyond a distance of about 9 inches, by slightly but sufficiently varying the curvature of one of the lenses; by an effort of will, we can make the accommodating power of the eye slightly greater, and so see things clearly a little nearer; but at about the distance of 9 inches, the normal eye is unconscious of any effort in thus accommodating itself to different distances. The picture produced by the lens of the eye whose focal length we will assume to be six-tenths of an inch falls on the retina, which we will assume further to be formed of a great number of separate sensible points, which, as it were, pick up the picture where it falls on these points, and through the nervous organization, produce the sense of vision. Possibly when these points are affected by light, there may be some connective action, either produced by some slight spherical aberration of the lens or otherwise; but I do not wish to go any further in this matter than is necessary to elucidate my subject. What I am concerned with now is the extent to which the sensibility of the retina extends. Experiment tells us that it extends to the perception of two separate points of light whose angular distance apart is one minute of arc, or in other words at the distance we can see best, two points whose distance apart is about 1/400 of an inch.

This marvellous power can be better appreciated when we remember that the actual linear distance apart of two such points on the retina is just a little more than 1/6000 of an inch.

In dealing with the shape of small objects the difference between a circle, square, and triangle, can be detected when the linear size of either is about 1/2000 of an inch. It may be therefore fairly taken that these separate sensible points of the retina are somewhere about 1/12,000 part of an inch apart from each other. Wonderfully minute as must this structure be, we must remember, as we have already shown, that the actual size of the image it deals with is also extremely small. This minuteness becomes apparent when we consider what occurs when we look at some well-known object, such as the full moon. Taking the angular diameter of the moon as 30 minutes of arc, and the focal length of the eye at six-tenths of an inch, we find the linear diameter of the picture of the full moon on the retina is about 1/200 of an inch, and assuming that our number of the points in the retina is correct, it follows that the moon is subject to the scrutiny of 2800 of these points, each capable of dealing with the portion of the picture that falls upon it.

That is to say, the picture, as the retina deals with it, is made up of this number of separate parts, and is incapable of further division, just as if it were a mosaic. I think this is really the case, and as such a supposition permits us to explain not only what occurs when we assist the eye by means of the telescope, but also what occurs when we use the telescope for photographing celestial objects, we will follow it up.

In the case of the eye we suppose the image of the moon to be made up through the agency of these 2800 points, each one capable of noting a variation in the light falling upon it. In order to make this rather important point plainer, I have had a diagrammatic drawing made on this plan. Taking a circle to represent the full moon, I have divided it into this number of spaces, and into each space I have put a black dot, large or small, according to the intensity of the light falling on that part of the image as determined by looking at a photograph of the moon. You will see by the picture of this moon the effect produced. It represents to those who are at a sufficient distance the moon much as it is really seen in the sky.

We can now with a lens of the same focal length as the eye obtain a picture of the full moon exactly of the size of the actual picture on the retina, and if we take a proper photographic process we can get particles of silver approximately of the same sizes as the dots we have used in making our diagram of the moon; the grouping is not exactly the same, but we may take it as precisely so for our purpose. I have not any photographs of the full moon of this size, but I have some here of the moon about five, seven, and eight days old, which give a good idea of what I mean by the arrangements of the particles of silver being like our diagram.

It is now quite apparent that if we can by any means increase the size of the picture of the moon on the retina or make it larger on the photographic plate, we would be able to employ more of our points or particles of silver, and so be able to see more clearly just in proportion as we increase the size of the picture in relation to the size of the separate parts that make it.

Now the telescope enables us to do this for the eye, and a longer focussed lens will give us a larger photographic picture.

Let us assume that by means of the telescope we have increased the power of the eye one hundred times. The picture of the moon on the retina would now be one-half inch diameter, and instead of employing 2800 points to determine its shape, and the various markings upon it, we should be employing 28,000,000 of these points; and similarly with the photograph, by increasing the size of our lens we will obtain a picture made up of this enormous number of particles of silver. But we can go further in the magnification of the picture on the retina—we can also use a still longer focus photographic lens.

A power of magnification of one thousand is quite possible under favourable circumstances; this means that the picture of one two-hundredth of an inch would be now of five inches in diameter, so we must deal with only a portion of it. Let us take a circle of one-tenth of this, equalling one-hundredth of our original picture, which in the eye, unaided by the telescope, would have a diameter of one two-thousandth of an inch, or an area of less than one five-millionth of a square inch. This means that with this magnification, we have increased the power so enormously that we are now employing for the photographic picture two thousand eight hundred million particles of silver, and in the eye the same degree of increase in the number of points of the retina employed in scrutinizing the picture piece by piece as successive portions are brought into the central part.

Photography enables me to show that the result I have given of the wonderful effect of increasing the optical power is perfectly correct as far as it is concerned. We will deal with a part only of the moon, representing, as I have just said, about one-tenth of its diameter, or one-hundredth of its visible surface. Two such portions of the moon are marked, as you see, on the diagram. I have selected these portions as I am able to show you them just as taken on a large scale by photography so that you can make the comparison in the most certain manner;

but let us first analyze our diagrammatic moon—let us magnify it about ten times, and see what it looks like.

I now show you a picture of this part of the diagram, inclosing the portions I wish to speak about, magnified ten times, so that you can see that about twenty-eight of our points, and by supposition twenty-eight of our particles of silver on the photographic plate, make up the picture. You will see that these dots vary in size; the difference is due to the amount of light falling within what we may call the sphere of action of each point, and should represent it exactly. The result can hardly be called a picture, as it conveys no impression of continuity of form to the mind. We have got down to the structure or separate parts, and to the limit of the powers of the eye and the photographic plate, of course on the assumption we have made as to the size of the points in the one case and the particles of silver in the other. I will now show you the same parts of the moon as represented by the circles on our diagram exactly as delineated by photography. You now see a beautiful picture giving mountains, valleys, craters, peaks, and plains, and all that makes up a picture of lunar scenery. We have thus seen how the power of the eye is increased by the enlargement of the picture on the retina by the telescope, and also how, by increasing the size of the photograph, we also get more and more detail in the picture.

We know we cannot alter the number of those separate points on the retina which determine the limit of our powers of vision in one direction, but we may be able to increase enormously the number of particles of silver in our photographic picture by processes that will give finer deposits, and so, in conjunction with more perfect and larger photographic lenses, we may reasonably look for a great improvement in our sense of vision—it may be even beyond that given by the telescope alone; although it always will be something in favour of the telescope that the magnification obtained in the eye is about fifteen times greater than that obtained by photography when the image on the retina is pitted against the photograph of the same size, unless we use a lens to magnify the photograph of the same focal length as the eye, in which case it is equal. But we *may* go much further in our magnification of the photographic image. In other ways there is great promise when we consider the difference in the action of the eye and the chemical action in the sensitive film under the action of light. As I pointed out in the discourse I gave about four years ago in this theatre, the eye cannot perceive objects that are not sufficiently illuminated, though this same amount of illumination will, by its cumulative effect, make a photographic picture, so that there are ways in which the photographic method of seeing celestial bodies can be possibly made superior to the direct method of looking with a telescope.

With some celestial objects this has been already done: stars too faint to be seen have been photographed, and nebulæ that cannot be seen have also been photographed; but much more than this is possible: we may be able to obtain photographs of the surface of the moon similar to those I have shown, but on a very much larger scale, and we may obtain pictures of the planets that will far surpass the pictures we would see by the telescope alone.

I have mentioned that the distance at which the normal eye can best see things is about nine inches, as that gives the greatest angular size to the object while retaining a sharp picture on the retina; but, as many of us know, eyes differ in this power: two of the common infirmities of the eyes are long- or short-sightedness, due to the pictures being formed behind the retina, in the first case, and in front of it in the other. Towards the end of the thirteenth century it was found that convex lenses would cure the first infirmity, and, soon afterwards, that concave lenses would cure the second, as can be easily seen from what I have said about the action of these lenses; so that during the fifteenth and sixteenth centuries the materials for the making of a telescope existed; in fact, in the sixteenth century, Porta invented the camera obscura, which is in one sense a telescope. It seems very strange that the properties of a convex and concave lens when properly arranged were not known much earlier than 1608. Most probably, if we may judge from the references made by some earlier writers, this knowledge existed, but was not properly appreciated by them. Undoubtedly, after the first telescopes were made in Holland in 1608, the value of this unique instrument was fully appreciated, and the news spread rapidly, for we find that in the next year "Galileo had been appointed lecturer at Padua for life, on account of a perspective like the one which was sent from Flanders to Cardinal Borghese." As far as can be ascertained, Galileo heard of the telescope as an instrument by which distant objects appeared nearer and larger, and that he, with this knowledge only, reinvented it. The Galilean telescope is practically, though not theoretically, the simplest form. It is made of a convex lens in combination with a concave lens to intercept the cone of rays before they come to a focus, and render them parallel so that they can be utilized by the eye. It presents objects as they appear, and the picture is freer from colour in this form than in the other, where a convex eye-glass is used. It is used as one form of opera-glass at the present time. Made of one piece of glass in the shape of a cone, the base of which is ground convex, and the apex slightly truncated and ground concave, it becomes a single-lens telescope that can be looked upon just as an enlargement of the outer lens of the eye.

Galileo was undoubtedly the first to make an astronomical discovery with the telescope: his name is, and always will be, associated with the telescope on this account alone.

Very soon after the introduction of the Galilean telescope, the difficulties that arise from the coloured image produced by a single lens turned attention to the possibility of making a telescope by using the reflecting surface of a concave mirror instead of a lens. Newton, who had imperfectly investigated the decomposition of light produced by its refraction through a prism, was of opinion that the reflecting principle gave the greatest possibilities of increase of power. He invented, and was the first to make, a reflecting telescope on the system that is in use to the present day; thus the two forms of telescope—the refracting and reflecting—came into use within about 60 years of each other. It will be perhaps most convenient in briefly running through the history of the telescope, that I should give what was done in each century.

Commencing, then, with the first application of the telescope to the investigation of the heavenly bodies by Galileo in 1609, we find that the largest telescope he could make gave only a magnifying power of about 30.

The first improvement made in the telescope, as left by Galileo, was due to a suggestion—by some attributed to Kepler, but certainly used by Gascoigne—to replace the concave eye-lens that Galileo used by a convex one. Simple as this change looks, it makes an important, indeed vital improvement. The telescope could now be used, by placing a system of lines or a scale in the common focus of the two lenses, to measure the size of the image produced by the large lens; the axis or line of collimation could be found, and so the telescope could be used on graduated instruments to measure the angular distance of various objects; in fact, we have now in every essential principle the true astronomical telescope. It is useless as an ordinary telescope, as it inverts the objects looked at, while the Galilean retains them in their natural position. The addition, however, of another lens or pair of lenses reinverts the image, and we then have the ordinary telescope. It was soon found that the single lens surrounds all bright objects with a fringe of colour, always of a width of about one-fiftieth of the diameter of the object-glass, as we must now call the large lens; and as this width of fringe was the same whatever the focal length of the object-glass, the advantage of increasing this focal length and so getting a larger image without increasing the size of the coloured fringe became apparent, and the telescope therefore was made longer and longer, till a length of over one hundred feet was reached; in fact, they were made so long that they could not be used. A picture of one of these is shown, from which it can be easily imagined the difficulties of using it must have been very great, yet some most important measurements have been made with these long telescopes. Beyond the suggestions of Gregory and Cassegrain for improvements in the reflecting telescope, little was done with this instrument.

During the eighteenth century immense advances were made in both kinds of telescopes. With the invention of the achromatic telescope by Hall and Dollond, the long-focussed telescopes disappeared.

Newton had turned to the reflecting telescopes believing from his investigations that the dispersion and refraction were constant for all substances; this was found not to be so, and hence a means was possible to render the coloured fringe that surrounds bright objects when a single lens is used less prominent, by using two kinds of glass for the lens, one giving more refraction with somewhat similar dispersion, so that while the dispersion of one lens is almost corrected or neutralized by the other, there is still a refraction that enables the combination to be used as a lens giving an image almost free from colour.

In 1733, Hall had made telescopes having double object-glasses on this plan, but never published the fact. Dollond, who had worked independently at the subject, came to the conclusion that the thing could be done, and succeeded in doing it; the invention of the achromatic telescope is with justice, therefore, connected with his name.

Although this invention was a most important one, full advantage could not be taken of it owing to the difficulty of getting disks of glass large enough to make into the compound object-glass, disks of about four inches being the largest diameter it was possible to obtain. With the reflecting telescope, unhampered as it always has been by all except mechanical difficulties, advance was possible, and astronomers turned to it as the only means of getting larger instruments. Many most excellent instruments were made on the Newtonian plan. The plan proposed by Gregory was largely used, as in this instrument objects are seen in their natural position, so that the telescope could be employed for ordinary purposes.

Many were also made on the plan proposed by Cassegrain. The diagrams on the wall enable you to at once see the essential points of these different forms of reflectors.

About 1776, Herschel commenced his astronomical work; beginning with reflecting telescopes of six or seven inches, he ultimately succeeded in making one of four feet aperture with these instruments. As everyone knows, most brilliant discoveries were effected, and the first real survey of the heavens made.

Herschel's larger telescopes were mounted by swinging them in a surrounding framed scaffolding that could itself be rotated. The smaller ones were mostly mounted on the plan of the one now before us, which the Council of the Royal Astronomical Society have kindly allowed me to bring here. The plan nearly always used by Sir William Herschel was the Newtonian, though for the larger instruments he used the plan proposed years before by Le Maire, but better known as the Herschelian, when the observer looks directly at the large mirror, which is slightly tilted, so that his body does not hinder the light reaching the telescope. In all cases the substance used for the mirrors was what is called speculum metal.

During the present century the aperture of the refracting telescope has increased enormously; the manufacture of the glass disks has been brought to a high state of perfection, particularly in France, where more attention is given to this manufacture than in any other country. Early in the century the great difficulty was in making the disks of flint glass. M. Guinand, a Swiss, beginning in 1784, succeeded in 1805 in getting disks of glass larger and finer than had been made before, and refractors grew larger and larger as the glass was made. In 1823 we have the Dorpat glass of 9·6 inches, the first large equatorial mounted with clock-work; in 1837 the 12-inch Munich glass; in 1839 the 15-inch at Harvard, and in 1847 another at Pulkowa; in 1863 Cooke finished the 25-inch refractor which Mr. Newall gave, shortly before his death last year, to the Cambridge University.

This telescope the University has accepted, and it is about to be removed to the Observatory at Cambridge, where it will be in charge of the Director, Dr. Adams. In accordance with the expressed wish of the late Mr. Newall, it will be devoted to a study of stellar and astronomical physics. There is every prospect that this will be properly done, as Mr. Frank Newall, one of the sons of the late Mr. Newall, has offered his personal services for five years in carrying on this work. Succeeding this we have the 26-inch telescope at Washington, the 26-inch at the University of Virginia, the 30-inch at Pulkowa, and the 36-inch lately erected at Mount Hamilton, California—all these latter by Alvan Clark and his sons. By Sir Howard Grubb we have many telescopes, including the 28-inch at Vienna. Most of these telescopes have been produced during the last twenty years, as well as quite a host of others of smaller sizes, including nearly a score of telescopes of about 13 inches diameter by various makers, to be employed in the construction of the photographic chart of the heavens, which it has been decided to do by international co-operation.

The first of these photographic instruments was made by the Brothers Henry, of the Paris Observatory, who have also made many very fine object-glasses and specula, and more important than all, have shown that plane mirrors of perfect flatness can be made of almost any size; the success of M. Lœwy's new telescope, the equatorial *coudé*, being entirely due to the marvellous perfection of the plane mirrors made by them.

The reflecting telescope has quite kept pace with its elder brother.

Lassell in 1820 began the grinding of mirrors, he like Sir William Herschel working through various sizes, finally completing one of 4 feet aperture, which was mounted equatorially. Lord Rosse also took up this work in 1840; he made two 3-foot specula, and in 1845 finished what yet remains the largest telescope, one of 6 feet aperture. All these were of speculum metal, and all on the Newtonian form. In 1870, Grubb completed for the Melbourne Observatory a telescope of 4 feet aperture, on the Cassegrain plan, the only large example. This is of speculum metal. In 1856 it was proposed by Steinheil, and in 1857 by Foucault, to use glass as the material for the concave mirror, covering the surface with a fine deposit of metallic silver in the manner that had then just been perfected. In 1858, Draper, in America, completed one on this plan of 15 inches aperture, soon after making another of 28 inches. In France several large ones have been made, including one of 4 feet at the Paris Observatory: in England this form of telescope is largely used, and mirrors up to 5 feet in diameter have been made and mounted equatorially.

Optically the astronomical telescope, particularly the refractor, has arrived at a splendid state of excellence; the purity of the glass disks and the perfection of the surfaces is proved at once by the performance of the various large telescopes. No limit has yet been set to the increase of size by the impossibility of getting disks of glass or working them, nor is it probable that the limit will be set by either of these considerations. We must rather look for our limiting conditions to the immense cost of mounting large glasses, and the absorption of the glass of which the lenses are made coming injuriously into play to reduce the light-gathering power, though it will be probably a long time before this latter evil will be much felt.

With the reflecting telescope the greater attention given to the working and testing of the optical surface has enabled the concave mirror to be made with a certainty that the earlier workers never dreamed of. The examination of the surface can be made optically at the centre of curvature of the mirror in the manner that was used by Hadley in the beginning of the last century, and revived some years ago by Foucault, who brought this method of testing specula to a high degree of perfection; in fact, with the addition of certain methods of measuring the longitudinal aberrations we have now a means of readily testing mirrors with a degree of accuracy that far exceeds the skill of the worker. It enables every change that is made in the surface during the progress of the figuring, as the parabolization of the surface is called, to be watched and recorded, and the exact departure of any part from the theoretical form measured and corrected; mirrors can be made of very much greater ratio of aperture to focal length. I have one here where the focal length is only $2\frac{1}{4}$ times the aperture: such a mirror in the days of speculum metal mirrors with the methods then in use would have necessarily had a focal length of about 20 feet. The difference in curvature between the centre and edge of this mirror is so great that it can be easily measured by an ordinary spherometer, amounting as it does with one of 6 inches diameter to 3/10,000 of an inch, an amount sufficient to make the focus of the outer portion about 1 inch longer than the inner when it is tested at the centre of curvature. The diagram on the wall, copied roughly from one of the records I keep of the progress of the work on a mirror during the figuring, shows how this system of measurements enables one to follow closely the whole operation.

The use of silver on glass as the reflecting surface is as important an improvement in the astronomical telescope as the invention of the achromatic telescope. It gives a permanency to a good figure once obtained that did not exist with the mirrors of speculum metal. To restore the surface of silver to the glass speculum is only a small matter now. How readily this is done may be seen by the practical illustration of the method I will give. I have here two liquids—one a solution of the oxide of silver, and another a reducing agent, the chief material in solution being sugar. I pour the two together in this vessel, the surface of which has been cleaned and kept wet by distilled water, which I shall partly empty, leaving the rest to mix with the two solutions; you will see in the course of about 5 minutes the silver begin to form, eventually covering the whole surface with a brilliant coating that can be polished on the outer surface as bright as that you will see through the glass.

Reflecting telescopes have advantages over the refracting

telescopes in many ways, but in some respects they are not so good. They give images that are absolutely achromatic, while the other form always has some uncorrected colour. They can be made shorter, and as the light-grasping power is not reduced by the absorption of the glass of which the lenses are made, it is in direct proportion to the surface or area of the mirror. They have not had in many cases the same care bestowed upon either their manufacture or upon their mounting as has been given in nearly every case to the refracting telescope. Speaking generally, the mounting of the reflecting telescope has nearly always been of a very imperfect kind—a matter of great consequence, for upon the mounting of the astronomical telescope so much depends. To direct the tube to any object is not difficult, but to keep it steadily moving so that the object remains on the field of view requires that the tube should be carried by an equatorial mounting of an efficient character. The first essential of such a mounting is an axis parallel to the axis of rotation of the earth. The tube, being supported on this, will follow any celestial object, such as a star, by simply turning the polar axis in a contrary motion to that of the earth at the same rate. If we make the telescope to swing in a plane parallel to the polar axis, we can then direct the telescope to any part of the sky, and we have the complete equatorial movement. There are several ways in which this is practically done: we can have a long open-work polar axis supported at top and bottom, and swing the telescope in this, or we can have short strong axes. As examples of the first, I will show you pictures of the mountings designed for Cambridge and Greenwich Observatories some forty years ago by Sir G. Airy, lately and for so long our eminent Astronomer-Royal; and as examples of the other form, amongst others, the large telescope lately erected at Nice, and also the larger one at Mount Hamilton, California, now under the direction of Prof. Holden.

The plan of bringing all the various handles and wheels that control the movement of the telescope and the various accessories down to the eye end, so as to be within reach of the observer, is carried to the highest possible degree of perfection here, as we can see by an inspection of the picture of the eye end of this telescope. The observer with the reflecting telescope is, with moderate-size instruments, never very far from the floor, but in the case of the Lick telescope he might have to ascend some thirty feet for objects low down in the sky; but, thanks to the ingenuity of Sir Howard Grubb, to whom the idea is due, the floor of the whole Observatory is made to rise and fall by hydraulic machinery at the will of the observer—a charming but expensive way of solving the difficulty, as far as safety goes, but not meeting the constant need of a change in position as the telescope swings round in keeping up with the motion of the object to which it is directed. The great length of the tube of large refractors is well seen in this picture of the Lick telescope; it suggests flexure as the change is made in the direction in which it points, and the consequent change of stress in the different parts of the tube.

The mounting of the reflector has been treated, if not so successfully, with more variety than in the case of the refractor as we shall see from the pictures I will show you, especially where the Newtonian form is used. The 4-foot reflector at Melbourne is mounted on the German plan, in a similar way to a refractor, and an almost identical plan has been followed by the makers of the 4-foot at the Paris Observatory. Lassell, who was the first to mount a large reflector equatorially, used a mounting that may be called the forked mounting, the polar axis being forked at its upper end, and the tube of the telescope swinging between the forks: a very excellent plan, dispensing with all counterpoising. Wishing to obtain certain conditions that I thought and think now favourable to the performance of the reflector, I devised a mounting where the whole tube was supported at one end on a bent arm; a 3-foot mirror was mounted on this plan in 1879, and worked admirably. The Newtonian form demands the presence of the observer near the high end of the telescope, and the trouble of getting him there and keeping him safely close to the eye-piece is very great. As we see from the various photographs, several means have been employed to do this, none of them quite satisfactory.

All the refracting telescopes of note in the world are covered by domes that effectually protect them from the weather; these domes are in some cases comparable in cost with the instruments they cover. It is not surprising, therefore, that efforts have been made to devise a means of getting rid of this costly dome and the long movable tube.

It was suggested many years ago that a combination of plane mirrors could be used to direct light from any object into a fixed telescope. This idea in a modified form has often been used for special work, one plane mirror being used as we see in the picture on the screen to throw a beam of light into a telescope fixed horizontally; for certain kinds of work this does admirably, but the range is restricted, as can be easily seen, and the object rotates in the field of view as the earth goes round. The next step would be to place the telescope pointing parallel to the axis of the earth and send the beam of light into it from the mirror, which could now be carried by the tube so that by simply rotating the tube on its own axis the object would be kept in the field of view. Sir Howard Grubb makes a small telescope on this plan, and some years ago proposed a somewhat similar plan. A sketch of this plan I will show you. You will see, however, that here again the range is restricted, and, to use the telescope, means would be required to constantly vary the inclination of the small mirror at one-half the rate of inclination of the short tube carrying the object-glass.

By the use of two plane mirrors, however, the solution of the problem of a fixed rotating telescope tube placed as a polar axis is solved. By having such a telescope with a plane mirror at an angle of 45° to the axis of the telescope in front of the object-glass, we can, by simply rotating the telescope, see every object lying on the equator; and by adding another similar plane mirror at an angle of 45° to the axis of the telescope, *as bent out at right angles by the first plane mirror*, and giving the mirror a rotation perpendicular to this axis, we obtain the same power of pointing the telescope as we have in the equatorial. The idea of doing this was published many years ago, but it was left to the skill and perseverance of M. Lœwy, of the Paris Observatory, to put it into practical use. He devised, and had made, a telescope on this principle, of 10½ inches aperture, which was completed in 1882. It has proved itself an unqualified success, and many other larger ones are now being made in Paris, including one of 23 inches aperture, now nearly completed, for the Paris Observatory.

A lantern copy of a drawing of this latter telescope will be thrown on the screen, in order that you may see what manifest advantages exist in this form of telescope. There is but one objection that can be urged—that is, the possible damage to the definition by the plane mirrors; but this seems, from what I have seen of the wonderful perfection of the plane mirrors made by the Brothers Henry, to be an unreasonable one—at any rate not an insurmountable one. In every other respect, except perhaps a slight loss of light, this form of telescope is so manifestly superior to the ordinary form that it must supersede it in time, not only for general work, but for such work as photography and spectroscopy.

ANNUAL VISITATION OF GREENWICH OBSERVATORY.

THE Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich, was read at the Annual Visitation on June 7. The Report presented refers to the year 1889 May 11 to 1890 May 10, and exhibits the state of the Observatory on the last-named day.

With respect to astronomical observations it is noted that, at the request of Dr. Gill, special attention has been paid to the oppositions of the minor planets Victoria and Sappho. Victoria has been observed 15 times on the meridian, and Sappho 9 times; while 244 observations have been made of 41 comparison stars for Victoria, and 151 observations of 42 stars for Sappho. At the request of Dr. Auwers, observations of the Sappho stars will be renewed in the autumn of this year, and an investigation made of the variation of personality with magnitude, for use in reducing the observations to a uniform system.

The Lassell, south-east, Sheepshanks, and Shuckburgh equatorials are in good working order. Great difficulty has been experienced at times in turning the south-east dome, and a careful examination shows that this may be largely due to the irregular shape of the cannon balls on which it rolls, and to a sagging of the dome curb in some parts.

The tube for the 28-inch refractor, which is of special construction, has been made by Sir H. Grubb in preparation for the object-glass which is now being figured. The experimental

4-inch object-glass referred to in the last Report was mounted on the Sheepshanks equatorial, and 18 photographs were taken with it last summer, the lenses being separated for photographic achromatism, and the crown lens reversed to correct for the spherical aberration introduced by the separation. The best distance of separation was determined, and the photographs obtained were found to be quite satisfactory. The completion of the 28-inch object-glass has been delayed presumably by the pressure of work on the 13-inch photographic telescopes, which have engaged so much of Sir H. Grubb's attention, but it is hoped that the new refractor will be ready for mounting very shortly.

The 13-inch photographic refractor, with 10-inch guiding telescope, by Sir H. Grubb, has been lately mounted in the new 18-foot dome, and one or two trial photographs have been taken with it.

Since the date of the last Report, 14 occultations of stars by the moon (9 disappearances and 5 reappearances) and 13 phenomena of Jupiter's satellites have been observed with the equatorials, or with the altazimuth. These observations are completely reduced to the end of 1889. The occultation of Jupiter by the moon on August 7 was observed with 5 instruments.

Comets have been observed with the Sheepshanks equatorial on 11 nights as follows: Comet *a* 1889 on 6 nights, Comet *d* 1889 on 1 night, Comet *a* 1890 on 4 nights.

The conjunction of Mars and Saturn on September 19 was observed with the south-east equatorial under favourable atmospheric conditions, and nineteen differential observations made of right ascension and north polar distance.

As regards spectroscopic and photographic observations, 457 measures have been made of the displacement of the F line in the spectra of 36 stars, and 20 of the *b* line in the spectra of 5 stars for determinations of motions of approach or recession. Observations of Algol on 7 nights confirm, as far as they go, the previous results indicating orbital motion. The observations of Spica made in past years are found by Prof. Bakhuyzen to be tolerably well represented on the hypothesis of orbital motion with a period of 4 days 0·386 hours, which agrees well with that recently discovered by Dr. Vogel with his photographic method. As the series of observations with the $12\frac{3}{4}$-inch refractor (extending over 15 years) will be shortly brought to a conclusion, it is proposed to discuss them with a view to the detection of orbital motion. The spectra of R Andromeda, χ Cygni, and Uranus, have been examined on several occasions, and Comet *e* 1889 on 1 night.

The sun has been free from spots on 211 days in the year 1889, the longest spotless period being October 23 to December 11. There were also eight other spotless periods of more than a fortnight. The mean daily spotted area in 1889 was 78, as compared with 89 for 1888: but the mean daily area for the latter half of the year was nearly twice as great as for the earlier half, being 103 as compared with 53. Again, the mean distance of spots from the equator was 5°·46 in the first six months, and 14°·72 for the last six; and both these facts thus point to the middle of the year 1889 as a well-defined date for the sunspot minimum.

The following are the principal results for the magnetic elements for 1889:—

Mean declination	17° 34'·9
Mean horizontal force	3·9494 (in British units).
	1·8210 (in metric units).
Mean dip	67° 22' 52" (by 9-inch needles).
	67 23 58 (by 6-inch needles).
	67 25 36 (by 3-inch needles).

In the year 1889 there were only two days of great magnetic disturbance, but there were also about twenty other days of lesser disturbance, for which tracings of the photographic curves will be published, as well as tracings of the registers on four typical quiet days.

The mean temperature of the year 1889 was 48°·8, being 0°·4 below the average of the preceding 48 years. The highest air temperature in the shade was 86°·6 on August 1, and the lowest 18°·7 on March 4. The mean monthly temperature in 1889 was below the average in all months excepting May, June, and November. In February and December it was below the average by 2°·4 and 2°·2 respectively, and in May above by 3°·9.

The mean daily motion of the air in 1889 was 245 miles, being 39 miles below the average of the preceding 22 years. The greatest daily motion was 736 miles on October 7, and the least 25 miles on September 3. The greatest pressure registered was 15 lbs. on the square foot on October 7.

The number of hours of bright sunshine recorded during 1889 by the Campbell-Stokes sunshine instrument was 1156, which is about 146 hours below the average of the preceding 12 years, after making allowance for difference of the indications with the Campbell and Campbell-Stokes instruments respectively. The aggregate number of hours during which the sun was above the horizon was 4454, so that the mean proportion of sunshine for the year was 0·260, constant sunshine being represented by 1.

The rainfall in 1889 was 23·3 inches, being 1·3 inches below the average of the preceding 48 years.

It was mentioned in the last Report that the Indian invariable pendulums had been mounted in the Record Room under General Walker's supervision. The three pendulums have each been swung 8 times, at pressures of both 2 inches and 27 inches, and the observations completely reduced, giving the following results for number of vibrations in a mean solar day, reduced to vacuum, a temperature of 62°, an infinitely small arc, and sea-level; the corresponding values obtained at Kew being appended for comparison:—

Pendulum.	Greenwich.	Kew.
4	86,165·54	86,166·50
6	86,065·70	86,066·61
11	86,117·04	86,117·03

The tabulation of results for the period of the fifty years of observations will be completed at the end of this year, and will be useful for many purposes. In the twenty years' meteorological reductions, the values were grouped generally in months, mainly for the determination of diurnal inequalities of the thermometer and barometer. In the tables of meteorological averages now proposed, however, the values will be grouped by days, so as to exhibit mean values for each day of mean daily temperature, maximum, minimum, barometer, velocity of wind, frequency of gales, rainfall, and cloud, obtained from the Greenwich observations of fifty years, 1841–90.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The following are the speeches delivered by the Public Orator (Dr. Sandys, tutor of St. John's College) in presenting Sir Andrew Clark, Mr. Jonathan Hutchinson, Dr. John Evans, Prof. Sylvester, and Mr. A. J. Ellis for honorary degrees on June 10:—

Salutamus deinceps salutis ministrum, Aesculapii e filiis unum, quem idcirco praesertim Machaona nominaverim quod saeculi nostri oratorum cum Nestore ipso totiens consociatus est;—nisi forte, Romano potius exemplo delectatus, mavult Asclepiadis illius disertissimi nomen mutuari, quo medico et amico utebatur Lucius Licinius Crassus, saeculi sui oratorum eloquentissimus. In re publica partium liberalium studiosus, in re privata liberalitate singulari insignis, non modo medicinae sed etiam philosophiae et religionis penetralia ingressus est. Etiam antiquos meministis quondam non de corporis tantum salute sed etiam de rebus fere omnibus quae vitam anxiam et sollicitam reddant, ab ipso Aesculapio solitos esse oracula exposcere. Viri talis igitur, velut iurisconsulti Romani, domus, est velut civitatis oraculum, unde cives eius, ut Apollo Pythius apud Ennium dicit, consilium expetunt, non salutis tantum sed etiam "summarum rerum incerti," quos incepti certos "compotesque consili dimittit." Ergo virum, quem aut litterarum aut scientiae aut medicinae doctorem nominare potuissemus, iuris doctorem non immerito creamus.

Duco ad vos medicinae professorem emeritum, Regii Medicorum Collegii Londinensis praesidem, baronettum insignem, suavem, eruditum, eloquentem, Andream Clark.

Etiam alter Aesculapii filiorum, Podalirius (nisi fallor), hodie nobis sese praesentem obtulit, quem a fratre suo idcirco disiungere neque possumus neque volumus, primum quod professoris in munere quondam erat collega eius coniunctissimus, deinde quod forte quadam domum vicinam atque adeo proximam incolit, denique quod dignitate non minore Collegio alteri praesidet, ubi

Britanniae chirurgi per tot annos quasi penates suos posuerunt. Medicinae studiosis nota sunt scripta eius per seriem longam edita, in quibus pars ea medicinae quae manu curat illustratur, et litterarum monumentis mandatur. Neque silentio praeterire possumus quaecumque de pathologia praesertim, quam quondam profitebatur, accuratissime scripsit; scilicet mortem ipsam, quae aliis tacet, huic velut rerum naturae vati et interpreti constat esse eloquentem. Neque prorsus intacta relinquimus quicquid de morborum contagione disputavit. Medicorum nemo fortasse Horatii verba in re medica saltem eruditius illustravit:—

delicta maiorum immeritus lues.

Duco ad vos Regii Chirurgorum Collegii praesidem, chirurgum illustrem, JONATHAN HUTCHINSON.

Archaeologiae studia nonnulli certe arida mentis nutrimenta arbitrantur. Hic autem etiam difficili in materia ingenii sui non minus facilis quam felicis alimentum invenit, qui etiam silices duros diu habuit in deliciis, ex ipsoque saxo doctrinae scintillam saepenumero excudit,

suscepitque ignem foliis atque arida circum
nutrimenta dedit, rapuitque in fomite flammam.

Quicquid lapidis, quicquid aeris, quicquid auri et argenti Britannia antiqua usurpabat, assidue conquisivit; conquisitum erudite illustravit. Britanniae nummorum investigator acerrimus, propterea etiam ultra fretum Britannicum numismate aureo honoris causa donatus est. Neque antiquis tantum thesauris operam dedisse videtur, sed etiam Societatis Regiae praefectus aerario, tot scientiis auxilium quotannis certatim flagitantibus, pecuniae publicae dispensator providus, aequus, benignus exstitit. Quondam Geologicae, iamdudum Numismaticae Societati praepositus, nunc etiam Antiquitatis peritorum Societati maximae summa cum dignitate praesidet. Quot scientiarum trans provincias aquilas suas felices tulit! Quid si non (velut alter ille quem hodie expectabamus)—quid, inquam, si non "nomen ab Africa lucratus rediit," tamen laudes eius Musae nullae "clarius indicant, quam Calabrae Pierides, neque

si chartae taceant quod bene feceris
mercedem tuleris.

Audite igitur ipsum Ennium viri huiusce praeconia praesagientem:—

doctus, fidelis,
suavis homo, facundus, suo contentus, beatus,
scitus, secunda loquens in tempore . . .
multa tenens antiqua.

Duco ad vos virum de antiquitatis studiis praeclare meritum, JOANNEM EVANS.

Plusquam tres et quinquaginta anni sunt elapsi, ex quo Academiae nostrae inter silvas adulescens quidam errabat, populi sacri antiquissima stirpe oriundus, cuius maiores ultimi primum Chaldaeorum in campis, deinde Palestinae in collibus, caeli nocturni stellas innumerabiles, prolis futurae velut imaginem referentes, non sine reverentia quadam suspiciebant. Ipse numerorum peritia praeclarus, primum inter Londinenses Academiae nostrae studia praecipua ingenii sui lumine illustrabat. Postea trans aequor Atlanticum plusquam semel honorifice vocatus, fratribus nostris transmarinis doctrinae mathematicae facem praeferebat. Nuper professoris insignis in locum electus, et Britanniae non sine laude redditus, in Academia Oxoniensi scientiae flammam indies clariorem excitat. Ubicunque incedit, exemplo suo nova studia semper accendit. Sive numerorum θεωρίαν explicat, sive Geometriae recentioris terminos extendit, sive regni sui velut in puro caelo regiones prius inexploratas pererrat, scientiae suae inter principes ubique conspicitur. Nonnulla quae Newtonus noster, quae Fresnelius, Iacobius, Sturmius, alii, imperfecta reliquerunt, Sylvester noster aut elegantius explicavit, aut argumentis veris comprobavit. Quam parvis ab initiis argumenta quam magna evolvit; quotiens res prius abditas exprimere conatus, sermonem nostrum ditavit, et nova rerum nomina audacter protulit! Arte quali numerorum leges non modo poetis antiquis interpretandis sed etiam carminibus novis pangendis accommodat! Neque surdis canit, sed "respondent omnia silvae," si quando, inter rerum graviorum curas, aevi prioris pastores aemulatus,

Silvestrem tenui musam meditatur avena.

Duco ad vos Collegii Divi Ioannis Socium, trium simul Academiarum Senatorem, quattuor deinceps Academiarum Professorem, IACOBUM IOSEPHUM SYLVESTER.

Claudit seriem viri eiusdem aequalis, qui doctrinae rudimentis primum Salopiae, deinde Etonae, denique Trinitatis in Collegio maximo imbutus, eadem in Academia isdem e studiis lauream suam primam reportavit. Sed ne his quidem finibus contentus, etiam musices mysteria perscrutatus est, et philologiae provinciam satis amplam sibi vindicavit. Quanta perseverantia etiam contra consuetudinem, ut Quintiliani verbis utar, "sic scribendum quidque iudicat, quomodo sonat"! Quanta subtilitate de linguae Graecae et Latinae vocalibus disputat; quam minuta curiositate etiam patrii sermonis sonum unumquemque explorat! A poetis nostris antiquioribus exorsus, non modo saeculorum priorum voces temporis lapsu obscuratas oculis et auribus nostris denuo reddidit, sed etiam nostro a saeculo in dialectis variis usurpatam litterarum appellationem, signis accuratis notatam, posteritati serae cognoscendam tradidit. Venient anni (licet confidenter vaticinari) quibus dialectorum nostrarum tot varietates, non minus quam Arcadum et Cypriorum linguae antiquae, hominum e cognitione prorsus obsolescent; tum profecto viri huiusce scriptis cura infinita elaboratis indies auctus accedet honos.

Mortalia facta peribunt,
nedum sermonum stet honos et gratia vivax.

Interim a nobis certe sermonis Britannici conservator anim, grati testimonium, honoremque diu debitum, diu duraturum accipiet.

Duco ad vos philologum insignem, ALEXANDRUM JOANNEM ELLIS.

At the annual election at St. John's College, on June 16, the following awards in Mathematics and Natural Science were made:—

Mathematics—Foundation Scholarships continued or increased: Bennett (£100), Reeves (£80), Alexander (£70), Dobbs (£60), Finn (£50), Gedge (£40), Hough (£80), Chevalier (£60), Pocklington (£80), Rosenberg (£50). Foundation Scholarships awarded: Wills (£60), Owen (£50), Schmitz (£40), Pickford (£40), Maw (£40). Exhibitions: Dobbs, Wills, Finn, Owen, Schmitz, Pickford, Maw, Robertson, Bloomfield, Spaight, Ayers, Morton. Proper Sizarship: Le Sueur. Natural Science—Foundation Scholarships continued or increased: Groom (£60), Hankin (£40), Horton-Smith (£40), Hewitt (£80), Lehfeldt (£80), Woods (£40). Foundation Scholarships awarded: Blackman (£40), MacBride (£60), Cuff (£40), Whipple (£40). Exhibitions: Woods, Baker. Proper Sizarship: Baker. Hutchinson Studentship for Pathological Research, Hankin. Wright's Prizes: Mathematics, Hough; Natural Science, Hewitt, Lehfeldt, MacBride. Hughes Prize for most distinguished student of the third year, Bennett (Mathematics). Hockin Prize for Experimental Physics, Lehfeldt.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 12.—"A Record of the Results obtained by Electrical Excitation of the so-called Motor Cortex and Internal Capsule in an Orang Outang (*Simia satyrus*)." By Charles E. Beevor, M.D., F.R.C.P., and Victor Horsley, B.S., F.R.S. (From the Laboratory of the Brown Institution.)

Having been engaged for some time in investigating the representation of motor function in the cortex of the bonnet monkey, we thought it advisable to perform the same in an anthropoid as likely thereby to gain a closer insight into the modes of representation in man.

We first describe the peculiarities noticeable in the configuration of the convolutions in the orang.

As in the bonnet monkey, after narcotization with ether, we divided the cortex into squares of 2 millimetres side, and excited the same with minimal stimuli from the secondary coil of an inductorium; a remarkably high intensity of the stimulus being required.

General Results.—The mode of representation of motor function was found to be highly specialized. The general plan was identical with that seen in the bonnet monkey in that the representation of each segment and part of the body in the orang was arranged in the same order as that according to which we found the representation of the primary movements to be grouped in the macacque monkey.

In addition to this, the areas for the representation of the different parts of the body we found not to be continuous with each other, but that between the areas of representation (for instance, of the face and the upper limb) there were regions of inexcitable cortex showing a degree of differentiation not obtained in the lower monkey.

A further remarkable evidence of specialization was noticeable in the fact that excitation of any one point elicited rarely more than one movement, and only of one segment, *e.g.* simple flexion of the elbow. Consequently, any sequence of movement or march was conspicuously infrequent.

Finally, the character of each movement and its localization was recorded.

After the cortex had been removed, we proceeded to stimulate the fibres of the internal capsule, and the results obtained confirmed those obtained from the bonnet monkey, and at the same time showed the relative position of the cortical areas.

The internal capsule was exposed by removing half of one hemisphere by a horizontal section; the outlines of the basal ganglia were then transferred to paper ruled with squares of 1 millimetre, and the resulting movement obtained by stimulating each of these squares contained in the internal capsule was recorded. The movements obtained correspond generally with the results which we have in another paper presented to the Royal Society, and read on December 12, 1889.

Physical Society, May 16.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Lord Rayleigh exhibited and described an arrangement of Huyghens's gearing in illustration of electric induction. This gearing consists of two loose pulleys mounted on the same axle, with an endless cord laid over them, the loops or bights of which carry weighted pulleys whose planes are parallel to the axis on which the upper pulleys turn. If one of the latter pulleys be started to rotate, the other one turns in the opposite direction until such time as the speed of the first one becomes constant. Whilst this constant speed is maintained, the second pulley remains stationary, one weight being raised and the other lowered, but on retarding the motion of the first pulley, the second begins to turn in the same direction as the first. It will be noticed that the phenomena are analogous to those which occur in electric induction, where starting or increasing a current in one circuit induces an opposite current in a neighbouring circuit, whilst decreasing or stopping a current induces one in its own direction. Lord Rayleigh pointed out that in this apparatus there is nothing strictly analogous to electric resistance, for the friction does not follow the same law. The analogy, he said, was complete as regards there being no change of potential energy, and the mathematical equations for the kinetic energy of the system are precisely the same as those given by Maxwell for electric induction.—Dr. S. P. Thompson made a communication on Dr. Kœnig's researches on the physical basis of music, in the course of which Dr. Kœnig performed numerous novel and interesting experiments, clearly illustrating the subject to a crowded audience. After referring to the classical researches of the great mechanician, and to the remarkable precision with which his ingenious and unique acoustical apparatus is constructed, Dr. Thompson said the subject with which he wished to deal could be divided into two parts, the first relating to *beats*, and the second to the *timbre* of sounds. On the question of beats considerable discussion had taken place as to whether they formed independent tones if they were sufficiently rapid. Different authorities had come to different conclusions on the subject, the disagreement probably arising from the impure tones used in their investigations. Dr. Kœnig, however, had succeeded in making tuning-forks whose sounds are very nearly pure tones, and by the aid of such forks had conclusively answered the question in the affirmative. Before proceeding to show experimentally the truth of the conclusions arrived at, Dr. Thompson said it was necessary to define exactly the meaning of the term "harmonics." By this he meant tones whose frequencies are *true integral multiples* of their fundamental. This, he said, might seem to be identical with the "upper partial tones" of Helmholtz or the "overtones" of Tyndall, but such was not the case, as the upper partial tones of piano-wires, &c., are not true integral multiples of the fundamentals, for the rigidity of the wire comes into play, and prevents the subdivision being exact. According to Helmholtz's theory, two tones harmonize when they do not produce beats of sufficient slowness to grate upon the ear, and the frequency of the two sets of beats were supposed to be equal to the difference and the sum of the frequencies of the two fundamental tones. In investigating the subject, Kœnig finds it necessary to distinguish between primary and secondary beats, and also that primary beats belong to two categories. These categories he calls "inferior" and "superior" respectively, and the frequencies of the two sets correspond respectively to the positive and negative remainders obtained by dividing the number representing the number of vibrations in the tone of lowest pitch into the corresponding number for the higher tone. For example, two forks of 100 and 492 vibrations produce beats having 92 and 8 as their vibration frequencies, for

$$492 = 100 \times 4 + 92,$$

and also

$$492 = 100 \times 5 - 8.$$

A set of "superior" beats of 8 per second and an "inferior" beat-tone of 92 per second may be heard when two such forks are sounded together. These primary beats or beat-tones act as independent tones and produce secondary beats. Tertiary ones may also be obtained. To demonstrate the existence of beats to the large audience assembled, Dr. Kœnig had provided two large tuning-forks with resonators about 4 feet long. One of the forks gave 64 vibrations per second, and the other 128, but the latter had sliding weights, whereby its frequency could be made anything between 128 and 64. Adjusting the weights so as to give 72, and bowing both forks, the beats of about 8 per second were distinctly heard at the extremity of the room. By varying the weights so that the fork gave 80, $85\frac{1}{3}$, 96, $106\frac{2}{3}$, 112, 120, and 128 vibrations successively, beats of various frequencies were produced, and it was remarkable to note that tones of 64 and 120 produced 8 beats a second exactly like 64 and 72. When the forks made 64 and 96 vibrations—*i.e.* at an interval of a fifth—then the inferior and superior beats agree in frequency, viz. 32, and by careful observation a low tone of about this pitch could be heard. If the tones sounded simultaneously differ by more than an octave, the same law for the numbers of beats holds good, whilst Helmholtz's difference and summation tones law, is inapplicable. This was shown by sounding a fork and its double octave slightly mistuned by weighting; slow beats were quite evident, although the difference in the frequencies of the primary notes was large. Similarly forks vibrating approximately at rates in the proportions 1 : 5 and 1 : 6 gave slow beats. Coming to the main question, as to whether beats when sufficiently rapid blend into tones just as primary shocks do, Dr. Thompson briefly recalled the various arguments for and against such an effect, and then Dr. Kœnig proceeded to experimentally prove the affirmative. Taking two forks tuned to 2048 and 2304 vibrations respectively (ratio 8 : 9) and sounding them simultaneously, the middle C of the piano (256) was distinctly heard. The same beat tone resulted from forks having frequencies in the ratio of 8 : 15, whose negative remainder was 256. Various other tones were sounded simultaneously in pairs, and in all cases the corresponding beat-tone was quite distinct. In these experiments the existence of nodes and loops in air was particularly noticeable, for as Dr. Kœnig turned the tuning-forks in his hand, the intensity of the beat-tones heard at a particular spot varied enormously. The experiments were carried a step further by impressing vibrations of different frequencies on one and the same body: the beat-tones in this case were quite perceptible. In carrying this out, Dr. Kœnig had constructed steel bars of approximately rectangular section, whose periods of vibrations were different in two directions at right angles. Striking one face of the bar a certain note resulted, whilst a blow on an adjacent face produced a different one. When the bar was struck on the edge joining the two faces, both the notes could be heard as well as the beat-tone resulting therefrom. The experimenter had gone still further, and made such bars so short that neither of the fundamental notes are within the limits of audition, but the resulting beat-tone can be heard quite distinctly. In all cases the frequency of the beats agrees with that calculated from Dr. Kœnig's formula, and secondary beats follow the same law. It was then pointed out that not only beats, but the maxima of a series of pulsations varying in intensity will, if isochronous and sufficiently rapid, give tones, just as a series of primary shocks do. This was illustrated by tuning-forks, and by directing a stream of air issuing from a slit against a notched rim of a rotating disk. A further confirmation was given by a modified disk siren; in this the holes, instead of being of the same size all round a circle, increase to a maximum and then decrease again, there being several sets of such holes in one circumference. When this was put in operation, notes corresponding in pitch to the number of holes and also to the number of sets of holes,

could be heard. A wave siren was also used to illustrate the same fact. The matter was further illustrated by moving a tuning-fork towards a wall or other reflecting surface at various velocities. According to Doppler's principle, as the fork recedes from the observer and approaches the wall, the frequency of the direct waves is less and that of the reflected waves greater than that of the fork, and these two series of waves produce beats. By sufficiently increasing the velocity and using a fork of high pitch, the beats blend into tones. Coming to the second half of Dr. Kœnig's researches, Dr. Thompson said that Helmholtz contended that the *timbre* of musical sounds was not affected by differences of phase amongst the component tones; on this point, however, Kœnig had come to the opposite conclusion. To illustrate graphically why phase should affect *timbre*, a number of diagrams were exhibited, some showing the resultant wave-form produced by combining a tone with its harmonics of equal intensity, when the differences of phase between them were o, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ respectively; whilst others represented the wave-forms when the harmonics and the fundamental were of different intensities. The effect of phase on the shape of the wave-form was very marked. The subject was treated experimentally by means of a wave siren, against which a stream of air issuing from a slit could be directed. By inclining the slit to one side of the radius or the other, the phases of the component waves could be altered, and this had a marked effect on the character of the sound produced. Illustrations of Kœnig's multiple wave sirens, both of the cylinder and disk forms, were next shown, and the results of investigations made with the apparatus described. From these results it appears to be impossible to produce the *timbre* of instruments such as trumpets, clarionets, &c., by any combination of a tone and its pure harmonics. This led to the investigation of impure harmonics. By plotting and combining curves it was shown that the wave-form obtained from a tone and impure harmonics changes in successive periods; this peculiarity was observed to exist in a record taken from a vibrating string. Various disks with wavy edges of different form were spun before an air slit, and the varying character of the resulting sounds as the slit was turned, demonstrated. Before concluding, Dr. Thompson remarked that the word "*timbre*" requires to be re-defined, for the rigidity of strings, wires, &c., and the interference of the wood and metal parts of organ pipes and other wind intruments generally, prevent the formation of pure harmonics. A model consisting of vibrating strips placed vertically or inclined was exhibited to show the different kinds of *timbre*. The differences between mixtures and compounds of tones was pointed out, and the inability of the ear to distinguish between pure and impure sounds referred to. Lord Rayleigh thought more information was required on the important subjects brought forward, and asked in what class of musical sounds are the overtones strictly harmonious. He could admit that in piano wires they may not be so, but he was not quite so clear about organ pipes. He said he was filled with admiration by the perfection of the apparatus displayed, and expressed a wish that such mechanical acousticians could be found on this side of the Channel. Mr. Bosanquet said he had been carefully over the ground investigated by Dr. Kœnig. He believed Dr. Kœnig was the first to get at the facts concerning beats, but it was difficult to admit all that had been said about them. However, the chief difference between authorities seemed to be one of language. Owing to the lateness of the hour he could not discuss the question fully, and so asked to be allowed to reserve his opinion on the matter. As regards *timbre*, he thought the experiments on the effects of phase were not conclusive. The sounds of wind instruments such as trumpets, he said, depended greatly on who produced them. It was no easy matter to bring out their full sweetness, and it was comparatively few persons who could ever attain perfection. He ventured to think that in a properly used instrument none of the harmonics are out of tune. Mr. Blaikley agreed with Lord Rayleigh about piano wires, and as regards wind instruments he could hardly think that the overtones were so inharmonious as Dr. Thompson would have him believe. In fact, Mr. Stroh had obtained wave-forms for him from various instruments, but in none of them was there any discontinuity such as shown on one of the diagrams exhibited. However, he was of opinion that there is something in *timbre* not accounted for by the ordinary theory. The President said that in view of the production of audible sounds by the beats from notes beyond the range of audition, it might be possible to demonstrate that insects produce sounds inaudible to the human ear by putting several together in a box, and listening for the beat-tones. Dr. Kœnig acknowledged the most cordial vote of thanks accorded to himself and Dr. Thompson.

Zoological Society, June 3.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of May 1890, and called special attention to a pair of Hartebeests (*Alcelaphus caama*), and a pair of Swainson's Long-tailed Jays (*Calocitta formosa*), acquired by purchase; and to a pair of Beatrix Antelopes (*Oryx beatrix*), presented by Colonel E. C. Ross, Consul-General for the Persian Gulf.—Mr. Sclater exhibited and made remarks on two young specimens of Darwin's Rhea (*Rhea darwinii*), obtained by Mr. A. A. Lane in the province of Tarapacà, Northern Chili, and forwarded to Mr. H. H. James.—Mr. Sclater exhibited and made remarks on a flat skin of a Zebra, received from Northern Somaliland, which appeared to be referable to Grévy's Zebra (*Equus grevyi*).—Mr. A. D. Michael read a paper on a collection of non-parasitic *Acarina* lately made in Algeria, where he had found the *Acarina* less abundant than in England, and, indeed, almost absent from the true southern vegetation. The species met with were not of larger size than the British. The collection consisted almost entirely of Oribatidæ, and contained examples of 46 species belonging to 15 genera. Amongst them were 8 species new to science, 27 were British, and the rest South European. Amongst the new species were a remarkable new *Cæculus*, there being previously only one known species of this curious genus, which forms a separate family. There was also a new *Notaspis*, which had not been found in Europe, but had been received from the shores of Lake Winnipeg, in Canada. There were likewise some very singular new species of the genus *Damæus*, and a triple-clawed form of *Nothrus anauniensis*.—Mr. Frank E. Beddard read a paper on the anatomy of the Fin-foot (*Podica senegalensis*). The paper dealt chiefly with the myology and osteology of this doubtful form. The conclusion arrived at was that it showed most resemblance to the Rails, but that in its muscular anatomy it agreed in many particulars with the Grebes and Divers.—Mr. O. Thomas read some notes on the specimens of Mammals obtained by Dr. Emin Pasha, during his recent journey through Eastern Africa, as exemplified in the specimens contained in two collections presented to the British Museum and the Zoological Society respectively.—Mr. G. A. Boulenger read a paper containing the descriptions of two new species of the Siluroid genus *Arges*, from South America.—A communication was read from Mr. James Yate Johnson, containing descriptions of five new species of fishes from Madeira.

Linnean Society, May 24.—Anniversary Meeting.—Mr. W. Carruthers, F.R.S., President, in the chair.—The Treasurer presented his Annual Report, duly audited; and, the Secretary having announced the elections and deaths of Fellows during the past year, the President proceeded to deliver his annual address. In this he dealt with the distribution of British plants both before and after the Glacial period, making special allusion to the discoveries of Mr. Clement Reid amongst the vegetation of the Cromer Forest Bed, and showed that the forms which have come down to us at the present day do not differ in any respect from the same species found in the Glacial beds.—A vote of thanks was moved by Sir Joseph Hooker and seconded by Mr. Stainton to the President for his excellent address, with a request that he should allow it to be printed, and carried unanimously.—On a ballot taking place for new Members of Council, the following were declared to be elected:—Dr. P. H. Carpenter, Dr. J. W. Meiklejohn, Mr. E. B. Poulton, Mr. D. Sharp, and Prof. C. Stewart. On a ballot taking place for President and Officers, the following were declared to be elected:—President: Prof. Charles Stewart. Secretaries: B. D. Jackson and W. P. Sladen. Treasurer: Frank Crisp.—The Linnean Society's gold medal for the year 1890 was then formally awarded and presented to Prof. Huxley for his researches in zoology.

Entomological Society, June 4.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The Secretary exhibited, on behalf of Mr. J. Edwards, Norwich, two specimens of *Ilybius subæneus*, Er., and a single specimen of *Bidessus unistriatus*, Schr. Mr. Champion alluded to the fact that the only recorded British specimens of the first-mentioned beetle had been taken many years ago at Peckham. Lord Walsingham, in alluding to the exhibit, referred to the list of Norfolk

Coleoptera compiled some years ago by Mr. Crotch, which appears to have been lost sight of.—Mr. McLachlan alluded to the damage done by insects to orange-trees in Malta, and stated that the Rev. G. Henslow had lately been studying the question; one of the chief depredators was the widely-spread "fly," *Ceratitis citriperda*, well known as devastating the orange. He found, however, that another and more serious enemy was the larva of a large Longicorn beetle (*Cerambyx miles*, Bon.), which bores into the lower part of the stem and down into the roots, making large galleries; in all probability the larva, or that of an allied species, is the true *Cossus* of the ancients. Lord Walsingham stated that a species of *Prays* allied to *P. olcellus* and our common *P. curtisellus* was known to feed in the buds of the orange and lemon in Southern Europe.—The following papers were communicated, and were read by the Secretary:—Notes on the species of the families *Lycidæ* and *Lampyridæ* contained in the Imperial Museum of Calcutta, with descriptions of new species, and a list of the species at present described from India, by the Rev. H. S. Gorham.—A catalogue of the Rhopalocerous Lepidoptera collected in the Shan States, with notes on the country and climate, by Dr. N. Manders, Surgeon, Medical Staff. The latter paper contained a very interesting description of the chief physical features of the Shan States and neighbouring parts of Burmah.

Mathematical Society, June 12.—J. J. Walker, F.R.S., President, in the chair.—The President announced that the Council had unanimously awarded the De Morgan Memorial Medal to Lord Rayleigh, Sec.R.S., for his writings on mathematical physics.—The following papers were read:—On simplicissima in space of n dimensions (third paper), by W. J. C. Sharp.—Rotatory polarization, by Dr. J. Larmor.—Parabolic note, by R. Tucker.—Prof. Greenhill, F.R.S., communicated a paper by Prof. Mathews on the expression of the square root of a quartic as a continued fraction, and one by R. Russell on modular equations.—The President gave a brief sketch of a paper by A. R. Johnson, on certain concomitants of a system of conics and quadrics, and on the calculation of the covariant S of the ternary quartic.

PARIS.

Academy of Sciences, June 9.—M. Hermite in the chair.—On the movement of a prism, resting on two supports, submitted to the action of a variable normal force following a particular law, applied at a determined point of the axis, by M. H. Resal.—Theory of the state produced near to the wide opening of a fine tube where the threads of a liquid which flows there have not acquired the normal inequalities of velocity, by M. J. Boussinesq.—Action of the alkalies and alkaline earths, alkaline silicates, and some saline solutions on mica: production of nepheline, sodalite, amphigene, orthoclase, and anorthite, by MM. Charles and Georges Friedel.—On the fauna of deep parts of the Mediterranean around Monaco, by the Prince of Monaco. Some dredging operations carried on at various depths up to 1650 metres show that, at certain parts at least of these regions, the Mediterranean Sea is by no means devoid of inhabitants as has been previously asserted.—Observations of Brooks's comet (a 1890), made with the *coudé* equatorial of Algiers Observatory, by MM. Rambaud and Renaux. The observations of position extend from May 10 to 31.—Photographic observation of Brooks's comet made at Algiers Observatory, by M. Ch. Trépied (see "Our Astronomical Column").—On a particular case of the movement of a point in a resisting medium, by M. A. de Saint-Germain.—Propagation of light in gold-leaf, by MM. Hurion and Mermeret.—On the amplitude of the diurnal variation of the temperature, by M. Alfred Angot. The author shows how the diurnal temperature variation in any station on the earth may be expressed by the formula—

$$a = \frac{K}{r^2} (A + B \sin l + C \cos 2l),$$

in which K is a function of cloudiness, and $= 1$ when the sky is clear, A, B, and C are coefficients depending only upon the geographical position of the station and its climatological characters, l the sun's longitude, and r the distance of the earth from the sun.—Electrolysis of fused aluminium fluoride, by M. Adolphe Minet. The author finds a mixture of 40 parts of the double fluoride of aluminium and sodium with 60 parts of sodium chloride to give him the best results yet obtained.—On the isomeric states of chromium sesquibromide: the blue sesquibromide, by M. A. Recoura. A method of preparing the solid hydrated bromide, $Cr_2Br_6, 12H_2O$, corresponding to the violet solutions is given. It is shown that the grey-blue solid obtained is less stable than the green crystals formerly described, whereas the violet solutions corresponding to the blue solid salt are more stable than the green solutions; thermochemical data are given in confirmation.—On the estimation of zinc in the presence of iron and manganese, and its separation from those metals, by M. J. Riban. The zinc is separated as sulphide from a solution to which has been added an excess of sodium thiosulphate.—On the composition of clays and kaolins, by M. Georges Vogt.—On the synthesis of the fluorides of carbon, by M. C. Chabrié.—On the products of saccharification of amylaceous matters by acids, by M. G. Flourens.—On the decomposition of organic manures in the soil, by M. A. Muntz.—On the anatomy of horny sponges of the genus *Hircinia*, and on a new genus, by M. H. Fol.—On the circulatory system in the carapace of decapodous Crustacea, by M. E. L. Bouvier.—On two new species of Coccidia, parasitic on the stickleback and sardine, by M. P. Thélohan.—Interesting nuclear modifications of the nucleolus which may ultimately throw some light on its signification, by M. E. Bataillon.—On a hymenopterous insect injurious to the vine, by M. E. Olivier.—On the diversities and similarities in some dentary systems of mammifers, by M. Heudes.—Researches on the development of the seminal integuments of Angiosperms, by M. Marcel Brandza.—On the nature of the phosphate beds of Dekma (département de Constantine), by M. Bleicher.—On the existence of marine deposits of the Pliocene age in the Vendée, by M. G. Vasseur.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Japan and the Pacific: M. Inagaki (Unwin).—The Mineral Resources of Ontario (Toronto).—A Treatise on Practical Chemistry and Qualitative Analysis, 5th edition: Dr. F. Clowes (Churchill).—Primo Resoconto dei Risultati della Inchiesta Ornitologia in Italia; Parte Seconda, Avifauna Locali: E. H. Giglioli (Firenze).—The Species of Ficus of the Indo-Malayan and Chinese Countries, Appendix: Dr. G. King (Calcutta).—Sammlung von Vorträgen und Abhandlungen, Dritte Folge: W. Foerster (Berlin, Dümmler).—Lehrbuch der Verg. Entwicklungsgeschichte der Wirbellosen Thiere, Specieller Theil, Erstes Heft: Dr. E. Korschelt and Dr. K. Heider (Jena, Fischer).—The Life and Letters of the Rev. Wm. Sedgwick, 2 vols.: J. W. Clark and T. McK. Hughes (Cambridge University Press).—The Forest Flora of South Australia, Part 9: J. E. Brown (Adelaide).—Les Bactéries, 2 vols.: A. V. Cornil and V. Babes (Paris, Alcan).—Physiological Botany: Dr. G. L. Goodale (Macmillan).—An Elementary Treatise upon the Method of Least Squares: G. C. Comstock (Arnold).—The Lepidopterous Fauna of Lancashire and Cheshire: J. W. Ellis Leeds (McCorquodale).—La Révolution Chimique Lavoisier: M. Berthelot (Paris, Alcan).—Beiträge zur Geologie Syriens, Die Entwickelung des Kreidesystems in Mittel- und Nord-Syrien; eine Geognotisch-Paläontologische Monographie: Dr. Max Blanckenhorn (Berlin, Friedländer).—Zur Kenntniss der Fauna der "Grauen Kalke" der Sud-Alpen: Dr. L. Tausch v. Gloeckelsthurn (Wien, Hölder).—The Law and Practice of Letters Patent for Inventions: L. Edmunds and A. W. Renton (Stevens).

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THURSDAY, JUNE 26, 1890.

ZOOLOGICAL GEOGRAPHY.

La Géographie Zoologique. Par le Dr. E. L. Trouessart. Avec 63 figures intercalées dans le texte et deux cartes. (Paris: J. B. Baillière et Fils, 1890.)

DR. TROUESSART, author of a "Catalogue des Mammifères Vivants et Fossils," and until recently Curator of the Museum at Angers, has enriched the Bibliothèque Scientifique Contemporaine with a most interesting and valuable book on zoological geography. This work must have caused its author a great amount of labour, to judge from the painstaking way in which he has worked in the facts collected by numerous specialists. Their results, and those of his many predecessors in the fascinating field of the distribution of animals, have been augmented by his own views, and have been condensed into a form which it is agreeable and easy to read.

The first six chapters are devoted to a description of the various zoo-geographical regions as they are now generally accepted. The different types of animals which are to serve as a basis for the investigation of the laws of geographical distribution are grouped in four classes, according to their means of dispersion and their usual habitats: terrestrial, fresh-water, aërial—*i.e.* provided with wings—and marine. The author has greatly increased the value of his book by the graphic method he has employed to show the distribution of given groups of creatures.

The general scheme is given on p. 175, the eight regions into which the author divides the globe being indicated by blocks, which are arranged and connected with each other as follows:—

Arctic Region
Palæarctic Region Nearctic Region
Ethiopian Region—Oriental Region Neotropical Region
Australian Region
Antarctic Region.

The *mammalian distribution*, when expressed by such a lucid scheme, comes out thus:—

Arctic
Holarctic [1]
Ethiopian Neotropical
Australian
Antarctic.

This indicates that, so far as mammals only are concerned, the Palæo- and Nearctic regions are practically one, while the Oriental is merged in the Ethiopian region. Australia stands, of course, alone; but that this continent must have been once connected with the Indo-Malayan countries is strongly indicated by the dingo and several other, chiefly rodential, placental mammals in Australia. The discovery by Prof. MacCoy of fossil bones of the dingo in Pliocene strata of Victoria disposes at once of the hypothesis of its having been introduced by man.

The distribution of *reptiles* (p. 204 *f*) is almost entirely based upon G. A. Boulenger's results, as published by him in the "Catalogue of Lizards and Tortoises in the British Museum," and shows to what valuable account such a publication can be turned if worked out upon a proper basis:—

(Palæogæa) (Neogæa)
Occidental + Oriental Nearctic Region
\+ \+ \+
African Australian and Neotropical Region
Polynesian Regions
New Zealand Region.

This scheme indicates that in the distribution of reptiles the principal relationships range vertically, with few or hardly any (except, of course, in Europe and Asia) transverse or longitudinal similarities. The globe is practically divisible into two great regions—namely, into Neogæa and Palæogæa, or into an American, Oriental, and Occidental region, New Zealand being a remote and peculiar appendix of the Oriental portion.

On p. 215 *f*, *terrestrial insects*, especially Coleoptera, are discussed. The two Polar regions possess a few forms only, and need hardly be considered. The Holarctic region, connected by the broad vertical belt of Polynesia and the west coast of both Americas with the Australian, New Zealand, and Patagonian regions, is comparatively poor in Coleopterous types, and the forms which occur have certain resemblances in common. Two large centres of rich development in forms and numbers are the Indo-African or Ethiopian, and the Brazilian or typical Neotropical regions.

Région Microtypique (Nord)
\+
Indo-African Polynesia Brazilian
Region Region
Australia Patagonia
New Zealand
Région Microtypique (Sud).

The division of the globe into Palæogæa and Neogæa is equally applicable to the *Arachnids*, the differences between Arcto and Notogæa being of by far less importance. Arachnid regions are: (1) Palæarctic, (2) Ethiopian, (3) Oriental, (4) Australian, (5) American. The whole of America has practically an Arachnid fauna from north to south, and it is divisible into eight sub-regions, which do not correspond with those of Wallace. The Ethiopian Arachnid region comprises the whole of Africa south of the Atlas, and Central Arabia; it has therefore been called the Libyan region, since it differs by the whole extent of the Sahara from the Ethiopian region of Wallace. The Oriental and Australian regions are those of Wallace, but the Oriental includes Madagascar and South-Eastern Africa as a sub-region. Certainly, so far as Coleoptera and Arachnida are concerned, Madagascar is much more Malayan than African. The Palæarctic region, as a whole, is that of Wallace, but the four sub-regions are differently arranged—namely, Europeo-Siberian, Hispano-Italian (Western Mediterranean and Canary Islands), Taurian or Eastern Mediterranean, with Asia Minor and the Turanian Steppes,

[1] For Holarctic the more convenient and more correct name of the Periarctic region might be substituted. Holarctic should logically include the Arctic together with its subdivisions. Triarctic, an American term, means, of course, Arctic + Palæo + Nearctic, while Periarctic would indicate what is wanted—namely, the Holarctic minus the Arctic region.—H. G.

and lastly the Manchurian sub-region, which consists of China and Japan.

The scheme which represents, after Boulenger, the distribution of *Amphibia*, strongly indicates their distribution in parallel zones—namely, a northern, equatorial, and southern zone. Australia is obviously American in character, but the Indian region includes the whole of the Malayan islands and even New Guinea.

Palæarctic—Nearctic Region

Ethiopian + Indian Region   Neotropical Region
+
Australian Region.

Concerning *fresh-water fishes*, after Dr. Günther, the diagram shows at a glance that the form of the continents has little influence upon the distribution. There are likewise parallel zones, one of which, the Arctic, contains no fresh-water fishes.

Northern Zone = Palæarctic + Nearctic Region.
Equatorial Zone =
African + Indian (Cyprinoid Section)   Pacific or Australian + Neotropical Region (Acyprinoid Section)
Antarctic Zone = $\underbrace{\text{Tasmanian} \quad \text{New Zealand} \quad \text{Patagonian}}_{\text{Sub-regions.}}$

The distribution of *terrestrial mollusks* is that of S. P. Woodward and P. Fischer's "Manuel de Conchyliologie," 1887. The six molluscan regions correspond almost exactly with those of Wallace, with this exception, that the Patagonian or Chilian sub-region is elevated to the rank of a seventh region.

In dealing with the distribution of *flying creatures*, the author rightly draws attention to the circumstance that the recent volcanic outbursts of Krakatāo might have killed all the terrestrial inhabitants with the exception of such animals as could fly to neighbouring islands. Therein lies, according to M. Trouessart, the explanation why the Polynesian islands are inhabited by bats and birds only, some of which are peculiar island forms. But these he considers to be the last survivals of a Polynesian fauna, of which we have now only dispersed members. This is one of those perplexing ideas which, although arrived at by a perfectly logical process of thinking, are nevertheless without any real justification.

For the distribution of *birds* Dr. Reichenow's six zones have been adopted; these zones are widely different from the now time-honoured six regions.

Arctic Zone
Oriental Zone   *Occidental Zone*
Oriental Temperate Region   Occidental Temperate Region
Ethiopian Region   Malayan Region   South American Region
Madagascar Zone   *Australian Zone*
Antarctic Zone.

The Oriental and Occidental zones are, of course, nothing but the Palæo- and Neogæa; the word zone should not be applied to eastern and western hemispheres, but rather to horizontal belts. Dr. Reichenow lays stress upon the idea that the annual migration of European and of Asiatic birds shows their connection with the Ethiopian and Malayan regions; hence their combination into one zone, together with what has been called hitherto the Palæarctic region. Madagascar has been elevated to separate rank, and so have the Arctic and Antarctic zones, so that on the whole the old arrangement (based by Dr. Sclater chiefly upon birds) has been completely altered. Birds are far less cosmopolitan than one might suppose, judging only by the strength of their wings.

The distribution of *Lepidoptera* is rather surprising, because the applied schematic representation shows that there are only three great regions. The New World, from Canada to Cape Horn, stands alone; Africa forms only a sub-region of the large Holarctic region, which includes Canada; while Australia forms a sub-region of the Indian region, which again gradually merges in the eastern half of the Holarctic. These results, however, are based upon the somewhat antiquated conclusions drawn by Koch and Staudinger in 1850.

Holarctic Region

American Region   African Sub-region   Indian Region

Australian Sub-region.

According to the Spanish naturalist, J. Bolivar, the distribution of the Orthoptera, which, being possessed of great power of flight, are given to long migrations, agrees rather with that of a part of the Coleoptera.

Pp. 280 and 281 contain a map of the world, on Mercator's projection, upon which the principal ocean currents are indicated, and by conventional signs the distribution of seals, sea-lions, penguins, and auks. The Sphenisciclæ have been carried along the west coast of South America as far north as the Galapagos Islands by the cold Humboldt's current, a circumstance which, by the way, was first pointed out by the late Dr. Watson in his *Challenger* Report on the Spheniscidæ. The Pinnipede genus Macrorhinus follows strange lines across the Pacific Ocean, apparently in conformity with existing currents, but the conclusions as to the original home of these sea-elephants seem somewhat far-fetched. The seal *Pelagius monachus*, until recently considered a chiefly Mediterranean species, is known to occur at the Canaries and at Madeira. Another closely allied species, *P. tropicalis*, Gill, has been discovered at certain lonely islands off Yucatan. But is it probable that this species owes its origin to a small group of the Mediterranean species, which has been carried across the Atlantic, past the numerous Antillean islands, almost to the mainland of Central America, there to be transformed into a new species? Is it not more likely that the occurrence of Mediterranean seals in the Atlantic is due to a formerly wider extra-Mediterranean distribution, especially since remnants of such seals have been found recently in the Furfoz caves near Setubal?

The eleventh chapter deals with the faunas of deep seas, high mountains, coast zones, lacustrine and subterraneous regions.

The twelfth, the last chapter, gives a very short account of the distribution of animals in time. Hardly any types of fossil animals are known to have existed in all regions of the globe. The largest animals enjoyed the most restricted range, both in space and in time. The small size and the early occurrence of mollusks and insects explain their now almost cosmopolitan distribution, while the greater abundance of Tertiary mammals, with their subsequent local extermination, gives the clue to

their present often scattered range. The great divisions of the world into Palæogæa and Neogæa are confirmed by palæontology. Still more marked, however, is and has been the contrast between Arcto- and Notogæa; so that one feels inclined to suppose that the Neogæa is only an exaggerated extension of the Notogæa.

Such is a short outline of the contents of this book, which, we feel sure, everyone interested in the study of geographical zoology or of zoological geography, as the case may be, will be pleased to read.

H. GADOW.

JEVONS AND MILL.

Pure Logic and other Minor Works. By W. S. Jevons. Edited by Prof. Adamson and Harriet Jevons. (London: Macmillan and Co., 1890.)

THE services of the late Prof. W. S. Jevons to logic were so eminent that considerable interest attaches to his minor writings on that subject, which are now collected into a volume. The earlier works, "Pure Logic" and "The Substitution of Similars," which are contained in the first and larger part of the volume, possess, indeed, no more than a historic value. They expound his well-known theory of equational logic, but for all practical purposes they are replaced by the later and more interesting exposition which is contained in the "Principles of Science."

The second part of the volume is a reprint of the articles which Jevons contributed to the *Contemporary* in criticism of J. S. Mill. A short chapter on the method of difference is all that the editors were able to add to them out of the mass of manuscript which the author had in preparation for a systematic criticism. These essays do not add to Jevons's reputation. They are a passionate indictment of Mill's consistency: Jevons thought that "Mill's mind was essentially illogical," and in the name of logic he thought it his duty to undermine the authority of Mill's writings. It may be doubted whether any work of theory could bear such a strain of rigorous verbal precision as Jevons endeavours to impose upon Mill. However, not even the most devoted admirers of Mill would maintain that Mill was a consistent thinker; they would find his merit elsewhere. To them and to others it will seem that Jevons has left behind him a criticism far worthier both of himself and of Mill, in the positive advances which he made upon Mill's doctrines in his own work on the principles of science, in the light which he threw upon the fundamental nature of induction, upon the function of hypothesis, and upon Mill's so-called deductive method.

Among the generation which is now entering upon maturity many persons must have passed through a similar history in their feelings with regard to Mill. They became acquainted with his philosophy in youth, and were carried away by its apparent clearness, its freshness and youthful feeling, its love of truth, its dignity, and the large views it gave them of human thought and human life. It opened to them a new world of thought and feeling. Afterwards, as they reflected upon it, with the help of teachers anxious that enthusiasm should not blunt the edge of their pupils' powers of rigorous thinking, they discovered that it was riddled with contradictions real as well as verbal; was full of doctrines laid alongside of each other without adjustment. By and by, when they recovered from the shock of this discovery, they began to perceive that its very errors were light-giving, that its inconsistencies were due to Mill's large-mindedness, his susceptibility to every side of a subject; that where his reasoning was least rigorous it was often most stimulating, and directed inquiry upon new and truer doctrines; and perhaps they often fell into the paradox of cherishing its errors above its truths. Such persons will be inclined to resent a criticism which contents itself with exposing the obvious contradictions of Mill's philosophy.

It is paying poor respect to a thinker to excuse his want of consistency. But with a writer like Mill, above all others, a mere destructive criticism conveys a positively false impression. It is for any higher purpose of little value, because it fails to point out the real significance of the incriminated doctrines. And this is just the vice of Jevons's attack.

Let it be granted at once that Mill's doctrines of geometrical axioms, of the foundation of induction, of pleasure, contain glaring contradictions. Mill holds that geometrical axioms are derived from experience; but while he admits that there is no such thing in existence as a straight line, he declares that we can reason about straight lines because our ideas of spatial figures exactly correspond to the reality. He declared that all induction rests ultimately, in a syllogistic relation, upon the law of causation, and at the same time that this principle is itself derived from particular inductions by the process of simple enumeration, which he elsewhere stigmatizes as vicious. With a theory of pleasure not different in principle from that of Hume and Bentham, he at the same time asserts a distinction of pleasures in kind. In exposing these contradictions, as well as in pointing out the difficulties of the method of difference, Jevons is completely successful; but in leaving the reader to infer that Mill's doctrines are therefore valueless he omits the most necessary part of the critic's task. Mill distinctly says that the axiom that two straight lines cannot inclose a space represents the limit to which many actual experiences approximate. It is true that he did not solve the ultimate difficulty of the relation to reality of such a limiting proposition—call it a hypothesis, or call it an ideal experiment. But, in spite of the gratuitous inconsistencies he introduces into the argument, Mill's contention remains unassailed that geometrical truths derive their authority from the same source as all other truths. With regard to the basis of induction, a more impartial criticism would have pointed out that Mill failed because he was untrue to himself. His doctrine of the syllogism is one of the most important contributions ever made to logic, but if he had been true to it he would have given to the law of universal causation as major premiss of the inductive syllogism a function like that which he assigns to the major premiss of every syllogism, and both secured the consistency of his whole theory as well as the truth of this particular doctrine. His distinction of pleasures according to kind is impossible on his own theory, its real position in his mind uncertain, and its suggestiveness in any case small; but, with its appeal to the judgment of

the good man, it corresponds to a real fact that pleasures do differ according to the position they occupy in the whole moral order, and that this is reflected in the judgment of good men.

But while Jevons's critical attack is successful in the above points, it fails even of its limited object in the attack on Mill's doctrine of resemblance, which is a mere verbal criticism and a misinterpretation. Mill limits the name of propositions of resemblance to those which explicitly state resemblance, or the particular form of resemblance called equality. But because he shows that attributes, propositions, syllogisms, inductive methods, analogy, all involve resemblance, and the word is used on every page of the discussion of these subjects, Jevons accuses him of contradiction. The fallacy of the criticism is obvious. Though all argument and reasoning may depend on resemblance, they need not be concerned with resemblances as such. Who would say when he feels two similar impressions, and feels them therefore similarly, that he necessarily feels and thinks of their similarity as an explicit relation subsisting between them?

The sketch which Prof. Adamson gives of Jevons's full plan leads to the presumption that the rest of his criticisms would have been of the same kind as those published. As one of the subjects discussed is the theory of the syllogism, and of inference from particulars to particulars, reference may be made to the impartial and sagacious treatment of the same subject in Mr. Bosanquet's "Logic," made by a writer of a very different school from Mill. Those who look to what Jevons effected in political economy and logic will not be able to avoid regretting that he should have felt it his duty to bestow so much of the energies of his fine intellect upon a task for which, except for an acuteness not much greater than that of hundreds of students of Mill, he was disqualified by lacking the most essential requisite of a critic.

S. A.

THE WASHINGTON MEDICAL LIBRARY.

The Index Catalogue of the Library of the Surgeon-General's Office, U.S.A. Vol. X. O—Pfutsch. (Washington: Government Printing Office, 1889.)

IT has always been a pleasure to watch the steady growth of this unique Catalogue, and the pleasure increases when we see it now within four, or at most five, years of its completion with the same accurate finish in detail as when its first volume appeared in 1880. It bears on it throughout the stamp of Mr. J. S. Billing's hand, and the elaborate method of cataloguing both books and all signed journalistic articles under the subject-heading, as well as all the books and republished articles under the author's name also, has been fully justified in its results, and has shown its very high value in these ten volumes. This volume can give some clue to the labour that has been involved in that system by its article on "Periodicals," which has been most justly thought so remarkable, as well as useful, as to have been republished by itself. Room can just be found in 212 large quarto pages for the titles of the medical journals—daily, weekly, monthly, quarterly—and the annual reports not only of hospitals, but of all medical and surgical societies, on many matters touching more or less on professional matters. These amount to some 7250 entries, and some 43,670 volumes. That is a total of medical periodical literature which is not approximately reached by the British Museum, the Bibliothèque Nationale of Paris, or any other library, general or professional, in the world. Of course some thousands of these entries—about half, in fact—do not represent living current publications, but about 3600 may be calculated as the total of current medical periodicals catalogued, using the term periodical in the wide sense that will include such publications as the "*Theriaki*, a Magazine devoted to the interests of the opium-eater," the "*Revue Spirite*, ed. par Allan Kardec," and the "*American Rushlight*, by Peter Porcupine." We do not notice any single continuous periodical that has published more than 314 bound volumes such as are furnished by the *Annales de Chimie*, which has been uninterrupted since 1790. A few old Latin *Annales*, or *Acta*, date back to 1692-6, but do not run to any length.

Looking at them as distributed by the countries of their publication, the largest number of past and current together falls to the United States, viz. about 2000; but it must be admitted that on the whole they are smaller and shorter-lived than their fellows, and are more constantly changing their names, a point which is carefully and usefully noted in the Catalogue. The German Empire has rather more than France, viz. about 1100 to 900; Great Britain about 700, Italy about 450, and so on till we come to Syria with two, and Malta with only one. Among so many it can hardly be feasible to avoid every possible mistake. It is a pity, for instance, to enter two such similar publications as the Transactions of the Royal Medical and Chirurgical Society, and of the Clinical Society, the one under Medical and Chirurgical, and the other under Transactions.

This immense mass of literature, however, gives to anyone who looks into it a very striking impression of all the careful labour that must have been necessary to tabulate all the articles in these so-called periodicals under the subject-headings, as has been done, so that the inquirer under any of the commoner subjects may find himself at any moment referred back to an article in a Dutch paper more than 150 years ago.

The "Pest" is the name chosen under which to group all the ancient and modern accounts of the vague and terrible plagues. Under that heading are to be found four editions of Defoe's classical tract on the Plague of London. The collection under this heading of archæological works well illustrates the energy of the American librarians, and the funds that must have been placed at their disposal, for we find of books printed in the fifteenth century 6 dealing with it, of the sixteenth century 169, and of the seventeenth century 207—of themselves not an easy collection to make in the last 30 years on either side of the Atlantic.

A. T. Myers.

OUR BOOK SHELF.

Food in Health and Disease. By J. Burney Yeo, M.D., F.R.C.P., Professor of Clinical Therapeutics in King's College, London, and Physician to King's College Hospital. (London: Cassell and Co., 1889.)

A GOOD book on food is greatly wanted, one treating of the varieties of food, and their arrangement in the dietaries of health and disease. In some respects Dr.

Yeo's small work fulfils the requirements of a satisfactory book on the subject. It will be found useful for reference by the busy practitioner, and it contains numerous facts, as a rule clearly stated; and it will perhaps also be found acceptable to the lay public, as, in many parts, the style is more or less popular. The chemistry of food-stuffs is not treated as accurately as it might be. Thus we have "syntonin or muscle fibrin; myosin, from muscle," placed in separate lines as food-stuffs. In the table (p. 10), "casein" (probably a misprint for ossein) is placed under "gelatigenous substances"; and gelatin is itself considered a "gelatigenous" substance. This, it must be confessed, is a somewhat loose way of describing these substances.

Dr. Yeo makes the statement (p. 16) that albumen, together with water and salts, is able "alone to support the vital processes," and can "replace in nutrition the fats and carbohydrates." With this statement most physiologists would disagree. Several more instances of somewhat vague statements might be quoted from the work. Milk is considered by all classical writers on the subject a complete or perfect food; but Dr. Burney Yeo goes further than this, and classes eggs as "the only other complete food afforded by the animal kingdom" (p. 51): "but when regarded in the light of a complete food, the shell must be taken into account" (p. 69). In a second edition of the work, the physiological and chemical portion wants careful revision.

In the discussion of the diet in disease, Dr. Yeo is more at home; and he has set forth the various modes of dietetic treatment of disease in a clear manner. The only fault to be found with this part of the book is that the style is somewhat too diffuse to be of great service to the general practitioner, for whose use the work is evidently chiefly intended. Although we have criticized the loose physiological and chemical statements in Dr. Yeo's work (some of which have been quoted), yet the book will no doubt be found useful by many.

Fifth and Sixth Annual Reports of the Bureau of Ethnology to the Secretary of the Smithsonian Institution. By J. W. Powell, Director. (Washington: Government Printing Office, 1887–88.)

THESE Reports, each of which is presented in a large, well-printed volume, contain the record of much solid and useful work. The first of them—the Report for 1883–84—includes an elaborate paper, by Prof. Cyrus Thomas, on burial-mounds of the northern sections of the United States. This is followed by an essay in which Mr. Charles C. Royce tells the story of the official relations of the Cherokee nation of Indians with the Colonial and Federal Governments of North America. In the third paper, Dr. W. Matthews gives an account of what Prof. Powell describes as one of the most illustrative ceremonies of the Navajo, a tribe formerly widely diffused, and now settled in parts of New Mexico and Arizona. Dr. Clay MacCauley deals with the Seminole Indians of Florida, and Mrs. Tilly E. Stevenson gives a vivid picture of the religious life of the Zuñi child. Of the papers associated with the Report for 1884–85, the first is on the ancient art of the province of Chiriqui, Colombia, by Mr. William H. Holmes. To this excellent paper we have already called attention. It is followed by another, by the same author, on textile art in its relation to the development of form and ornament. Dr. Franz Boas contributes to the volume an instructive and well-arranged paper, in which he sets forth the results of his observation and study of the central Eskimo. Prof. Cyrus Thomas gives some aids to the study of the Maya codices, and Mr. J. Owen Dorsey brings together interesting versions of two Osage traditions. These versions are printed in the original language, with an interlinear and a free translation of each, and with explanatory remarks.

Light, Heat, and Sound. By Chas. H. Draper, B.A. D.Sc. (Lond.). (London: Blackie and Son, 1890.)

THE syllabus of contents of this little work is that of the elementary stage of the Science and Art Department, some additions being made in the sections on light and Heat in order to bring them up to the standard of the London University matriculation paper. Viewed as an examinational text-book, there is much that is meritorious in the arrangement and general character of the work, the information being conveyed in the disintegrated fashion now so common. We would, however, point out to Dr. Draper that hoar-frost is not frozen dew, but water deposited in the solid form, and that hail is not simply rain-drops frozen as they fall through a cold stratum of air. The questions placed as exercises at the end of the chapters have been selected from papers set at the above examinations, and will serve not only as a test of the student's progress, but as a branch of his mental education worth cultivating.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Bourdon Gauge.

ALLOW me to suggest to such of your readers as are interested in this subject the following experiment. Cut out of cardboard two annular strips, each of somewhat more than a quadrant, the inner radius being say 7 inches, and the outer radius 9 inches. Along the middle of each strip—that is, along the circle of 8 inches radius—cut the boards half through, so as to render them flexible, and then join the two strips together with gum paper at the inner and outer edges. In this way we obtain a curved tube whose section is a rhombus, and whose curvature is connected with the magnitudes of the angle of the rhombus. The manipulation of such a tube gives definiteness to one's ideas, and enables one to recognize that internal pressure, tending to augment the included volume, and therefore to make the section square, must also cause the curvature of the axis to approach a definite associated value. In this case the deformations are practically by bending, principally, indeed, at the hinges; and I cannot doubt that in its main features the mechanism of an ordinary Bourdon gauge may be looked at in the same light.

RAYLEIGH.

The Optics of the Lightning Flash.

IN the extract from Mr. Shelford Bidwell's recent lecture on "Lightning" at the London Institution, which appeared in your issue of June 12 (p. 151), I notice the author says that the lightning flash of artists has no existence in nature, and that it is an artistic fiction or symbol. May I venture to trespass on your valuable space to refer to a paper which I had the honour of reading before the Royal Meteorological Society (published in the current Quarterly Journal of the Society) only a few days after the delivery of Mr. Shelford Bidwell's lecture? In this paper I endeavoured to show how the "zigzag" flash so often seen by observers, and frequently depicted by artists, may have its counterpart in nature, quite consistently with the evidence of the photographs of lightning flashes collected by the Royal Meteorological Society.

I suggested that such an appearance is not the flash itself, but the optically projected image of the flash formed on clouds, not of a smooth surface, but of the rocky cumulus type. The image of the flash takes the angles of the uneven surface and becomes zigzagged. I showed how this might be by casting the photograph of a lightning flash—the "streaming" flash—by means of the optical lantern, on model cumulus clouds, made of cotton wool. The "streaming" flash became distorted, and in fact zigzagged, so that it could not have been recognized as the type mentioned.

"Projection" lightning flashes surely must happen in nature, and might be accounted for in more ways than one. I will

mention now one simple way which I illustrated by experiment at the meeting referred to. It is fairly well recognized that sheet lightning is the reflection of a flash on a cloud, for example; but if there happens to be the presence of a cloud with a small opening in it somewhere between the actual flash and the distant surface of clouds, then, instead of "sheet" lightning appearing on the latter, there will be "projection" lightning—that is, the image of the flash, whose shape will depend upon the shape of the cloud on which it is cast.

In speaking of zigzag representations of lightning flashes, it is important to make some distinction between the artistic zigzag and a common pictorial type such as is seen on the covers of electrical books, in dissolving views, in scenic effects, and even in street advertisements. It is hardly fair to saddle the artists with the latter class. A good specimen of an artistic zigzag flash, and one which shows an observance of nature, can be seen in Wilson's famous picture of "Celadon and Amelia."

It certainly seems at first sight strange that the "projection" flash should not be included in the photographs of lightning flashes. Its non-appearance may be due (1) to the photographic plates not being sufficiently sensitive to register a flash of diminished brilliancy, for the projected image of any source of light has not the same intensity as the source itself. (2) The "projection" flash being of rarer occurrence, the number of photographs yet taken may not have included it. If the type is rarer, it may be objected that it is not likely that artists would generally depict a rare type in preference to the more common one; but the less dazzling nature of the "projection" would be sufficient to account for its adoption, rendering the form of the flash more distinct to the average eye. To take an illustration, if an electric arc light is suddenly flashed before our eyes, we fail to distinguish the form of the white-hot carbon points, but if its image were flashed upon a screen, their form would be distinctly visible.

It is worthy of note that some painters have chosen to represent other types than what I have termed the "projection" flash. See Turner's "Stonehenge," where "streaming" lightning is pictured. Eric Stuart Bruce.

10 Observatory Avenue, Kensington, W., June 16.

The Bagshot Beds of Essex.

In the second part of the paper on the Westleton beds, by Prof. Prestwich, recently published in the *Quarterly Journal of the Geological Society* (vol. xlvi. p. 152), a section of the Brentwood railway cutting is given, which is, if possible, of more interest from the Eocene beds described than from its bearing on the questions dealt with in the paper.

Reading the new section together with what we already know, we get the following succession of beds at Brentwood:—

(1) *Pebble beds*, capping the plateau up to 15 feet thick.

(2) *Bagshot beds*, about 50 feet, consisting of—

(*a*) Yellow or white sands (bed 6 of Mr. Whitaker's section, "Geology of London," i. 274).

(*b*) The green sands and clays *with fossils* of the railway cutting.

(*c*) Yellow sand with seams of clay of the railway cutting.

(3) *London clay*, about 435 feet, the upper part consisting of dark grey clay, with one or more beds of loam and yellow sand, the so-called "passage beds" exposed in the brick-fields near the station.

The fossils which Mr. Herries and I found near Frierning (Whitaker, "Geology of London," i. 276) came from white sand probably answering to bed 2*a*.

This section seems to show pretty clearly that the Bagshot beds of Brentwood are more nearly allied to the marine Bracklesham (Middle Bagshot) series than to the Lower Bagshots of the Bagshot Heath district, which are probably freshwater. If this be so, the masses of pebbles which overlie them may well be the remains of the pebble beds which so often mark the base of the Upper Bagshot (Barton) beds, and the parallel drawn by Mr. Herries and myself between the pebbles which cap the Warley and Brentwood plateau in Essex and those which cap Hook Heath and other hills in the Bagshot district becomes the more marked (Proc. Geol. Assoc., vol. xi. pp. 13, 16, 20).

1 Hare Court, Temple. Horace W. Monckton.

Electro-Magnetic Repulsion.

Those who have not the means of showing the striking effects produced by Prof. Elihu Thomson may be glad to know a simple illustration of the same principles.

A top consists of a soft iron disk with a brass axis put through it. A small magnet is held over the edge whilst spinning; each elementary sector as it moves up to and away from the poles of the magnet has currents induced which are repelled by the magnet; as the rotation dies out, the currents at a certain point become too feeble to overcome the attraction of the soft iron by the magnet. I bought the top two or three years back of M. Manet, 49 Rue Lourmel, Paris. W. B. Croft.

Winchester College, June 21.

A Remarkable Appearance in the Sky.

There was an appearance in the sky last night, so remarkable that I am tempted to describe it, in case, our situation being high, it should have been better seen here than elsewhere. Along the horizon, from north to about north-east, a faint bank of cloud extended, above which was a space of light like that of the early dawn or of the rising moon. There was no quivering, or shooting upwards of rays, as in the ordinary northern lights; the light was steady, white tending to yellow, brighter at the lower part. Above it hung a purplish haze, through which the stars shone brightly, and occasional strips of dark cloud. It did not happen to be observed till 10.30 p.m., and it was hardly altered at 1.30 a.m., when it was still bright enough to mark the window-frame through a white blind, like moonlight. Besides the position, the fact of a solar eclipse occurring that day proved the moon to have nothing to do with it.

Sussex, June 18. M. E.

PROBLEMS IN THE PHYSICS OF AN ELECTRIC LAMP.[1]

I.

More than eighty years ago Sir Humphry Davy provided the terminal wires of his great battery of 2000 pairs of plates with rods of carbon, and, bringing their extremities in contact, obtained for the first time a brilliant display of the electric arc.[2] The years that have fled away since that time have seen all the marvellous developments of electro-magnetic engineering, have placed in our possession the electric glow-lamp, and brought the art of electrical illumination to a condition in which it progresses each year with giant strides. In addition to the importance attaching to their ever-increasing industrial use, there are many questions of purely scientific interest which present themselves to our minds when we proceed to examine the actions that take place when a carbon conductor is rendered incandescent in a high vacuum, or when an electric arc is formed between two carbon poles. It is to a very few of these physical problems that I desire to direct your attention to-night, but more especially to one which is particularly interesting from the bearing which it has on the general nature of electric discharge.

We know as a very familiar fact that if we attempt to raise the temperature of a carbon conductor inclosed in a vacuum beyond a certain limit, not far removed from the melting-point of platinum, the carbon begins to volatilize with great rapidity. If an electric glow-lamp has passed through its carbon more than a certain strength of current, the glass bulb speedily becomes darkened by a deposit of this volatilized carbon condensed upon it; and experience shows us that we cannot raise the temperature of that carbon beyond a definite point without causing this waste of the conductor to become very rapid. In the highly rarefied atmosphere within the bulb of a glow-lamp, the carbon, when at its normal incandescence, must be con-

[1] Friday Evening Discourse delivered at the Royal Institution by Prof. J. A. Fleming, M.A., D.Sc., on February 14, 1890.

[2] Sir Humphry Davy laid a request before the managers of the Royal Institution on July 11, 1808, that they would set on foot a subscription for the purchase of a large galvanic battery. The result of this suggestion was that a galvanic battery of 2000 pairs of copper and zinc plates was set up in the Royal Institution, and one of the earliest experiments performed with it was the production of the electric arc between carbon poles, on a large scale. It is probable, however, that Davy had produced the light on a small scale some six years before, and, according to Quetelet, Curtet observed the arc between carbon points in 1802. See Dr. Paris's "Life of Sir H. Davy."

sidered to be projecting off molecules of carbon in all directions, partly in virtue of purely thermal actions, but probably also in consequence of certain electrical effects to be presently discussed. This scattering of the material of the carbon conductor takes place with disadvantageous rapidity from an industrial point of view at and beyond a certain temperature,[1] but it exists as well at much lower temperatures than that which is found to determine the practical limit of durability. A curious appearance is found in many incandescent lamps which have been "over-run," which shows us that this projection of carbon molecules from the hot conductor is not, perhaps, best described by calling it a vaporization of its substance, but that the surface molecules are shot off in straight lines, and that they reach the glass envelope without being hindered to any great extent by the molecules of the residual air.

If an electric current is passed through an otherwise uniform carbon conductor, which possesses at any one place a specific resistance higher than that of the remaining portion, the current, in accordance with a well-known law, there develops a higher temperature, and the molecular scattering at that spot may in consequence be greatly exaggerated. It may be that the detrition of the conductor at that locality will be so great as to cut it through after a very short time. When the carbon has the form of a simple horseshoe loop, and when this mole-

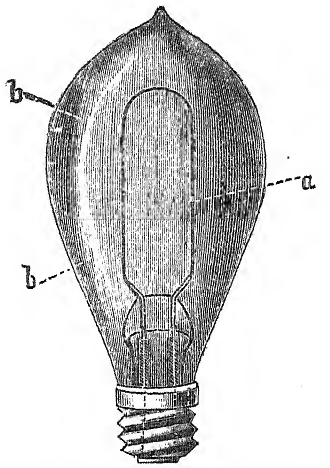


FIG. 1.—Glow-lamp, having the glass bulb blackened by deposit of carbon, showing the molecular scattering which has taken place from the point *a* on the filament, and the shadow or line of no deposit produced at *b*.

cular scattering takes place from some point in the middle of one branch, the molecular projection makes itself evident by producing a "molecular shadow" of the other leg upon the interior of the glass. I will project upon the screen an image of the carbon horse-shoe loop taken from an old glow-lamp, and you will be able to see that the filament has been cut through at one place. At that position some minute congenital defect caused the carbon to have a higher resistance, the temperature at that point when it was in use became excessive, and an intensified molecular scattering took place from that locality. On examining the glass bulb from which it was taken, we find that the glass has been everywhere darkened by a deposit of the scattered carbon except along one narrow line (see Fig. 1), and that line is in the plane of the carbon loop and on the side opposite to the point of rupture of the filament.[2]

[1] When the rate of expenditure of energy in the carbon conductor is raised until it reaches a value of about 500 watts, or 360 foot-pounds per second per square inch of radiative surface, a limit of useful temperature has been reached for economical working, under the usual present conditions of steam-engine-driven dynamos and modern glow-lamps.

[2] The writer desires to express his indebtedness to the editor of the *Electrician* for the loan of the blocks illustrating this abstract.

I may illustrate to you, by a very simple experiment, the way in which that "shadow" has been formed. Here is a ∩-shaped rod: this shall represent the carbon conductor in the lamp; this sheet of cardboard placed behind it, the side of the glass receiver. I have affixed a little spray-producer to one side of the loop, and from that point blow out a spray of inky water. Consider the ink spray to represent the carbon atoms shot off from the overheated spot. We see that the cardboard is bespattered on all points except along one line where it is sheltered by the opposite side of the loop. We have thus produced a "spray shadow" on the board (Fig. 2). The existence of these molecular shadows in incandescent lamps leads us therefore to recognize that the carbon atoms must be shot off in straight lines, or else obviously no such sharp shadow could thus be formed. This phenomenon confirms in a very beautiful manner the deductions of the kinetic theory of gases. I may remind you that at the ordinary temperature and pressure the mean free path of a molecule of air is deduced to be about four one-millionths of an inch. This is the average distance which such a gaseous molecule moves over before meeting with a collision against a neighbour which changes the direction of its path. Let the air be rarefied, as in these bulbs, to something like a millionth of the ordinary atmospheric pressure, and the mean free path is increased to several inches. The space within the bulb—though from one point of view densely populated with molecules of residual air—is yet, as a fact, in such a condition of rarefaction that a carbon molecule projected from the conductor can move over a distance of three or four inches on an average

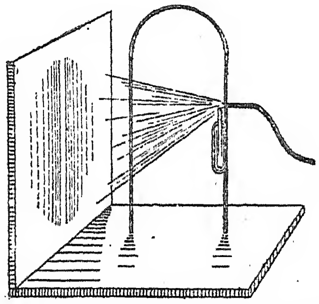


FIG. 2.—"Spray shadow" of a rod thrown on cardboard screen to illustrate formation of "molecular shadow" in glow-lamps.

without meeting with interference by collision with another molecule, and the facts revealed to us by these shadows show that this must be the case. I have also at hand some Edison lamps in which these "molecular shadows" are finely shown, but in these cases the deposit on the interior of the bulb is not carbon but copper, because the molecular scattering has here taken place by excessive temperature developed at the copper clamps by which the carbon filament is attached to the platinum wires. The theory, however, is the same. The deposit of copper shows a fine green colour by transmitted light in the thinner portions. One curious lamp also before me had by an accident an aluminium plate volatilized within the bulb. The glass receiver has in consequence been covered with a mirror-like deposit of aluminium, which on the thinner portions shows a fine blue colour by transmitted light, and a silvery lustre by reflected light. This lamp also shows a fine "molecular shadow."

These facts prepare us to accept the view that when a glow-lamp is in operation the highly rarefied residual air in the interior of the bulb is being traversed in all directions by multitudinous carbon atoms projected off from the incandescent carbon conductor. I now wish to pass in review before you some facts which indicate that these carbon atoms carry with them electric charges, and that they are charged, if at all, with *negative electricity*.

I may preface all by saying that much of what I have to show you will be seen to be closely related to the phenomena studied by Mr. Crookes in his splendid and classical researches on radiant matter. Our starting-point for this purpose is a discovery made by Mr. Edison in 1884, and which received careful examination at the hands of Mr. Preece in the following year,[1] and by myself more recently. Here is the initial experiment. A glow-lamp having the usual horseshoe-shaped carbon (see Fig. 3) has a metal plate held on a platinum wire sealed through the glass bulb. This plate is so fixed that

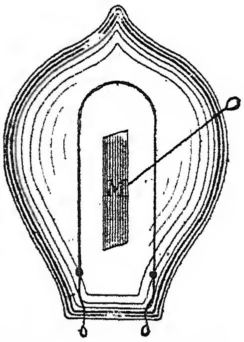


FIG. 3.—Glow-lamp having insulated metal middle plate M sealed into bulb to exhibit "Edison effect."

it stands up between the two sides of the carbon arch without touching either of them. We shall illuminate the lamp by a continuous current of electricity, and for brevity's sake speak of that half of the loop of carbon on the side by which the current enters it as the positive leg, and the other half of the loop as the negative leg. The diagram in Fig. 4 shows the position of the plate with respect to the carbon loop. There is a distance of half an inch, or in some cases many inches, between either leg of the carbon and this middle plate. Setting the lamp in action, I connect a sensitive galvanometer between the

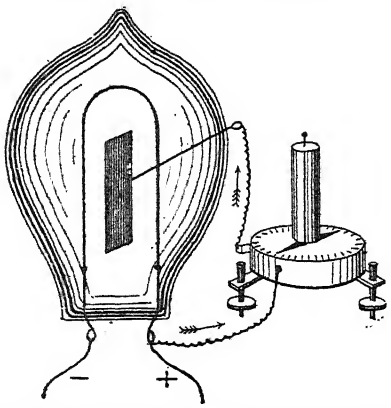


FIG. 4.—Sensitive galvanometer connected between the middle plate and positive electrode of a glow-lamp, showing current flowing through it when the lamp is in action ("Edison effect").

middle plate and the *negative terminal* of the lamp, and you see that there is no current passing through the instrument. If, however, I connect the terminals of my galvanometer to the middle plate and to the *positive electrode* of the lamp, we find a current of some milli-amperes is passing through it. The diagrams in Fig. 5 show the mode of connection of the galvanometer in the two cases. This effect, which is often spoken of as the "Edison effect," clearly indicates that an insulated plate so placed in the vacuum of a lamp in action is brought down to the same potential or electrical state as the negative electrode of the carbon loop. On examining the direction of the current through the galvanometer we find that it is equivalent to a flow of negative electricity taking place through it *from* the middle plate *to* the positive electrode of the lamp. A consideration of this fact shows us that there must be some way by which negative electricity gets across the vacuous space from the negative leg of the carbon to the metal plate, whilst at the same time a negative charge cannot pass from the metal plate across to the positive leg. Before I pass away from this

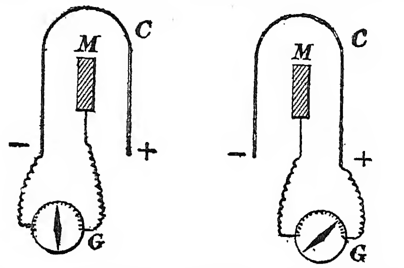


FIG. 5.—Mode of connection of galvanometer G to middle plate M and carbon horseshoe-shaped conductor C in the experiment of the "Edison effect."

initial experiment, I should like to call your attention to a curious effect at the moment when the lamp is extinguished. Connecting the galvanometer as at first, between the middle plate and the negative electrode of the lamp, we notice that though made highly sensitive the galvanometer indicates no current flowing through it whilst the lamp is in action. Switching off the current from the lamp produces, as you see, a violent kick or deflection of the galvanometer, indicating a sudden rush of current through it.

In endeavouring to ascertain further facts about this effect one of the experiments which early suggested itself was one directed to determine the relative effects of

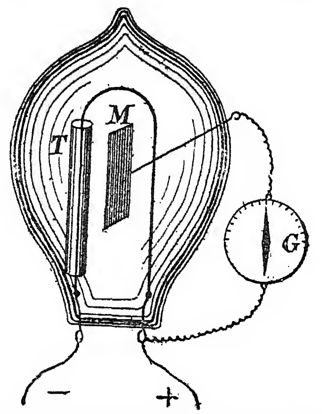


FIG. 6.—Glow-lamp having negative leg of carbon inclosed in glass tube T, the "Edison effect" thereby being annulled or greatly diminished.

different portions of the carbon conductor. Here is a lamp (see Fig. 6) in which one leg of the carbon horseshoe has been inclosed in a glass tube of the size of a quill, which shuts in one-half of the carbon. The bulb contains, as before, an insulated middle plate. If we pass the actuating current through this lamp in such a direction that the covered or sheathed leg is the *positive* leg, we find the effect existing as before. A galvanometer connected between the plate and positive terminal of the lamp yields a strong current, whilst if connected between the negative terminal and the middle plate there is no current at all. Let us, however, reverse the current

[1] Mr. Preece's interesting paper on this subject is published in the "Proceedings" of the Royal Society for 1885, p. 219. See also the *Electrician*, April 4, 1885, p. 436.

through the lamp so that the shielded or inclosed leg is now the negative one, and the galvanometer is able to detect no current, whether connected in one way or the other. We establish, therefore, the conclusion that it is the negative leg of the carbon loop which is the active agent in the production of this "Edison effect," and that if it is inclosed in a tube of either glass or metal, no current is found flowing in a galvanometer connected between the positive terminal of the lamp and this middle collecting plate.

Another experiment which confirms this view is as follows:—This lamp (see Fig. 7) has a middle plate, which is provided with a little mica flap or shutter on one

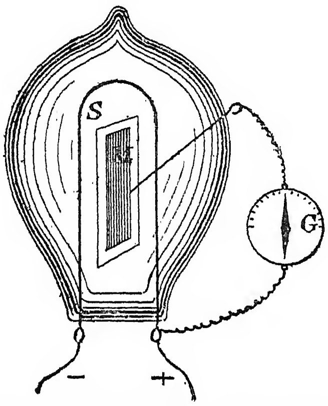


FIG. 7.—Glow-lamp having mica shield S interposable between middle plate M and negative leg of carbon, thereby diminishing the "Edison effect."

side of it. When the lamp is held upright the mica shield falls over and covers one side of the plate, but when it is held in a horizontal position the mica shield falls away from the front of the plate and exposes it. Using this lamp as before, we find that when the positive leg of the carbon loop is opposite to the shielded face of the plate, we get the "Edison effect" as before in any position of the lamp. Reversing the lamp current and making the same leg the *negative* one, we find that when the lamp is so held the metal plate is shielded by the interposition of the mica, and the galvanometer current is very much less than when the shield is shaken on one side and the plate exposed fully to the negative leg.

(*To be continued.*)

SOME EXPERIMENTS ON FEEDING FISHES WITH NUDIBRANCHS.

WITH the view of testing the theory that the remarkable shapes and colours of Nudibranchs are either of a protective or of a warning nature,[1] and are definitely related to the edibility or the reverse of the animals, I have been offering lately various kinds of Nudibranchs to the fishes in the aquarium of the Liverpool Free Public Museum,[2] and have carefully noted the result of each trial.

Although these experiments will have to be repeated, and additional evidence accumulated, still it may be interesting to other biologists working on similar lines to have this account of the inquiry, in its present stage, laid before them, and I need scarcely say that I would be glad of any suggestions which would be useful in future investigations.

Most of the experiments were made in three large fish-tanks, which may be called A, B, and C. A and B are rectangular slate and plate-glass wall-tanks, lit from the top, measuring $7\frac{1}{2}$ feet long, $5\frac{1}{4}$ feet wide, and $3\frac{1}{4}$ feet high. A has a gravel bottom, and contains about 20 very healthy and active shannies (*Blennius pholis*) obtained from the Menai Straits; while B has a sandy floor, and is devoted to flat-fish. It contains a considerable number of soles and plaice, a few small thornback rays, turbot, and brill, and on one of the occasions had some young cod. The average size of these flat-fish is 6 inches in length, and there are over 60 of them in the tank. Both A and B have some rock-work. C is an octagonal centre-tank with a sandy bottom, measuring $4\frac{1}{2}$ feet in diameter, and 17 inches in depth. It contains various small fishes, viz. bullhead, goldsinny, pogge, gemmeous dragonet, five bearded rockling, viper-weever, and young cod.

All these fishes are apparently in a healthy condition, and some of them have been living undisturbed in their tanks for periods varying up to four years. They are usually fed upon mussels, cockles, and occasionally worms, which are thrown in at the top of the tank and allowed to sink through the water. Such food-matters are usually seen at once, and eagerly pounced upon and eaten during their descent. I adopted the same plan in putting most of the Nudibranchs into the tanks, and as the fishes were not fed on the days I intended to experiment with them, and had usually been fasting for 24 hours when I began, they may be regarded as being unusually eager to seize any object dropped into the water. At the beginning and again at the end of each day's experiments, we threw a couple of cockles or mussels into the tanks, and found that they were at once caught and bolted in the usual manner.

I. *October* 29, 1889. [A supply of *Doris*[1] *bilamellata* was obtained from the rocks at New Brighton.]

TANK A.—*Doris.*

(1) Seized, when falling, by a shanny, and taken at once to dark corner.

(2) Seized and at once rejected, seized by another shanny and at once rejected, seized by a third and rejected, then allowed to lie on bottom of tank.

(3) Seized and rejected by two fish in rapid succession, then seized by third and taken to dark corner.

(4) Seized and rejected by first fish, taken to dark corner by second.

(5) Seized and rejected by three fish in rapid succession, and then left.

TANK B.—*Doris.*

(1) Seized and rejected in rapid succession by a turbot, a sole, another sole, and a plaice, and then left lying on the sand.

TANK C.—*Doris.*

(1) Seized and rejected by a goldsinny, tried again by same and again rejected, then left.

(2) Seized and rejected by a bullhead and by a dragonet in rapid succession, and then left.

Finally, another *Doris* was dropped gently into a fourth tank containing a conger eel, so as to fall in front of its nose; but although the fish passed close to the Nudibranch several times while under observation, it apparently took no notice of it.

From these nine experiments it seems probable that *Doris bilamellata* is distasteful to at least most of these eight kinds of fishes tried. This was an unexpected result, as *Doris* has no stinging apparatus, and certainly seems to be protectively coloured. The distastefulness may be due to the spicules in the skin or to the abundant mucus covering the body.

[1] *Quart. Journ. Microsc. Sci.*, vol. xxxi. p. 41.
[2] With the kind permission and assistance of Mr. T. J. Moore, the curator, and his assistants, who were present at all the experiments.

[1] I use throughout this article the old well-known generic names *Doris* and *Eolis*, instead of the modern genera, only known to specialists, in which the species I am dealing with have been placed. No possible confusion can arise from doing so.

II. *February* 21, 1890. [Large supply of *Ancula cristata*, and a few *Dendronotus arborescens*, *Eolis rufibranchialis*, and *Eolis picta* from Hilbre Island.]

Mr. Moore and I each ate an *Ancula*. The specimen was placed alive upon the tongue. No stinging or other disagreeable sensation was perceived. It was then chewed slowly and swallowed. The taste was pleasant, and distinctly like that of an oyster.

TANK A.—*Ancula*.

(1) Seized and rejected by a shanny, and then bolted suddenly by a second.

(2) Seized and rejected by ten fish in rapid succession.

(3) Seized, when falling, and swallowed by a fish.

(4) Seized and rapidly rejected by five fish in succession.

(5) Seized and rapidly rejected by four fish in succession.

TANK B.—*Ancula*.

(1) Seized and rejected by a young cod and six plaice in rapid succession.

(2) Seized and rejected by seven plaice, and left lying on sand.

(3) Seized and rejected by four plaice, and left lying on sand.

The fish were then tried with some cockles, which, when thrown in, were eagerly pounced upon and eaten.

(4) Then four specimens of *Ancula* were dropped in together, and were tried and rejected by two young cod and three plaice.

TANK C.—*Ancula*.

(1) Touched by a young cod, but not taken, then tried and rejected by goldsinny.

(2) Touched and rejected several times by young cod.

(3) Touched and rejected by first cod, bolted suddenly by second.

The shannies at once take an object into the mouth, even though they reject it again immediately, but the young cod usually approach it very closely, and appear to smell it or feel it with the lips, and then turn away from it, or else suddenly bolt it, in which case it does not reappear. The shanny seems to test the edibility inside its mouth, the cod outside.

Some crabs (*Hyas araneus*) in two small tanks were then tried with specimens of *Ancula* with the following results :—

(1) Seized at once by crab, but eaten very slowly, and only partially.

(2) Taken no notice of.

(3) Taken up with chela, then dropped and left.

(4) Apparently not noticed by crabs.

The three last specimens of *Ancula* were found alive and fully expanded next day, and crawled about the two crab tanks undisturbed for some time afterwards.

Finally, a few specimens of *Ancula* were offered to two large anemones (*Actinoloba*), but were not taken.

In all, then, *Ancula* was rejected by 53 animals and taken by four. These experiments gave us the distinct impression that *Ancula* was distasteful to the animals tried, although we did not at that time understand why, and had expected to get a contrary result.

TANK A.—*Dendronotus*.

(1) Seized at once by shanny, and carried off to back of tank ; shortly afterwards two shannies were found fighting over it, each having hold of an end, as they do with a large worm ; finally, they each ate a part of the *Dendronotus*.

TANK B.—*Dendronotus*.

(2) Tried and rejected by brill and young cod. Then seized by plaice and kept in mouth for a long time (five to ten minutes), during which it was pursued by other fish.

TANK C.—*Dendronotus*.

(3) Touched and left by young cod ; taken partly into mouth and rejected by two bullheads four or five times.

The general impression we received was that *Dendronotus* was more acceptable to the fish than *Ancula*, but that they were incommoded by the size. Our specimens were large ones—over two inches in length—and none of the fishes tried seemed able to get the whole of the *Dendronotus* comfortably into the mouth, at once. Several took half the body into the mouth, and swam about with the other half hanging out. This was well seen in the case of the two shannies, who each ate half of the specimen, and of the plaice which carried about its prey for a considerable time, during which it was actively pursued by the others. That specimen was in all probability eaten by one or more of the plaice, as we could not find any trace of it a short time afterwards. The rejection by the bullheads may be accounted for by the awkward size of the morsel. The two fish had each at least two tries at it, taking it half into the mouth, giving it a shake, sending it out, and then going at it again as if to get a better hold.

TANK A.—*Eolis*.

(1) Seized by largest shanny, who at once shook it vigorously, and kept moving its jaws and ejecting the cerata[1] in groups of three or four, and finally put out the rest of the body. Then tried and rejected by four or five other fish in rapid succession, and then by the large shanny again, then by several others, and finally left lying on the bottom. The large shanny who first tried it was going about for some time afterwards with the mouth held open.

TANK C.—*Eolis*.

(2) Touched or tried, and rejected at once by cod, bullhead, and weever. The cod came very near it, or touched it with its snout, several times afterwards, but never took it into the mouth.

Eolis is undoubtedly distasteful. The cnida (stinging cells) on the tips of the cerata probably sting the lips of the fish. As it had occurred to me that the natural conditions would be more nearly reproduced if the Nudibranchs were not dropped into the tanks, on the following day, February 22, a few specimens of *Ancula* were placed upon pieces of stone and lowered cautiously into tanks A and B in such a way as not to attract the attention of the fish. The Nudibranchs reached the rockwork safely, and were seen crawling over various parts of the tanks for several days untouched by the fish (shannies and flat-fish). Woods, the aquarium attendant, tells me that the fish sometimes went up close to the *Ancula*, and looked at them, but never attempted to touch them. The Nudibranchs were last seen about a week after being put into the tanks. They then disappeared, but may possibly have retreated into the back part of the tank, or have crawled up out of the water, as *Ancula* is very liable to do when kept in captivity.

III. *March* 22, 1890. [*Dendronotus*, *Eolis*, and *Doris* from Hilbre Island.]

TANK A.—*Dendronotus*.

(1) Seized at once by the large shanny and kept in the mouth, half the Nudibranch projecting. This shanny was pursued by others, one of which caught the projecting end of the prey, and in the ensuing struggle tore half the body off and ate it. The large shanny at once retreated with the remainder to the back of the tank, came out shortly afterwards with the *Dendronotus* still in mouth, and was again pursued and retreated to the dark, appearing again soon without the Nudibranch.

[1] The coloured dorsal papillæ which contain the stinging cells.

TANK C.—*Dendronotus.*

(2) Pounced upon at once by three bullheads (*Cottus*), which made rapid dabs at it successively, until one secured it and carried it off to a quiet place, where he seized it in his mouth and ejected it nine times in succession, each time taking it half into the mouth and keeping it there for some seconds, then spitting it out and at once pouncing upon it again. Finally, the now somewhat mangled remainder of the *Dendronotus* was taken out and put into tank A, where one of the shannies at once seized and swallowed it. This *Dendronotus* was large. It was larger than the head of the *Cottus*, and caused the mouth-cavity to bulge out greatly when it was taken in. The general impression was that the *Cottus* found the *Dendronotus* desirable food, but an uncomfortably large mouthful, and was trying to worry it to pieces.

TANK A.—*Eolis.*

(1) Tried, and at once rejected by three shannies in rapid succession, then seized by the large shanny and carried behind some rock-work. Immediately, numerous red cerata were seen scattered through the water in that neighbourhood, showing that the *Eolis* had been forcibly ejected in pieces. The cerata floated about for some time in the water, but were not touched by any of the fish.

(2) Pounced upon by several fish together; one secured and at once rejected it, and then, seizing the white body, managed to bite it across, setting free the dorsal portion with all the cerata. It then retired to the back of the tank, while the cerata—after separating, as they very readily do in Eolids—were left floating about in the water, untouched.

TANK A.—*Doris.*

(1) Tried and rejected by two shannies, then seized by the largest shanny and carried off to the back of the tank.

TANK B.—*Doris.*

(2) Several fish darted at the Nudibranch, but a large sole suddenly slipped up vertically between them and bolted it.

(3) Tried and rejected by six or eight plaice, and finally left on the sand.

(4, 5, 6) These three specimens were gently lowered into the tank by a net, so as to reach a shelf of the rock-work without attracting attention. They soon began to expand and move. One plaice swam up and looked or smelled at them, but did not touch them.

The action of the large sole in bolting *Doris* No. 2 above may possibly be explained as a result of the habit of competing for their food. Three or four other fish were darting at the Nudibranch, and the sole took the only possible course by which it could secure the prey—it made a rapid movement upwards between the snouts of its competitors and swallowed the *Doris* entire; there was evidently no time for examination.[1]

In the above experiments I have used altogether 53 Nudibranchs, offered to twelve different kinds of fish and other predaceous shore animals, and there have been over 130 distinct transactions between the fishes and the Nudibranchs. My general impression is that the order of edibility of the Nudibranchs offered to the fishes is: *Dendronotus*, *Doris*, *Ancula*, *Eolis*; *Eolis* being the most distasteful form, *Ancula* next, *Doris* less so, and *Dendronotus* edible, but, from its size, offering difficulties to the rather small fishes which we tried.

[1] The last number of the Journ. Mar. Biol. Assoc., containing Mr. Bateson's paper on the sense-organs and perceptions of fishes, only reached Liverpool after this article had been sent to NATURE. In regard to the sole being one of those fishes which hunt for their food and recognize it by the sense of smell alone, I would remark that the specimens in the aquarium here certainly seem to perceive their food as the plaice do by sight, the two kinds of fish often darting together at a food morsel—and, as I have just shown above, the sole being sometimes more alert than its competitors. Possibly these soles have changed their habits like the rockling described (p. 238) by Mr. Bateson.

The Nudibranchs were all healthy and good-sized specimens, and the fish were probably the right kind, being nearly all shore fishes found in the immediate neighbourhood of where the Nudibranchs live. Still, the conditions were, of course, to a certain extent artificial, and that must be taken into account in drawing conclusions. Dropping the Nudibranchs into the tank from above is unnatural, and may give rise to a misleading result, especially where the fish are accustomed to have their food thrown in from above, and *only receive edible food.* Then, again, at least some of the fish—those that have been some time in captivity—have been educated to compete with one another for the food. When *anything* is thrown in—a bit of white shell will do—there is at once a rush made upon the falling object, and no time is allowed for inspection or consideration. I would account for the seizing of *Eolis* by the shannies (very active, voracious, and apparently impulsive fishes), even when the prey is evidently distasteful and has brilliant warning colours, as a result of this acquired habit of competition, and of pouncing upon anything thrown into the tank. Still, there is a marked difference between the manner in which they take a cockle and say an *Ancula.* The cockle is taken right in and swallowed at once, while the distasteful Nudibranch, even if seized, is usually only partly taken into the mouth; in some cases, it is seen to be held by the very front of the jaws, and is then ejected with force.

Ancula has been a particularly interesting case. Start-with the general opinion that *Ancula* is a perfectly defenceless soft-bodied animal, I have been astonished to find that it is sometimes present on the rocks at Hilbre Island in great abundance, in very prominent and exposed situations, and that its colouring was not protective, but rendered it conspicuous. The experiments at the aquarium next showed me that this Nudibranch is distasteful to fishes and other shore animals, but for a time I did not understand why. Lately, however, Mr. Clubb and I have found[1] that, besides the abundant mucous glands scattered over the integument, *Ancula* possesses special large glands occupying the apices of the cerata, and opening to the exterior. These glands are placed just where an offensive organ would be most useful, and where the stinging cells are found in *Eolis*, and it seems probable enough that it is the presence of their secretion on the most outstanding parts of the body which renders the animal objectionable to fishes.

The protective colouring of *Doris bilamellata* may be accounted for in one or both of two ways: (*a*) it may serve to protect the animal from certain other shore animals which have not yet been experimented with, and to which the spicules and mucus of the *Doris* are not objectionable; and (*b*) it may save the animal from being tried by fishes, &c., not sufficiently aware of its (to them) distasteful nature.[2] It is obvious that, if an animal is not *thoroughly* objectionable, and has not yet become conspicuous with warning colours, it will be better for it to be protectively coloured. So we need not be surprised to find that some inconspicuous protectively coloured animals have certain offensive organs, and are distasteful to certain of their enemies. *Eolis* is a most distasteful form, and has conspicuous colours of a warning nature. *Ancula* is also distasteful, and is conspicuously coloured. *Doris* is less distasteful, and is still protectively coloured; while *Dendronotus*, which is, I believe, edible, is very effectually concealed, amongst the red seaweeds it lives on, by its large branched cerata and red-brown colours.

W. A. HERDMAN.

[1] See "Third Report on the Nudibranchiata of Liverpool Bay" (Trans. Biol. Soc. Liverpool, vol. iv.).

[2] What seems to be a very similar case has been pointed out by Mr. Garstang (Journ. Mar. Biol. Assoc., October 1889, p. 191), viz. the two British species of *Hermæa*, which are both protectively coloured, and have no stinging cells, and yet seem to possess the power of emitting, when irritated, an offensive fluid. I would expect to find that they were distasteful to at least some fishes.

THE PULKOVA REFRACTOR.

ON the completion of the Pulkova Observatory, the jubilee of which has recently been celebrated, the late W. Struve published his "Description de l'Observatoire," which made the scientific world acquainted with the complete equipment of that institution. The additions which have been recently made to the Observatory, in order to preserve its high character and deserved reputation, have induced the authorities to publish what may be regarded as a supplement to that work, and the details now given of the history of the erection, and the results of a systematic examination, of the new refractor are not less interesting than were those of the old 15-inch.

The optical work of this recent addition, as is well known, is the work of Messrs. Alvan Clark, and the parallactic mounting that of Messrs. Repsold, and both of these eminent firms appear to have given, in their respective departments, complete satisfaction to the Russian authorities. Considerable difficulty was experienced in procuring the necessary disks for the object-glass, but eventually M. Feil, of Paris, supplied both flint and crown. The former appears to have given perfect satisfaction, but in the latter, near to the centre of the disk, there is collected, about a quarter of an inch below the surface, a quantity of small air-bubbles, which cover a space one and a half inch long by one-eighth broad. As in the opinion of the opticians, as well as of Prof. Pickering, this defect would not introduce any inconvenience, it was determined to proceed with the manufacture, rather than to wait for a more satisfactory casting. This defective spot, of elliptical shape, has no bad effect on the images of stars in the general use of the telescope, but bright objects, such as α Lyræ, are accompanied by two streams of false light, some minutes in length, in opposite directions, which appear to be produced by this defect in the crown lens. The position angle of these rays is found to be 114°–294°, and this direction is almost exactly perpendicular to the major axis of the air-bubble, which has been measured 23°–203°. Moreover, as this peculiarity is the more noticeable when the diameter of the object-glass is diminished by diaphragms, there can be no doubt that it is the result of diffraction produced by this spot.

The mounting of the object-glass in its cell differs in two respects from the plan generally adopted. The internal surfaces of the two lenses are separated by about six inches. Though this separation does not render the telescope available for photography, it doubtless tends to improve the achromatism; and, further, since openings are left in the cell for the purpose of cleaning the inner surfaces, currents of air can pass between the lenses and promote an equality of temperature between them and the atmosphere outside. The two lenses are not rigidly mounted in their cell of cast-iron, but, to prevent any risk of pinching or strain that might arise from the unequal expansion of metal and glass, a space of 0·5 mm is left. It was conjectured that a displacement of the lenses, relatively to each other, through this small amount would exercise no bad effect on the quality of the images, and this anticipation has been found correct.

The constants of the object-glass are as follows:—

	m.	
Radii of the crown-glass lens ...	− 5·1054	
	+ 5·2831	(computed).
	mm.	
Thickness of the crown	42·42	
Thickness of the flint	26·06	
	m.	
Radii of the flint...	+ 4·8386	
	− 140·130	

The focal lengths computed from these data, one of which, however, has been inferred, give the following results:—

				m.
Red,	λ = 636	...	Focal length	13·892
Yellow,	λ = 589	...	,, ,,	13·885
Green,	λ = 535	...	,, ,,	13·884
Blue,	λ = 481	...	,, ,,	13·892

from which it will be seen that the achromatism is satisfactory for the brighter parts of the visible spectrum, and in fact accords with that part of the spectrum which was originally selected for the minimum focal length, viz. λ = 0·00057.

The relative position at the focus for rays of different refrangibility was more accurately determined by the method of Prof. Vogel with the aid of a small spectroscope, as well as with the great spectroscope attached to the instrument. It was then seen that the part of the spectrum between D and b was so nearly linear that no certain determination of the difference of lengths for the different colours could be effected. For more distant parts of the spectrum the following measures were made of the distances of the three hydrogen lines from the normal position D − b:—

	mm.		
C df =	3·0	...	$\frac{df}{f}$ = 0·00021
F =	6·4	...	= 0·00045
H_γ =	32·9	...	= 0·00233

It is not uninteresting to compare this result with that which Prof. Vogel obtained from measurements on the Vienna refractor of 26 inches, where the general character of the achromatism is very similar to that of the Pulkova refractor, since in both the red images are joined between D and F, and beyond F a rapid increase in the secondary spectrum is exhibited—a defect common to all objectives of silica glass.

In the Vienna object-glass the distances of the focus of the three rays before mentioned from the focal plane D − b are—

	mm.
C	2·7
F	6·0
H_γ	23·5

Consequently the diameters of the circle of chromatic aberration, reckoned on the same plane, are, for the two telescopes, as follows:—

Pulkova.			Vienna.		
	mm.			mm.	
Aperture	762		Aperture	675	
Focal length	14,120	″	Focal length	10,360	″
Diameter C	0·162 or	2·37	Diameter C	0·176 or	3·51
,, F	0·345 ,,	5·05	,, F	0·391 ,,	7·81
,, H_γ	1·775 ,,	25·95	,, H_γ	1·831 ,,	30·48

The advantages of a proportionately greater focal length in the case of the Pulkova instrument are shown by the somewhat smaller values of the angular diameter. This want of perfect achromatism makes itself felt in the Pulkova instrument in the images of stars remote from the optical axis. For a circle about 16′ in diameter, no appreciable effect is noticeable, but outside this radius the image has a tendency to exhibit a red fringe on the side turned towards the optical axis, and a violet on the side more remote.

The parallactic mounting appears to possess and retain a very satisfactory stability. In the case, however, of exceptionally heavy object-glasses, it is of interest to rigidly investigate the flexure of the tube. The total weight of the object-glass and cell is in this case 400 lbs. approximately, and considering the great distance from the centre of the instrument at which it is supported, the coefficient of flexure might be expected to be large. As a

matter of fact, this constant when derived from the observed zenith distances of known stars is 40″, but this amount, of course, refers only to the difference of flexure at the eye and object-glass ends. Direct measurements have, however, been made of the deflection of either end. For this purpose a small telescope was attached to the cradle of the instrument, with which a scale placed at either end could be read, the instrument being in both a vertical and horizontal position. The result was that the object-glass dropped 5·48 mm., and the eye end 3·22 mm.; when all necessary corrections have been made, this gives a flexure of 34″, a satisfactory agreement with that obtained from observations of stars. This deflection from the straight line was observed at eight different angles with reference to the horizon, and the results are fairly represented by supposing the flexure to vary simply as the sine of the zenith distance.

As regards the light-collecting capacity, it may be mentioned that the satellite of Neptune can be observed in an illuminated field without difficulty, and that the satellites of Mars were observed on fifteen evenings in 1886, a year in which the opposition fell very unfavourably for their observation. Hyperion is visible on a feebly illuminated red field, while Enceladus and Mimas are visible till quite close to the planet's disk. That there are difficulties in the employment of such large telescopes goes without saying: it is, however, satisfactory to notice that the number of evenings on which the telescope cannot be used from bad definition or adverse meteorological conditions is not larger than in the case of the 15-inch equatorial. W. E. P.

SIR WARINGTON W. SMYTH, F.R.S.

Mining has suffered an irreparable loss by the death of Sir Warington Smyth, which occurred suddenly at his house in Inverness Terrace on the 19th inst. He was the eldest son of Admiral W. H. Smyth, F.R.S., and was born at Naples 73 years ago. He was educated at Westminster and Bedford Schools and at Trinity College, Cambridge, where he exhibited great skill as an oarsman, being one of the winning University crew on the Thames in 1839. In that year he graduated, and obtained a travelling fellowship which enabled him to devote more than four years to a journey through the chief mining districts of Europe, and thus to lay the foundation of that practical knowledge which subsequently made him the greatest British authority on mining matters. Continental travelling in 1839 was by no means the easy matter it is now, and his journey through the Harz, Saxony, Austria, Hungary, Turkey, and Asia Minor, was not devoid of risk and adventure. As a result of his travels through the European and Asiatic dominions of the Sultan, he published in 1854 a work entitled "A Year with the Turks." In subsequent years, he visited during his vacations the more important mines of France, Belgium, Spain, Italy, and Norway. His official career began in 1844, when he was appointed by Sir Henry De la Beche to a post on the Geological Survey, and while holding this position he explored and geologically mapped the metalliferous districts of Devon and Cornwall, North Wales, and Ireland, and the coal-fields of Lancashire and Yorkshire, North Staffordshire and Derbyshire. In 1845 he joined the Geological Society, and in 1866 was elected President of that body. For the last 17 years he has acted as foreign secretary, in which post his rare linguistic powers proved of great service to the Society. On the foundation of the Royal School of Mines in 1851, he was appointed the first lecturer on mining and mineralogy. On the reorganization of the School in 1881, he gave up the Chair of Mineralogy, but acted as Professor of Mining until his death. He held the office of inspector of the mines in the Duchy of Cornwall, and in 1857 he was also appointed comptroller of all the mineral properties belonging to the Crown. It would be tedious to enumerate the long list of Royal Commissions and International Exhibitions with which Sir Warington was prominently associated. His report as Secretary of the Jury on the mining industry at the Exhibition of 1862 is a model of what such a work should be, and to his energy on the Council of the Inventions Exhibition of 1885 the success of the mining section was largely due.

In 1879 a Royal Commission was appointed to inquire into accidents in mines and the possible means of preventing their occurrence and of limiting their disastrous consequences. Mr. Smyth was appointed Chairman, and, in order to secure time to attend to the duties of this arduous and honorary office, he resigned the post of Examiner to the Science and Art Department—an office he had held for several years. The Commission ended its work in 1886, and during the seven years it was in existence some thousands of experiments were made, and the Report, covering 858 pages, definitely settled many important questions bearing upon the diminution of accidents in mines.

To his scientific attainments, Sir Warington added singular literary skill. His early classical training enabled him to write with an elegance and vigour unfortunately rare in technical works. He spared no pains, and neglected no details. As a teacher he was very popular with his pupils, his success as a lecturer being due not only to his finished delivery, but also to his skill as a draughtsman, which enabled him to dispense with the aid of elaborate diagrams, and to rely merely on accurate blackboard sketches, which he drew with great rapidity in the presence of his class. His reputation as Professor attracted to the School of Mines students from all parts of the world, and no better evidence of the excellence of his teaching could be adduced than that afforded by the important positions so many of his pupils occupy in the mining world. Of his literary works, the most important is his "Rudimentary Treatise on Coal-Mining"—a standard work, bearing internal evidence of not being mere extracts of books, written in 1867, and now in its seventh edition. Besides this, he wrote the articles on mining for "Ure's Dictionary" and for Stanford's series of "British Manufacturing Industries," 1876.

For his labours on the Accidents in Mines Commission, and for his other public services, he received the somewhat tardy acknowledgment of knighthood on the occasion of Her Majesty's Jubilee. Throughout his life he refused the great pecuniary rewards offered by the commercial branches of mining, and preferred to devote the half-century during which he was engaged in business connected with mines to the service of science and of the State. Although he had been in ill-health for some time, he never neglected his official duties. He died in harness, with a partially corrected examination paper on the table before him. He was buried yesterday at St. Erth, in Cornwall, not far from his home at Marazion, in the centre of the mining district with which he was so long associated. B. H. B.

NOTES.

With the consent of the Prince of Wales, the President, the Council of the Society of Arts has awarded the Albert Medal to Dr. W. H. Perkin, F.R.S., "for his discovery of the method of obtaining colouring matter from coal tar, a discovery which led to the establishment of a new and important industry, and to the utilization of large quantities of a previously worthless material."

The Essex Field Club and the subscribers to the Gilbert Club will hold a meeting at Colchester on Saturday, July 5, in memory of William Gilbert, the founder of the science of

electricity, who was born and died at Colchester. A visit will be made to Gilbert's house and tomb, and Prof. Silvanus P. Thompson will lecture on "The Early Magnetic Experiments of Gilbert of Colchester." The chair at the public luncheon will be taken by Lord Rayleigh, F.R.S., Vice-President of the Essex Field Club and the Gilbert Club. Any persons wishing to attend the meeting may obtain full particulars on application to Mr. W. Cole, Hon. Sec., Buckhurst Hill, Essex.

THERE is no foundation for the report that Dr. G. J. Romanes is a candidate for the Linacre Professorship of Human and Comparative Anatomy, Oxford.

THE Photographic Convention of the United Kingdom is now holding its annual meetings at Chester. The series of meetings was opened on Monday, and will not be concluded until Saturday. At the official welcome of the Convention by the mayor, on Monday, Mr. A. Pringle, the retiring President, introduced his successor, Mr. C. H. Bothamley. In the course of his address, the new President said that the events of last year contained nothing of first-rate importance in photography; no discoveries of far-reaching influence had disturbed the photographic world. But a good deal of interest had been excited by the announcement that advances had been made towards the solution of the problem of photographing objects in their natural colours. Coloured photographs, more or less imperfect, had been made several times, but whether they should ever get a chromatic negative process was at present entirely a matter of conjecture, and so far even the direction in which the solution was to be looked for was not apparent. Photo-mechanical printing had not presented any new feature during the past year, but the processes already in operation had been taken much greater advantage of. The applications of photography to science were becoming every day more and more numerous, and he did not hesitate to say that it was here that photography had won, and probably would win in the future, its greatest triumph.

THE Elizabeth Thompson Science Fund, which has been established by Mrs. Elizabeth Thompson, of Stamford, Conn., "for the advancement and prosecution of scientific research in its broadest sense," now amounts to 26,000 dollars. As accumulated income will be available in December next, the trustees desire to receive applications for appropriations in aid of scientific work. This endowment is not for the benefit of any one department of science, but *Science* says it is the intention of the trustees to give the preference to those investigations which cannot otherwise be provided for, which have for their object the advancement of human knowledge or the benefit of mankind in general, rather than to researches directed to the solution of questions of merely local importance. Applications for assistance from the fund, in order to receive consideration, must be accompanied by full information, especially in regard to the following points: (1) precise amount required; (2) exact nature of the investigation proposed; (3) conditions under which the research is to be prosecuted; (4) manner in which the appropriation asked for is to be expended. All applications should reach, before December 1890, the Secretary of the Board of Trustees, Dr. C. S. Minot, Harvard Medical School, Boston, Mass., U.S.A. It is intended that new grants shall be made at the end of 1890. The trustees are disinclined for the present to make any grant exceeding 300 dollars: decided preference will be given to applications for smaller amounts.

THE U.S. National Academy of Sciences is considering whether it might not be expedient to divide its membership into classes. The following classification has been proposed: mathematics, physics, astronomy, geodesy and mechanics, chemistry, geology, botany, zoology, anthropology, and political economy and statistics. The *American Naturalist*, commenting on this list, suggests that a special place should be reserved for psychology. It also expresses a hope that the division into classes will not be made a pretext for increasing the membership to above one hundred persons.

LAST week Mr. Mundella asked the Vice-President of the Council of Education whether he had received remonstrances from the principal educational authorities and managers of higher elementary schools in England and Scotland against Article 40 of the Science and Art Department, which excluded scholars in public elementary schools from being henceforward examined or earning grants in science; whether he had seen the statement of the National Association for the Promotion of Technical Education, which described this circular "as one of the most serious blows which had been struck for some years at the development of scientific and technical education"; and whether, having regard to the feeling with which the circular had been received, he would cause it to be withdrawn. Sir W Hart Dyke replied that the matter was under consideration; and he has since issued an official letter, stating that the Department had anticipated the objection which had been made, and had decided to substitute a provision which would not in any way interfere with the present system of science instruction so admirably carried out by many School Boards. With reference to a complaint that in another circular dealing with the question of manual instruction in public elementary schools, a proviso had been inserted requiring such instruction to be given out of school hours, Sir W. Hart Dyke states that a supplementary circular will be shortly issued in order to remove the doubts which exist on the subject. Sir William believes there will be no difficulty in continuing the plan now adopted, providing the time devoted to manual instruction by any scholar for the purposes of the grant is outside the minimum period required to constitute an attendance under Article 12 of the Code, which does not prevent any further time from being given to the subject within the ordinary school hours.

THE *Kew Bulletin* publishes every year a complete list of the garden plants annually described in botanical and horticultural publications, both English and foreign. In Appendix II., which has just been issued, there is a list of all the introductions recorded during 1889. It is pointed out that these lists are indispensable to the maintenance of a correct nomenclature, especially in the smaller botanical establishments in correspondence with Kew, which are, as a rule, only scantily provided with horticultural periodicals. Such a list will also afford information respecting new plants under cultivation at the Kew establishment, many of which will be distributed from it in the regular course of exchange with other botanic gardens.

THE *Botanical Gazette* informs us that the first Annual Report of the Director of the Missouri Botanic Garden has been issued. It contains a statement of the changes that are being made in the Gardens, or that are in immediate prospect; and a map of the grounds on a large scale is being prepared. The Director requests from authors copies of their publications for the library, from collectors specimens for the herbarium, and promises all feasible assistance in work calculated to promote botanical knowledge.

THE first number is published of a new quarterly journal, *Le Diatomiste*, specially devoted to the natural history and literature of diatoms. It is published at 168 Rue Saint-Antoine, Paris, under the editorship of M. J. Tempère, assisted by MM. Brun, Bergon, Cleve, Dutertre, Grove, and Peragallo. The present number (quarto, with two plates) contains descriptions of a number of new species, and a bibliography of recent diatomological literature.

IN the *Journal of the Straits Branch of the Royal Asiatic Society*, Mr. Alfred Everett has just published a most important list of the birds of the Bornean group of islands. Hitherto the work of Salvadori has been the standard record for Bornean ornithology, but the numerous discoveries of recent years have rendered that author's "Uccelli di Borneo" considerably out of date, and the Catalogue of Bornean birds published by Dr. Vorderman in 1886 is a list of names merely. The work just completed by Mr. Everett will therefore be of great assistance to ornithologists, as it gives references to all the recent scientific memoirs on Borneo, published in England and in Germany. 570 species have now been recorded from the islands, the numbers having been considerably increased by the recent discoveries of the author himself and Mr. John Whitehead's expedition to Kina Balu. Mr. Everett has given a carefully compiled list of the localities where the species have been found by himself and other travellers. Two very good maps of Borneo are given, one "showing roughly the distribution of highlands and lowlands," with all the best-known collecting stations indicated as well; and the other being a map of Palawan, showing by the soundings that this island is intrinsically a part of Borneo rather than of the Philippine archipelago.

IN the current number of the *Board of Trade Journal*, it is stated that the French Consul-General at Warsaw has informed his Government of the establishment of a commercial museum in Warsaw. This is to form a permanent exhibition of specimens of the products and manufactures of Poland, as well as a bureau of information for Russian or foreign merchants. At a small charge all persons can be supplied at the office of this museum with information on any subject connected with trade. The museum is at present at No. 66 Faubourg de Cracovie, Warsaw.

A METEOROLOGICAL SOCIETY is to be established in New York, where many persons are giving attention to weather science, owing to the relations existing between some branch thereof and their own vocation. It is intended that the Society shall be purely local at first.

Das Wetter for May contains an article by Dr. P. Perlewitz upon the influence of the town of Berlin upon its climate. He finds that the difference of the mean temperature between the town and the open country outside differs, in various months, from 0°·7 to 2°·3, the town being always warmer. The smallest differences are in spring and winter. The greatest daily differences are found to be in the evening, owing to a retardation of radiation in the town; from this time the difference decreases until about midday, when there is no perceptible difference between the two localities. Dr. Hann has found similar results for Vienna, but the differences there are smaller, owing to the better exposure of the town station. The humidity is less in the town than in the country; in the evening, in June and July, the difference amounts to above 19 per cent. No appreciable effect appears to be exerted by the town upon the rainfall, as compared with that of the country stations.

DR. G. HELLMANN, to whom meteorologists are indebted for various interesting investigations into the history of that science, has contributed to *Himmel und Erde* (Heft. 3 and 4, 1890) two instructive articles on "the beginnings of meteorological observations and instruments." He divides the history of the development of observations into three periods: (1) that ending with the middle of the fifteenth century, up to which time they were of a very fragmentary and almost aimless character; (2) that in which observations were taken, at least once a day; and (3) that in which they were systematically taken with instruments, dating from about the middle of the seventeenth century. It is not exactly known who first kept a regular meteorological journal, but Humboldt attributes it to Columbus, on his first voyage to America in 1492, while the Italians also appear to have made daily observations from the middle of the fifteenth century. The wind-vane is by far the oldest of the meteorological instruments. In the periods of Homer and Hesiod, in the ninth and eighth centuries B.C., the qualities of the winds were correctly described. The first arrangement for observing the wind-direction is the Temple of Winds at Athens, which was built about 100 years B.C. A picture of this tower is given by Dr. Hellmann. Eginhard, in the reign of Charlemagne, denoted the winds by the four cardinal points, and their variations. The first instrument for denoting the *force* of the wind is ascribed to Robert Hooke (1667); this instrument is essentially the same as that now used and known as Wild's pendulum anemometer. The absorption or organic hygrometer was invented about the middle of the fifteenth century, by N. de Cusa, although the invention is generally ascribed to L. da Vinci. The first condensation hygrometer is attributed to the Grand Duke Ferdinand II. of Tuscany. The first continuous hygrometrical observations appear to have been by R. Boyle, at Oxford, in June 1666. The first thermometer is attributed to G. Galilei, towards the end of the sixteenth century. Some few years later, the instrument was improved, although the freezing-point was the only fixed point determined, and the graduation was made by means of little knobs in the glass, every tenth one being enamelled. The first rain-gauge was used by B. Castelli in 1639, although usually a later date is quoted. The discovery of the Torricellian tube, in 1643, is too well known to require special remark. These are only a few of the very interesting points referred to in Dr. Hellmann's instructive investigations.

IN an interesting paper contributed to the May number of the Ottawa *Naturalist*, and now reprinted separately, Dr. G. M. Dawson brings together some striking facts with regard to the extent of Canadian territory which is still unexplored. The entire area of the Dominion is computed at 3,470,257 square miles, and he calculates that an area of 954,000 square miles of the continent alone, exclusive of the inhospitable detached Arctic portions, is for all practical purposes entirely unknown. In this estimate the area of the unexplored country is reduced to a minimum by the mode of definition employed, and Dr. Dawson thinks we may safely assume that it is about one million square miles, or between one-third and one-fourth of the whole. That the aggregate of unknown territory is so vast is not quite creditable to Canadian energy; but Dr. Dawson hopes that the task of exploration will be undertaken by no one who has not the necessary scientific qualifications. "The explorer or surveyor," he says, "must possess some knowledge of geology and botany, as well as such scientific training as may enable him to make intelligent and accurate observations of any natural features or phenomena with which he may come in contact."

THERE is a disease in Japan known as *kakké*, a disorder of the kidneys communicated by bacilli, and closely related to the more virulent *beri-beri*. From the distribution of *kakké*, M. Gueit has recently drawn conclusions as to the ethnic composition of the present population of Japan. The fact that Chinese always escape the disease, even in localities where it is very prevalent, indicates (in his opinion) that the Chinese or Mongolian element is not the dominant one. He finds three constituents in the population: (1) descendants of Ainos; (2) of Negritos; and (3) a Malayan element, which is the most prominent. Wherever the Malayan goes, he brings with him the *beri-beri* order of disease: his liability to this being probably due to the Hindu blood in him. From India we find *beri-beri* spread, like the Malays, to Madagascar on the one side, and to Japan on the other; we meet with it also in Java, Sumatra, &c. According to the proportion of Malay blood in the natives of Japan is the frequency of the malady, which occurs in various forms and under different names. As to the Negrito element in Japan, M. Gueit

found an interesting proof of it in the island of Sikok, in the form of a small statuette of Buddha, having the characteristic nose and hair of the Negritos.

It is a well-known fact in biology that bacteria and bacilli absorb anilin and are killed by it. Two German observers—Stilling and Wortmann—have recently considered the possibility of utilizing this property in medical treatment (*Humboldt*). The diffusibility and harmlessness of violet anilin dyes (called, for brevity, "methyl-violet") without arsenic, in small doses, were first demonstrated on rabbits and guinea-pigs. Then certain eye-disorders were produced in those animals, and treated with anilin solution, the results being excellent. The authors proceeded to operate on the human subject. A skin-ulcer on a scrofulous child, which had been treated for a month with the ordinary antiseptic agents without success, was gradually healed by daily dropping a little anilin solution on the sore; and similar good results were had with bad cases of eye-disease. It soon appeared that many surgical cases were open to successful treatment in this way; and that, in general, wounds and sores developing suppuration could be sterilized with anilin. It is also thought that cases of internal inflammation, as in pleuritis and peritonitis, may prove to be not beyond the reach of this order of treatment.

Messrs. Friedländer and Son, Berlin, have issued an important monograph, by Dr. Max Blanckenhorn, on the development of the Cretaceous system in Central and Northern Syria. The author devotes especial attention to palæontological phenomena.

A monograph, by Dr. L. Tausch von Gloeckelsthurn, on the fauna of the "gray chalk" of the Southern Alps, has been issued by A. Hölder, Vienna. The work is illustrated with nine lithographic plates.

In the Statistical Report of the Colony of Victoria, just issued, the following are given as the latitudes and longitudes of the capitals of the Australian colonies, corrected by Mr. Ellery, the Government Astronomer of Victoria:—

Colony.	Capital City.	Latitude S.	Longitude E.
		° ′ ″	° ′ ″
Victoria	Melbourne ...	37 49 53	144 58 32
New South Wales ...	Sydney ...	33 51 41	151 12 23
Queensland	Brisbane ...	27 28 0	153 1 36
South Australia ...	Adelaide ...	34 55 34	138 35 4
Western Australia ...	Perth	31 57 24	115 52 42
Tasmania	Hobart ...	42 53 25	147 19 57
New Zealand	Wellington ...	41 16 25	174 46 38

We are glad to learn that after eight years' cessation, Mr. John Fryer, of Shanghai, has revived his Chinese periodical, the title of which is best translated *Science Quarterly*. The first number of the re-issue contains 128 pages of reading matter of great variety. From a review in the *North China Herald*, by Dr. Martin, of Pekin, we gather that the science articles open with a chapter on appliances for illustrating the principles of mechanics. This paper forms a connecting link with the last number of the series, taking up the subject where it was dropped, and promising to carry it on to completion. The second paper begins a treatise on the principles of mechanical drawing, a subject in which the Chinese are beginning to take much interest. This is followed by the great topic of the day—railways. The steps necessary for the initiation and conduct of a railway enterprise are pointed out, the question of gauge is discussed, and statistics of cost are supplied. Then comes an elaborate paper on the state of the silk trade in China, pointing out the way to improvement, and stimulating the Chinese by the example of Japan and Italy. There is a paper on the sanitary conditions to be observed in the construction of dwellings, and one on medicinal plants, one on several strange vegetable productions, and one on entomology. Besides these, there are short papers on Edison's phonograph, the Eiffel tower, and on observatories and telescopes. The dessert which closes the feast is a profound disquisition by Dr. Edkins on the evolution of the Chinese language. It will no doubt surprise the natives to find that a foreigner has something to teach them in respect to their own language, both written and spoken. At the end are mathematical problems, in the estimation of native scholars the first essential of a scientific magazine. Nearly all the papers are profusely illustrated.

The additions to the Zoological Society's Gardens during the past week include two Lions (*Felis leo*, juv. ♂ ♀) from Kattywar, India, presented by H.R.H. the Duke of Clarence and Avondale; a Grey Ichneumon (*Herpestes griseus* ♂) from India, presented by Mrs. H. F. Pollock; a Common Badger (*Meles taxus*), British, presented by Mr. W. H. B. Pain; a —— Galago (*Galago* sp. inc.) from South Africa, presented by Mr. Walter Carlile; a Spur-winged Goose (*Plectropterus gambensis*) from West Africa, presented by Mrs. Quayle Jones; two Common Rheas (*Rhea americana*) from South America, presented Mr. A. W. Neeld; three Grey Sparrows (*Passer simplex*) from West Africa, a Tintillon Chaffinch (*Fringilla tintillon*), two Yellow-throated Rock Sparrows (*Petronia petronella*) from Teneriffe, a Rosy Bullfinch (*Erythrospiza githaginea*) from the Canary Islands, presented by Mr. Edmund G. Meade-Waldo; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mr. F. C. S. Roper, F.Z.S.; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mrs. Obbard; two Common Barn Owls (*Strix flammea*), British, presented respectively by Mr. Charles Faulkner and Mrs. Frederick Tibbs; an American Box Tortoise (*Terrapene carinata*), a Horned Lizard (*Phrynosoma cornutum*) from Mexico, presented by Mr. John Pettit; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. C. S. Morris; four Houbara Bustards (*Houbara undulata* 2 ♂ 2 ♀) from the Canary Islands, a Bonnet Monkey (*Macacus sinicus* ♂) from India, deposited; six Speigel Carp (*Cyprinus carpio*, var.), European Fresh Waters, purchased; two Bennett's Wallabys (*Halmaturus bennetti* ♀ ♀), a Derbian Wallaby (*Halmaturus derbianus* ♀), two Four-horned Antelopes (*Tetraceros quadricornis* ♀ ♀), a Burrhel Wild Sheep (*Ovis burrhel* ♀), a Thar (*Capra jemlaica*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on June 26 = 16h. 19m. 54s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4230	—	—	16 37 45	+36 41
(1) β Lyræ	Var.	—	18 46 0	+33 14
(3) α Scorpii	1	Redddish-yellow.	16 22 40	+26 14
(4) β Herculis	2	Yellow.	16 25 30	+21 44
(5) δ Herculis	3	Bluish-white.	17 10 30	+24 58
(6) S Leonis	Var.	Yellowish.	11 5 9	+ 6 3·5

Remarks.

(1) This is the bright cluster of stars in Hercules which is probably well known to every possessor of a telescope. Seeing that it certainly consists of separate and distinct stars, no nebulosity being shown in Mr. Roberts's photograph of it, Dr. Huggins's observation of its spectrum in 1866 is very remarkable. He says:—"Spectrum of the central blaze continuous.

Spectrum ends abruptly in the orange. The light of the brighter part is not uniform; probably it is crossed either by bright lines or by lines of absorption" (Phil. Trans. 1866). As yet we know nothing of the spectra of the components of any star cluster except in the case of the loose cluster of the Pleiades, and in that case we know that the spectra are all of the same type—namely, Group IV. It seems pretty evident that the stars of the cluster in Hercules cannot have spectra of this kind; otherwise, their integrated light would not end abruptly in the orange, and the irregularities would only be obvious in the blue end, where the thick hydrogen lines ought to be visible. The absence of red light would lead rather to the supposition of bright lines than dark ones. Further investigations, with considerable optical power, may therefore lead to interesting results. It may be noted that Vogel, in 1872, recorded simply a continuous spectrum, but his attention had probably not been directed to Dr. Huggins's statement.

(2) The question of the periodicity of the appearance of the bright lines in β Lyræ cannot yet be said to have been satisfactorily settled, and as the star will be visible for some months, further continuous observations are desirable. It is not necessary here to recapitulate all the observations which lead to the conclusion that there is a periodicity in the spectrum. Gothard has probably given more attention to the star than any other observer, and he succeeded in following the variations of the line D_3 through several periods "from a bright, almost dazzling light to complete disappearance. . . . The variation is most marked in the case of D_3; it is much less striking in the hydrogen lines, although they, and probably also the dark bands in the red, are subject to a periodical variation." The period has been provisionally estimated as 7 days, but it does not seem to depend upon the fluctuations in the brightness of the star. In my own observations I have found that the bright lines in this star are best seen when no cylindrical lens is employed, and this has also been noted by other observers. Further observations, to be of any value, should be made as frequently as possible, and over a long period.

(3) Dunér describes the spectrum of this star as one of the most magnificent of Group II., the bands 1–9 being wide and dark. He also states that there is a narrow band between bands 3 and 4. As the spectrum is a bright one, this is a good opportunity for comparing the dark flutings with the brightest flutings of manganese, lead, and magnesium. In the recently issued volume of spectroscopic observations at Greenwich, Mr. Maunder states that he has found the bright green band in α Herculis coincident with the brightest carbon fluting and possessing the same characteristics. A similar comparison should also be made with α Scorpii.

(4 and 5) These stars, according to the observations of Gothard and others, have spectra of the solar type and of Group IV. respectively. The usual more detailed observations are required in each case.

(6) The spectrum of this variable has not yet been recorded. The magnitude ranges from about 9 to < 13 in a period of about 188 days. There will be a maximum about July 2.

A. FOWLER.

GREENWICH SPECTROSCOPIC RESULTS.—These results for 1888 contain observations of γ Cassiopeiæ, Mira Ceti, α Orionis, α Herculis, β Lyræ, R Cygni, P Cygni, β Pegasi, and Comets a and e 1888. On October 5, 1888, ten measures were made of a bright line in the violet part of the spectrum of Mira Ceti; the mean wave-length found was 4343·37, indicating that it was the third line of hydrogen. F and D_3 were searched for on this occasion, but without success. The spectrum of α Herculis was compared with those of carbon and manganese, as given by a Bunsen flame on several occasions, and it is noted: "The green band of the carbon spectrum accorded, both as to position and appearance, with the bright interspace or 'zone' to the blue of Band VII. (Dunér's numeration). So far as the dispersion employed would show, no accordance could be more complete, both as to the position of the edge and the gradation of the fading." The blue carbon band was also found to present an approximation in position and appearance to a bright zone in the blue. The wave-length of the brightest bands in the manganese spectrum was determined as 5579, and that of the more refrangible edge of Dunér's Band IV. as 5592, whence it is concluded that the connection of the spectrum of the star with the manganese spectrum did not appear to be made out. A bright line at 5873·92, that is, D_3, was measured in β Lyræ on August 10, 1888, was seen less distinctly a month later, and was found again to be quite distinct on September 19; two days later, D_3 was seen very bright, and C and F were also visible. D_3 was visible, but faint, on October 1; F could not be seen, and C was only suspected. On October 19, C and F were not visible as bright lines, but were first suspected as dark lines, whilst D_3 was glimpsed occasionally as a feeble bright line. R Cygni was observed on September 21, D_3 was identified with probability in its spectrum, and F with certainty; and, on October 1, ten measures were made of the F line in P Cygni. Comet a 1888 was observed on April 19, 1888; its spectrum appeared mainly continuous; two bright bands were just glimpsed, coincident with the bands in the green and yellow of the spectrum of a Bunsen flame, the band in the blue being suspected. On May 3 the spectrum was practically wholly continuous, traces of the green band only being suspected. Comet e 1888, observed on November 27, showed a local ill-defined brightening, corresponding nearly to the great carbon band, but apparently further towards the blue, otherwise it was perfectly continuous.

THE ROTATION OF VENUS.—Signor Schiaparelli has recently made an extended inquiry into the question of the rotation of the planet Venus, and has brought many facts to light concerning it (*Rendiconti del R. Istituto Lombardo*, vol. xxiii.). He finds, from observations of very definite spots, that the time of rotation of the planet is 224·7 days—that is to say, Venus, like the moon, and probably Mercury, rotates on her axis in the same time that she takes to make a sidereal revolution around the sun; the axis of rotation being nearly perpendicular to the plane of the orbit. By investigating the writings of previous astronomers who have estimated the rotation period, Signor Schiaparelli concludes that those observations which have been supposed to fix the time as about 24 hours are open to question. Domenico Cassini's observations of bright markings in 1866–67 are shown to have been wrongly interpreted, a discussion of them indicating that they also support a period of rotation of 224·7 days.

GEOGRAPHICAL NOTES.

THE Russian Geographical Society has received fresh news from M. Grombchevsky as to his attempts to penetrate into Tibet from the north. In the autumn of 1889 the expedition explored the Uprang, a tributary of the Raskem-daría, tried to enter again into Kanjut, and, having failed to do so, explored the tributaries of the Raskem river which flow from the Himalayas. On November 21, M. Grombchevsky, accompanied by two men only, crossed the Kara-korum Pass, and went to the Pahnu mountaineers, who live by sheep-breeding, and suffer a good deal from the Kanjut robbers. On December 7 the expedition was at the small fort of Shahi-dulla-hodja; the winter had come, and the thermometer fell in the nights to $-20°$ Celsius. Nevertheless, M. Grombchevsky, with two men only and a guide, explored the passes leading to Karakorum across the Raskem ridge. The tent had to be abandoned, although the temperature was $-35°$, and the party was soon obliged to return. On January 7, after having followed for some distance the Kara-kash river, the small party began its ascent of the steep slopes of the Tibet border-ridge. The plateau itself proved to be a desert, 17,000 feet high, upon which a few yaks, *Kulangs*, and mountain sheep were grazing. A very high ridge, called by M. Grombchevsky the Yurungkash ridge, was crossed, the pass receiving the name of "Russian." But the horses of the expedition were quite attenuated, and on January 13 the party was brought into a perilous condition by a frightful snow-storm and a temperature of $-27°$, without having either a tent or any kind of fuel. M. Grombchevsky was compelled to return, marching all day long. After having made another unsuccessful attempt at crossing the Hindu-tash Pass, the expedition went to Kilian, and thence to Polu, thus connecting its surveys with those of Prjevalsky. A telegram received from New Marghelan, in Russian Turkestan, announces that the explorer and his men have returned safely, and are making new schemes for further exploration. A map, annexed to the last issue of the *Izvestia* of the Russian Geographical Society, embodies the surveys made by M. Grombchevsky in 1888 and M. Grum-Grzimailo in 1887.

IN the course of last year the Geographical Society of Berlin published no fewer than thirty-nine remarkable maps. Three of them are reproduced from those of Mercator, now in the

town library of Breslau. Two othe. -a map of Europe (finished in 1554) and one of England (of 1564)—are unique. Another is the large map of the world, of which there are only two copies in existence, the second one being at the Paris National Library. The Society has agreed to publish the details of Dr. Konrad Kretschmar's journey to Rome, undertaken in the Middle Ages for purposes of research.

THE LADIES' CONVERSAZIONE OF THE ROYAL SOCIETY.

THE Ladies' *Conversazione* of the Royal Society was held on June 18, and was, as usual, a great success. Many of the exhibits were the same as those shown at the *conversazione* on May 14. Among those which had not been previously shown were the following:—

Exhibited by the Director-General of Ordnance Factories:—Magazine rifle, Mark I. The new magazine rifle now being made for the British Army. It has a calibre of 0″·303, is on the bolt principle, and is provided with a detachable magazine underneath, to hold eight cartridges; a cut-off on the right side enables it to be used as a single loader. It has two sets of sights, the ordinary ones are graduated up to 1900 yards, the long-range sights on the left side up to 3500 yards. The sword-bayonet, which is attached underneath the barrel, has a double-edged blade 12″ long.

Exhibited by the Director-General of the Geological Survey:—Diagrams illustrating some of the most ancient topography of the British Isles. (*a*) Corry on Ben More, Assynt. The rough bossy ground in the middle is the Archæan gneiss, the most ancient rock in this country. Above it to the left comes the Torridon sandstone, forming a range of cliffs, and lying unconformably on the gneiss. At the summit of the Corry, on the crest of the ridge, lies the early Palæozoic quartzite, which steals across the sandstone until it rests directly on the gneiss. (*b*) Sleagach, Loch Maree. The pinkish bossy rock is the old gneiss, which rises into a group of hills that have been buried under the Torridon sandstone. By prolonged and enormous denudation of the overlying sandstone, the gneiss hills have been uncovered, and now reveal a portion of the oldest known topography of Britain. The gneiss hill to the right rises to a height of 2500 feet, and in ascending it one can walk along the ancient shore-line and traverse beach after beach that was piled up over the sinking land. (*c*) View from the south shoulder of Sleagach looking east. The bossy hills of gneiss rise towards the left hand to a height of 3000 feet above the sea. The overlying cover of Torridon sandstone, though enormously denuded, still forms a range of lofty hills, beneath which knobs of gneiss at different elevations may be seen protruding. The quartzite (coloured yellow) caps the mountains to the right until a mass of the old gneiss overlies it. This cake of the most ancient rock of the region has been torn up and thrust over the younger formation. The line of junction or "thrust-plane" between them descends into the plain, and runs for miles to the westward. (*d*) Meall a Ghubhais, Loch Maree. The upper part of the mountain is a cake of Torridon sandstone, which has been driven westward by the same gigantic terrestrial movements just referred to, and has been placed upon the quartzite group of rocks which ought really to lie above it. In the lower part of the diagram the sandstone is seen in its normal position below the quartzite. (*e*) Section of Meall a Ghubhais, to show the detailed geological structure of the mountain. It will be observed that the upper shifted mass of Torridon sandstone is traversed by several thrust-planes, and that portions of the old gneiss have likewise been driven westward underneath it.

Exhibited by Mrs. F. W. H. Myers:—(1) Platinotype photographs. (2) Photographs on fabrics.

Exhibited by Sir William Bowman, Bart., F.R.S.:—(1) Jubilee portrait of the late Prof. Donders, For. Mem. R.S., painted by Mrs. Donders (Hubrecht). Gold Medal awarded at the Exposition International, Munich, 1888. Ultimately destined for the National Museum, Amsterdam. (2) Uncompleted portrait of the same, 1873, by G. F. Watts, R.A.

Exhibited by Prof. W. C. Roberts-Austen, C.B., F.R.S.:—Measurement of high temperatures. Experimental determination of the melting-point of gold (1045° C.) and of silver (945° C.), by means of Le Chatelier's pyrometer. This consists of a thermo-couple, composed of wires of platinum and platinum alloyed with 10 per cent. of rhodium, connected with a dead-beat galvanometer. The pyrometer scale has been calibrated by heating the thermo-couple to certain known temperatures determined by the air thermometer.

Exhibited by Prof. A. M. Worthington:—An apparatus for stretching a liquid and measuring simultaneously the stress and strain.

Exhibited by Mr. P. L. Sclater, F.R.S.:—Portrait of Dr. Emin Pasha, C.M.Z.S., and original letter from him, addressed to Mr. Sclater, dated Wadelai, April 15th, 1887.

Exhibited by the Postmaster-General:—Hughes's type-printing telegraphs, working to the Continent. This apparatus is mainly mechanical, the electrical action being confined to the sending a single short pulsation of current at the instant the type-wheel is in the proper position, and only one wave of current is needed to produce a letter. The sending and receiving instruments are combined. The key-board consists of as many keys as there are letters and signs to be printed. Connecting with the keys and corresponding with them, and also with the type-wheel, is a set of pins arranged radially in a circular horizontal plate. An arm revolves over these pins without touching them until a key is depressed, when a current is sent into the line. The instruments are caused to run approximately isochronously by means of suitable adjustments, and they are afterwards maintained in synchronism automatically by the actual working. The instrument is eminently suitable for Continental message traffic, for which purpose it is largely used. The three working instruments shown were connected with Paris, Berlin, and Rome. In the course of the evening the President held communication with Profs. Helmholtz and Du Bois-Reymond in Berlin, Prof. Mascart in Paris, and Prof. Cannizzaro in Rome.

Exhibited by Mr. Walter Gardiner, F.R.S.:—(1) Specimens of aquatic fen plants and algæ occurring in the neighbourhood of Cambridge. (2) Specimens illustrating the exhibitor's paper on a new method of printing photographic negatives, employing living leaves in place of sensitive paper.

Exhibited by Dr. Pole, F.R.S.:—Diagrams in illustration of colour-blindness.

Exhibited by Dr. Karl Grossmann:—Tests for colour-blindness.

Exhibited by Prof. J. W. Judd, F.R.S.:—Specimens of a remarkable nickel-iron alloy (awaruite), of terrestrial origin, from New Zealand, and of the minerals and rocks with which it is associated. Sent by Prof. G. H. F. Ulrich, of the Dunedin University, N.Z. This curious mineral, consisting of $2Ni + Fe$, was analyzed and named by Mr. W. Skey, in 1885, having been detected by him in specimens of sands obtained from streams in the south-western part of the South Island of New Zealand. Prof. Ulrich has since been able to show that the grains of this alloy are found over a considerable area, disseminated in peridotite and serpentine rocks; which rocks are intrusive in the metamorphic schists of the district, and form the Red Hill and Olivine Ranges. The substance which awaruite most closely resembles is the Oktibbehite meteorite, consisting of $Ni + Fe$: and the occurrence of this remarkable alloy in terrestrial rocks is comparable to the presence of nickel-iron alloys in the basalts of Ovifak and other localities in Greenland.

Exhibited by Prof. A. H. Church, F.R.S.:—A selection of Japanese sword guards, or *tsuba*, made of malleable iron, and variously decorated with chased, hammered, and pierced work, or with incrustations in gold, silver, shakudo, shibuichi, and bronze. The majority of the examples shown represent plant forms, and were executed between 1650 and 1850.

Exhibited by Prof. W. C. Roberts-Austen, C.B., F.R.S.:—Japanese art metal-work. The specimen is interesting as a modern example of flat inlaying in metals. The plate is of bronze, and the bird is of *shakudo*, or copper alloyed with a small quantity, about 2 or 3 per cent., of gold. The isolated feathers are of a darker variety of this alloy.

Exhibited by Dr. W. J. Russell, F.R.S.:—Ancient Egyptian colours discovered by Mr. Flinders Petrie in the Fayoum, and modern imitations of them; and colours from Hawara in the Fayoum.

Exhibited by Mr. A. P. Laurie:—Colours used by the fifteenth century painters.

Exhibited by Mr. W. F. R. Weldon, F.R.S. (on behalf of the Marine Biological Association):—Larvæ of certain food-fishes, together with other animals of interest inhabiting Plymouth Sound.

Exhibited by Prof. A. C. Haddon, on behalf of Mr.

A. Haly, Director of the Colombo Museum:—Some tropical fishes preserved in a mixture of gum and glycerine, as a means of displaying their natural colours. The results, although not as good as with some of the specimens located in the museum itself, represent the outcome of a series of experiments extending over a number of years, full details of which are to be found in the "Ceylon Administration Reports." Gum and glycerine have long been used in combination in microscopy, as a substitute for Canada balsam; on account, however, of the difficulty experienced with air-bubbles, their use is now very generally given up. Mr. Haly's experiments have shown that if the specimens preserved in the mixture which he employs be placed in a medium which will precipitate the gum, all colour is quickly lost, wherefore the preservation of the latter would appear to be due to the gum's influence. Mr. Haly is still prosecuting his experiments, and his latest researches show that the employment of pure glycerine for mounting (a well-nigh prohibitory condition) is no longer indispensable. He now finds that gum and glycerine are miscible with alcohol in all proportions necessary for his purposes, provided certain precautions be taken in the manipulation. He is thus enabled to check the ravages of fungoid organisms which earlier impeded his progress; and, by reducing the syrup to the necessary specific gravity with proof spirit, he is enabled to successfully preserve frogs, reptiles, and other organisms with which he originally failed, to no small degree as the result of the excessive dehydrating powers of his original medium. Mollusks, sea anemones, and jelly-fish, are among those forms of life with which the method has been least successful. Mr. Haly tells us, however, that for the Alcyonidæ his mixture is a good preservative, and that sea-water saturated with bichromate of potash has been found excellent for hardening jelly-fish. The power to preserve the natural colours of plants and animals is now the desideratum of the museum curator. Some of Mr. Haly's exhibits have stood the test of from two to three years' exposure to the light in a tropical climate. The outlook is a hopeful one; and the facts show the colonial worker, who is apt to be lost sight of in these days of competition and aggrandisement, to be fully abreast of the times, and alive to the best interests of the public.

Exhibited by the Zoological Society of London:—Eggs of a large python (*Python molurus*) laid in the Zoological Society's reptile-house. The pythons lay about thirty to fifty eggs at one time, and incubate like birds. The female python on the present occasion has "declined to sit," but on former occasions this process has been carried on in the gardens (see Proc. Zool. Soc., 1862, p. 365). Abnormal heat is developed by the sitting python as by the sitting hen.

Exhibited by Prof. A. Macalister, F.R.S.:—Two mummy heads of priests (12th and 18th Dynasties) from tombs near Assouan, Upper Egypt.

Exhibited by Sir Archibald C. Campbell, Bart:—Photographs of musical sparks, done at Blythswood, Renfrewshire, by the exhibitor.

Exhibited by Dr. Augustus D. Waller:—Demonstration of the electrical variations of the heart of man and of the dog.

The following demonstrations by means of the electric lantern took place in the meeting room:—

Animal and bird studies, photographed from life, exhibited by Mr. Gambier Bolton.

The orientation of some ancient temples, exhibited by Prof. J. Norman Lockyer, F.R.S.

Experimental demonstrations on electro-magnetic repulsion phenomena, and a series of experimental demonstrations illustrating the principal facts of the phenomena of electro-magnetic repulsion, discovered by Prof. Elihu Thomson, and their applications in alternate current electro-magnetic motors, as exhibited in the Paris Exhibition, exhibited by Prof. J. A. Fleming and Mr. Ernst Thurnauer.

THE SUNDAY SOCIETY.

ON June 21 the Sunday Society held its fifteenth annual meeting in Prince's Hall, Piccadilly. Prof. G. J. Romanes delivered his address as President of the Society. After a brief analysis of the Sunday question in general, he spoke as follows:—

As you will see from the fifteenth Annual Report which is now in your hands, the present year is one of unusual activity on the part of our Society. First of all, it has been marked by a wise stroke of policy in sending a deputation to the French Ministry for the purpose of obtaining information as to the practical results of opening the great Exhibition of Paris on Sundays. Moreover, as explained in the Report, the Committee desired to ascertain whether there be any reality in "the great bugbear of the Sabbatarian mind"—viz. that the Continental Sunday is a day of irreligion to the masses, and of overwork to the Government *employés*. As you will see from the Report, the result has been conclusively to prove the unreality of the bugbear, so far at all events as the specific question of the opening of national galleries and museums is concerned. With the more general aspects of the Continental Sunday we have not, as a Society, anything to do; but I may remark in passing that we must here remember differences of national taste and feeling. What would be irreligious levity in one community need not be so in another; and it would be absurd to attribute these differences of sentiment to differences in the matter of Sunday observance.

Next, you will find from the Report that the Trustees of the People's Palace received a memorial from the Working Men's Lord's Day Rest Association, which was promptly responded to by a counter-memorial from this Society. The latter document may best be left to speak for itself; and as it speaks with so much good English common-sense, I scarcely feel it desirable to move a vote of thanks to the Trustees of the People's Palace for having listened to us rather than to our opponents: I prefer to take it for granted that the Trustees perceive as plainly as we do on which side of this matter the truth and the wisdom lie.

Again, you will learn from the Report that, in addition to the public institutions previously opened on Sundays, several others have been this year added to the list, which now comprises 23 in all. Moreover, this year has likewise witnessed the great reform of throwing open the British Museum on certain week-day evenings; while both the authorities there and those at the National Gallery have expressed, not only their willingness, but their desire to throw open to the public on Sundays these by far the greatest of our national collections. In my opinion it is impossible for us as a Society to over-estimate the importance of having thus gained the express and cordial support of the most representative museum on the one hand, and of the most representative art gallery on the other. It now only remains that the Treasury should allow a small grant to defray the comparatively nominal expenses, and our cause would be won throughout the length and breadth of the land. For if once the British Museum and National Gallery were opened on Sundays, no other museum or art gallery could afford to resist any pressure that might be put upon them to follow so overwhelming an example. Our big guns, therefore, are at last fully charged and ready to fire; only the trigger waits to be pulled, and this it is that we are now about to attempt.

For you will observe, in the last place, that the Report in your hands contains a very weightily worded memorial which was sent to the Chancellor of the Exchequer in the middle of April. Where so many forcible considerations are comprised within so small a compass, one is much tempted to read the whole. But as other speakers are to follow me, I shall merely indicate one or two of the points in this memorial which appeal to me as of most importance.

First, then, I would have you observe how strong a ground the appeal is based upon, where it calls attention to the fact that the House of Commons has already and amply recognized the principle of their obligation to open on Sundays our national museums, galleries, gardens, &c., by having already furnished the funds requisite for the purpose to Kew, Hampton Court, Greenwich, Dublin, and Edinburgh. Again, as another very notable feature in this memorial, I may mention the enormously strong support to which it draws the attention of Mr. Goschen as having recently been given to the objects of this Society by the London County Council, who passed an almost unanimous resolution in favour of our policy. Yet once more, can anything be more calculated to sway the mind of a Minister than the anomalous state of matters to which the memorial draws attention, where it indicates that the governing bodies of the British Museum and National Gallery are expressly desirous of making arrangements whereby the priceless collections under their charge may at last become in very truth, or without any restriction, the property of the British public? When provincial institutions of incomparably less importance have already succeeded in obtaining funds from the Treasury for this purpose, is it right or fitting that the great Metropolitan institutions should be

denied a similar privilege, when their governing bodies unite with the London County Council in the petition which this memorial sets forth?

Seeing, then, that our position has now grown to be one of such well-nigh irresistible strength, I think you will all agree with me in holding that a policy which has gained such results during the past fifteen years of our existence as a Society ought to be the policy which we shall continue to follow. Having achieved this large measure of success by our quiet persistence in the way of enlightening public opinion, and patient gaining of all the strategic points of importance which we now hold, I, for one, would strongly deprecate the more noisy methods of popular agitation, with their Hyde Park processions, and so forth. But there is one piece of machinery which we have used with considerable effect on several occasions in the past; and this piece of machinery we intend once more to put into motion.

Three times in the fifteen years of its existence the Sunday Society has convened a National Conference, and in the opinion of our Committee the time is ripe for the convening of another. Therefore arrangements have been made for this the fourth Conference to meet in London during the present year. I must express my gratification that the Committee have thought fit to elect me President of the Society in a year which is thus destined to be one of unusual prominence in its annals, and I may be permitted to record my thanks for the honour which has thus been conferred, even while expressing my regret that the duty of presiding over the coming Conference has not fallen into abler hands.

As you are probably well aware, the importance of these Conferences consists in their bringing together, and combining in a collective manner, representative opinions upon the Sunday question from all parts of the kingdom. Not only are invitations issued to institutions which are already opened on Sundays to send their delegates, but statements of opinion are solicited from eminent men in all departments of science, art, and letters, as well as of public life and social organization. In this way we are able to focus the best thought of our time upon the objects which we have in view, and to deliver the result in the form of printed papers to the public, and of weighty resolutions to the Government. Time does not admit of my dwelling as fully as I should have desired upon this the most important feature of our programme for the current year, and therefore I will ask you to read an instructive historical sketch which has already been published by our Hon. Secretary, touching the work that has been accomplished by the three previous Conferences. You will find this sketch in the *Sunday Review* for January of the present year, and in order to give you a general idea of its substance, I will conclude by making two short quotations. The first I give as a sample of the opinions obtained from eminent men, and the second I give as a brief epitome of the work that we hope to accomplish by means of the fourth Conference.

The sample of opinion which I select for quotation is taken from what was said by Sir Joseph Hooker at the last Conference; and I select this expression of opinion, not only because its author, like his illustrious ancestor, is proverbially gifted with one of the best judgments that has ever helped to raise a man to the highest rank of eminence, but also because his opinion is, in this case, founded upon a statement of the most cogent facts.

Speaking as Director of the Royal Gardens at Kew, Sir Joseph Hooker said:—

"If there is one matter that gratifies me more than another, in respect of the administration of the Kew Gardens and Museums by the Government, it is the opening them to the public on Sundays. On no day of the week have we more interested visitors, or more of that class which we should wish to see profiting by the instructive contents of this institution. The Museums especially are crowded, and, when it is considered that the exhibits in them are not of articles that strike the eye or gratify the senses of colour or form, the interest they excite is almost to be wondered at. The artisan classes are great frequenters of these Museums, with their wives and families, and it is pleasing to see the delight with which the children recognize such articles as the sugar-cane, the coffee-plant and its products, and the various implements used in their preparation, manufacture, &c. I should add that this interest in the instructive character of the Gardens is largely on the increase, and is mnnifest to the most careless observer. It is further accompanied by a marked improvement in the conduct of certain classes, which were formerly troublesome in many ways, and a nuisance to quiet visitors. It speaks volumes for the moral effect of the Sunday opening when I add that such classes no longer exist at Kew. Whether it is that such no longer come, or that, coming, they now behave themselves, is immaterial—the moral gain is great. During the last two years we have had in each year a million and a quarter of visitors, of whom the greater proportion are Sunday afternoon arrivals from every quarter of the metropolis and its surroundings. Let the numbers speak for themselves:—1882, Sunday visitors, 606,935; week-days, 637,232. 1883, Sundays, 616,307; week-days, 624,182."

The other quotation is taken from the close of our Hon. Secretary's paper on National Conferences, already alluded to:—

"Thus the Fourth National Conference will be able to point to the friendly action of the Government in providing funds for opening the British Museum to those who desire to visit it on week-day evenings; it will have a friendly Chancellor of the Exchequer to appeal to in Mr. Goschen, who is backed up by the vote of the London County Council, and meets Parliament with a surplus which, there is a general opinion, should in part be devoted to education.

"Could education be better or more equitably promoted than by furnishing the Trustees of the National Museums and Galleries in the Metropolis with the funds necessary for throwing open these avenues of culture and refinement to the millions of people surrounding then? The people have already not only the inclination to become better acquainted with the contents of these Museums and Galleries, but they have for the most part the necessary leisure for this purpose on the fifty-two Sundays throughout the year, when the Trustees are precluded from opening them solely from want of funds, which it is just as much the duty of the Government to provide in London as outside of it, and for those who wish to visit the Museums on Sundays as well as for those who wish to do so on week-day evenings. Should the Conference make a strong appeal to Mr. Goschen, and through him to the Government, to deal justly by London in this matter, the time cannot be far distant when the reproach to the nation of having all such institutions as the National Museums and Galleries in the Metropolis closed on Sundays will be removed."

These, as I have said, are the words of our Hon. Secretary. And I cannot refer to him from the chair which I have now the honour to occupy without asking you, in conclusion, to join with me in heartily recognizing the unique value of his indefatigable work in promoting the objects of this Society. For I know it is not too much to say, that at whatever time the reproach to the nation of which he speaks will eventually be removed, its removal will have been due much more largely to one Englishman than to any other, and that the name of this Englishman is Mr. Mark H. Judge.

SCIENTIFIC SERIALS.

Studies from the Biological Laboratory of the Johns Hopkins University, Baltimore, vol. iv., Nos. 5 and 6.—No. 5 contains:— Some observations on the effect of light on the production of carbon dioxide gas by frogs, by H. Newell Martin and Julius Friedenwald. The influence exercised by light on the metabolisms of the animal body has been recognized for the last fifty years. Following up the researches of Moleschott, the authors experimentally proved that, in frogs deprived of their cerebral hemispheres, a greater quantity of carbon dioxide is given off in the light than in the dark; that, therefore, the influence of light in producing greater oxidations in normal frogs is simply reflex, and not due to greater bodily activity brought about by psychical conditions dependent on the light; that the cerebral hemispheres do not take any direct part in regulating the oxidations of the frog's body; and that this reflex action of the light, though mainly effected through the eyes, is produced partly also through the skin.—On the comparative physiological effects of certain members of the ethylic alcohol series (CH_4O to $C_5H_{12}O$) on the isolated mammalian heart, by John C. Hemmeter.—On the ventricular epithelium of the frog's brain, by A. C. Wightman. The author concludes that the epithelial layer of the frog's brain and spinal cord forms a continuous lining to the central nervous system. It is everywhere a single layer thick. The epithelium of the ventricles forms a central zone of cells, about which the brain-cells are concentrically arranged. The cells of the epithelium and of the brain are connected by processes which extend from the tips of the former. The epithelial layer consists

of cells of several varieties—the columnar, the spindle, and intermediate forms; all are ciliated.—On the temperature limits of the vitality of the mammalian heart, by H. Newell Martin and E. C. Applegarth.

No. 6 contains:—On the morphology of the compound eyes of Arthropods, by S. Watase (plates 29 to 35). In studying the structure of the ommatidium of the compound eye of Serolis it was found that it might be reduced to a simple ectodermic invagination of the skin. Extending his researches over several other Arthropods, the author found that the same interpretation could without exception be applied, and he thinks this view of the ommatidium finds its strongest support in the fact that in Limulus the ommatidium is an open pit of the skin. If these views be correct, the unit of the compound eye of an Arthropod is not so complex as has generally been conceived, and the total result is but the vegetative repetition of a similar structure. In an appendix the author alludes to his investigations into the structure of the eye in Echinoderms, the result of which he hopes shortly to publish.—On the anatomy and histology of *Cymbuliopsis calceola*, Verrill (plates 36 to 39), by J. I. Peck. A few specimens of this rare Pteropod were found in the Gulf Stream, off Cape Charles.—On the amphibian blastopore, by T. H. Morgan (plates 40 to 42), concludes that in some forms it becomes altogether or in part the neurenteric canal; in some it becomes the anus; in some, again, it closes and a new anus is formed, while he believes that in Amblyostoma it becomes both the neurenteric canal and the anus.—On a new Actinia, by Dr. Henry V. Wilson (plate 43). This new form was found on the small reef which fringes the shore of No Name Key, Abaco, Bahamas. It was discovered in a perforation on the under surface of a porites-like asteroid coral, and, though constantly looked for, but the one specimen was found. It has been called *Hoplophoria coralligens*. Below the twelve long tentacles are cycles of smaller ones, and below these four remarkable large organs, which give the animal a most *bizarre* appearance; these are diverticula of the gastro-vascular cavity, and are stinging weapons. The genus is placed provisionally with the Antheadæ.

Bulletins de la Société d'Anthropologie de Paris, tome xii. (série 3), fasc. 4, 1889.—Continuation of M. Variot's paper on pigmentation of the skin in the region of cicatrized lesions in the negro.—Descriptive ethnographic summary of the course of distribution of different races in Europe, by M. Lombard. Starting from the Neanderthal race as the only one referable to the Quaternary period, the author attempts to show that as early as the age of their descendants—the Cro-Magnon men—various alien races had already appeared contemporaneously with the latter in Central Europe. From this point, M. Lombard undertakes, on very vague premises, to trace the advance westward of successive and intersecting streams of brachycephalic and dolichocephalic peoples bringing with them their own special civilization of the dolmen, polished stone, or other, period. His view that the Pamir plateau is the cradle of the Aryans, and that they belonged primarily to the blonde races, is strongly combated by Mme. Clémence Royer, whose able refutation of his somewhat crude opinions gives to his paper its sole claim to notice.—Communications on the silex of Breonio, near Verona, and on spurious French and Italian flint implements, by M. de Mortillet, who shows the extent to which the manufacture of so-called palæontological objects is carried on.—On a case in which the gray commissure of the third ventricle was absent; and on the concomitant psychic characteristics of this anomalous condition, by Dr. F. de Marcedo.—On the mummified brain of an ancient cranium found in Venezuela, by M. Chudzinski.—On venous circulation in stumps, by Dr. Lejars.—On a rabbit with only one ear, by M. Chervin.—On the effects of the artificial deformity of the skull in a Bolivian infant, by M. Manouvrier.—On various prehistoric stations in the Department of Seine-et-Oise, by M. Vauvillé. The finds at Crespières included three implements of a sandstone not found in the district, the remainder being of cut flint. At Grancières evidence exists of the extensive manufacture of extremely small flint implements similar to those found in India, and in the neighbourhood of Tunis and Algiers, as well as in parts of South-Western Europe. The objects generally would seem to belong to the Palæolithic and Neolithic ages.—On the skeletons found at Castenedolo in Lombardy, and assumed by M. de Marcedo to be of Tertiary origin, by M. de Mortillet.—On the utility of family burying-places in reference to the study of the influence of heredity on anatomical characteristics, by Mme. Clémence Royer.—On the megalithic remains of the Department of La Somme, by M. Pouchon. The author points out the inaccuracy of the official lists published for the district, and describes a number of interesting, so-called polishing stones, and other monoliths, which demand immediate protection from the Government to save them from wanton destruction.—On the distribution of muscular force in the hand and foot, observed by means of a new form of dynamometer, by M. Féré.—Final Report of the Eighth Congress of Orientalists at Stockholm, by M. O. Beauregard.—The prehistoric stations of Coucouteni, Roumania, by M. Dimandi. The finds here are of special interest, as showing the advanced civilization of a people, probably of Greek origin, who as early as the fourth or fifth century B.C. occupied this region. The enormous number of idols, chiefly female, was a marked characteristic of the station. Besides anthropomorphic idols, a few animals, as cows and bulls, were used to represent some forms of divinities.—On the various forms of projectiles of the Neolithic age, by Dr. Capitan.—On bronze objects found in the bed of the Marne, by M. P. Masson.—On the flint knives and arrows of the Department of Aisne, by M. Vauvillé.—On the prehistoric station of Lengyel, in Hungary, by M. Nadaillac.—On a case of a pseudo-male hermaphrodite, by Dr. Pozzi.—On artificially induced deformity of the head as still practised in the Haute Garonne, and other parts of France, by Dr. Delisle (with illustrations).—Report of the Sixth Broca Conference.—On the erroneous establishment of a distinct order of true Bimana, by M. Hervé. The object of the essay is to prove that the Simians have, like man, two hands and two feet, and cannot therefore be classified as true Quadrumana, or true Bimana.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 5.—"On the Passive State of Iron and Steel. Part I." By Thomas Andrews, F.R.S., M.Inst.C.E.

The passive state of iron appears first to have been observed just a century ago by Keir, and brought before the notice of the Royal Society in 1790 (Phil. Trans., 1790, p. 379); he observed that strong nitric acid had no action on iron when the metal was placed therein. Since then, Bergmann, Schonlein, Faraday, Herschel, and others, have conducted investigations in relation to this phenomenon. In the present paper are presented the results of a study of certain magnetic, temperature, and other conditions which the author found to affect the passive state of iron and steel. The experiments of Part I. are classified under the following heads:—

Series I., containing the results of observations on the influence of magnetization on the passive state of steel in cold nitric acid, specific gravity 1·42.

Series II., treating of the influence of magnetization on the passive state of steel in warm nitric acid, specific gravity 1·42, the experiments showing that magnetized steel bars were less passive in warm nitric acid than unmagnetized ones.

The chemical composition and physical properties of the steel used are given in detail in the paper, together with the methods employed in the investigation, and detailed illustrations of the various apparatus used in course of the research. The results of the investigation are given in detail in Tables I. and II. The whole of the results on Table I. afford an indication that magnetization of comparatively low intensity, acting during considerable periods of time, exerts only a limited modifying influence on the passivity of iron or steel in the cold, though the influence is discernible when employing a delicate galvanometer. Magnetization, with the nitric acid at a higher temperature, produces a quicker effect (see results in Series II., Table II.). In a recent research by the author "On Electro-chemical Effects on Magnetizing Iron, Part II." (Roy. Soc. Proc., vol. xliv. p. 152), it was noticed that local currents were set up between the polar terminals and central portions of steel magnets exposed as electrolytes; and this class of local action, together with the slight alteration of the physical structure of the magnet bars consequent on their magnetization, may possibly be involved in producing the effects due to magnetism on passive steel or iron in conc. nitric acid.

"Observations on Pure Ice, Part II." By Thomas Andrews, F.R.S., M.Inst.C.E.

The experiments contained in the paper form a continuation of a previous research by the author. The experiments were

made to investigate the relative plasticity of pure ice at various temperatures ranging down to − 35° F., and also of pond ice. The arrangements of apparatus used in determining the plasticity of pure ice and pond ice are illustrated in detail in the paper. The ice for the pure ice experiments was frozen from distilled water; the coolest freezing mixture used, consisting of three parts by weight of crystallized calcium chloride and two parts by weight of snow, yielded a constant temperature of − 35° F.; other freezing mixtures were used for the temperatures above this. The cylinders of pure ice employed were 2 feet 1½ inch long, and 2 feet 1½ inch diameter, and weighed 470 pounds. The plasticity was ascertained by measuring the relative penetration, during equal periods of time, of the polished steel rods into the ice, care being taken to avoid errors from conductivity. A large number of experiments were also made on the plasticity of natural lake or pond ice. The influence of the composition of water on the plasticity of the ice frozen therefrom was investigated, and a number of experiments were made to ascertain the proportion of the saline constituents of the lake water taken up into the ice during crystallization. Roughly speaking, it was found that the proportion of inorganic matter in the melted ice was about ten per cent. of the total inorganic salts contained in the lake water from which it was frozen. The general summary of results of the experiments on the plasticity of pure ice at the various temperatures employed are plotted out in four curves on Diagram I., and the results of the experiments on the plasticity of pond ice are shown in detail on Diagram II. In the majority of instances, it was found that, if the plasticity of the ice at − 35° F. be called 1, at 0° F. it would be about twice as much, and at 28° F. the plasticity would be about four times as great as at 0° F., or eight times as much as at − 35° F. The comparatively great contractibility in ice observed at considerably reduced temperatures (see the author's former paper "On Observations on Pure Ice and Snow," Roy. Soc. Proc., No. 245, p. 544) may probably account for the great reduction in its plastic properties at low temperatures. This is in accord with the practical cessation of motion in glaciers during the cold of winter. It was also noticed in course of the research that the plasticity of the naturally frozen pond ice was manifestly greater than that of the prepared pure ice; the comparative difference in the behaviour of the pond ice was doubtless owing to a portion of the saline constituents of the water interspersing during congelation between the faces of the individual crystals of ice, thereby tending to reduce the cohesion of the mass as a whole, and increasing its plasticity.

Linnean Society, June 5.—Prof. C. Stewart, President, in the chair.—The President nominated as Vice-Presidents for the year Messrs. W. Carruthers, P. Martin Duncan, J. G. Baker, and F. Crisp.—Mr. H. Little exhibited and made some remarks upon a photograph of a remarkable Aroid, *Amorphophallus titanum*, which had flowered for the first time in this country.—Mr. James Groves exhibited a specimen of an *Orobanche* parasitic upon a *Pelargonium*.—The following papers were then read and discussed:—On a collection of plants made in Madagascar, by Mr. G. F. Scott Elliot.—On Weismann's theory of heredity applied to plants, by Rev. G. Henslow.—Teratological evidence as to heredity of acquired conditions, by Prof. Windle.—On the development of the tetrosporangia in *Rhabdochorton Rothii*, Naegeli, by Mr. Harvey Gibson.—On the position of *Chantransia*, with a description of a new species, by Mr. George Murray and Miss E. Bass.—On the development of the cystocarp in *Callophyllis laciniata*, by Miss A. L. Smith.—On the cystocarps of some genera of Florideæ, by Mr. J. B. Carruthers.

Royal Meteorological Society, June 18.—Mr. Baldwin Latham, President, in the chair.—The following papers were read:—On the difference produced in the mean temperature derived from daily maximum and minimum readings, as depending on the time at which the thermometers are read, by Mr. W. Ellis. In the publications issued by the Greenwich Observatory authorities, the maximum and minimum temperatures are those referring to the civil day from midnight to midnight. At many stations the observers only read their instruments once a day, viz. at 9 a.m., when the reading of the maximum thermometer is entered to the preceding civil day, and the reading of the minimum thermometer to the same civil day. Such stations are called "climatological stations." The author has tabulated the Greenwich maximum and minimum temperatures according to both methods for the years 1886–89, and finds that the climatological maximum and minimum means are in excess of the civil day means.—On the distribution of barometric pressure at the average level of the hill stations in India, and its probable effect on the rainfall of the cold weather, by Mr. W. L. Dallas. The weather over India during January 1890 was very dry, and in marked contrast to that which prevailed during January 1889. The distribution of barometric pressure was, however, much the same in both months. The author has investigated the records at the hill stations, and has prepared charts showing the distribution of barometric pressure from both high and low level stations. From the high level charts it appears that the mean barometric gradient in 1889 was rather more than twice that in 1890, and, considering what is known of air movements, even at moderate elevations above the earth's surface, it may be assumed that these differences in pressure were accompanied with large differences of air motion; and if it is also assumed that the evaporation over the Southern Ocean is in all years fairly comparable in amount, the deficiency of rainfall over India in the winter of 1889–90 can be attributed to diminished lateral translation of vapour owing to sluggish movements in the upper atmosphere.—On the relative prevalence of different winds at the Royal Observatory, Greenwich, 1841–89, by Mr. W. Ellis. The author gives the following as the average number of days of prevalence of different winds for the 49 years 1841–89, as derived from the records of the self-registering Osler anemometer:—

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
40	45	27	22	35	106	46	22	22

—On some recent variations of wind at Greenwich, by Mr. A. B. MacDowall.—On the action of lightning during the thunderstorms of June 6 and 7, 1889, at Cranleigh, by Captain J. P. Maclear, R.N. The author examined a number of trees which had been struck by lightning during these thunderstorms, and found that those which were struck before the rain fell were shattered, while those which were struck after the rain commenced were simply scored, with the bark blown off. It seems that during rain every tree is conducting electricity, and a disruptive discharge takes place where the conductor becomes insufficient. This depends on the position of the cloud, the amount of foliage on the tree, its condition of moisture, and its connection with running water.

Geological Society, June 4.—Dr. A. Geikie, F.R.S., President, in the chair.—The President referred to the sad loss which the Society had sustained through the death of Mr. Dallas, and read the following resolution, which had been passed by the Council and ordered to be entered upon its minutes:—"The Council desires to record on its minutes an expression of its deep regret at the death of the Assistant-Secretary, Mr. Dallas, which took place on the 29th ultimo, and of its sense of the loss inflicted on the Council and Society by the removal of one who, for the long period of twenty-two years, had done them invaluable service, and who, by his courtesy, kindliness, and helpfulness had endeared himself as a personal friend to the Fellows." It was moved by Dr. Evans, seconded by Dr. Hinde, and carried unanimously, that the resolution passed by the Council be communicated to Mrs. Dallas on behalf of the Society also.—The following communications were read:—As to certain "changes of level" along the shores on the western side of Italy, by R. Mackley Browne.—North Italian Bryozoa, by A. W. Waters.—Notes on the discovery, mode of occurrence, and distribution of the nickel-iron alloy "Awaruite," and the rocks of the district on the West Coast of the South Island of New Zealand in which it is found, by Prof. G. H. F. Ulrich. In an introduction, the author describes the original discovery, determination, and naming of the mineral in 1885 by Mr. W. Skey, and clears up a misunderstanding by which he himself had been credited with the discovery; he furthermore gives a historical sketch of the further investigations and publications referring to the mineral. The geology of the Awaruite-bearing district is described. The rocks consist of peridotites and serpentines, breaking through metamorphic schists with occasional massive intrusions of acid rock. The petrographical characters of the peridotites of the hill-complex, including the Olivine and Red Hill Ranges, and serpentines are considered in detail, and the mode of occurrence of the Awaruite in them and in the sands derived from their denudation is discussed. The author submits a sketch-map of the localities where the mineral has been discovered in sand, including not only George River, but also Silver Creek, Red Hill, and other localities, and quotes Mr. Paulin's belief that it occurs diffused

through the whole extent of peridotite and serpentine rocks, and inferentially in the drifts derived therefrom. The President noted the interest attaching to the gradual development of our knowledge of native iron of terrestrial origin. Prof. Judd was glad to have the present opportunity of removing a misconception that had arisen concerning this mineral. In bringing the matter before the Society on a previous occasion he dwelt upon the facts of special geological interest, and Mr. Skey's name was not mentioned in the few lines placed on record in the Proceedings. No attempt, however, had been made by Prof. Ulrich to claim the discovery of this mineral, though he appeared to have been the first to record its peculiar occurrence in the ultrabasic rocks. In the South Island was the well-known chromite-bearing olivine rocks of the Dun Mountain, but the rock now described was in a distant part of the same island. An interesting series of serpentines derived from peridotites has been sent over by the author, and these specimens contain the "Awaruite." A number of garnets and chlorites, with chrysotile, talc, and magnetite, have been found in the Red Hills. He was not aware that any "Awaruite" had been discovered in the peridotite; but this was probably due to the softer nature of the serpentine, where it could be more easily detected. He had recently heard that one of the serpentines of Norway had yielded a similar alloy.

EDINBURGH.

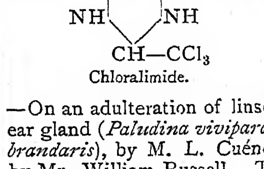
Royal Society, June 2.—Sir Douglas Maclagan, Vice-President, in the chair.—Prof. Crum Brown read a paper on the relation of optical activity to the character of the radicals united to the asymmetric carbon atom. He stated that—if we denote the optically active compound by the symbol C(ΑΒΓΔ)—where C denotes the carbon atom, and Α, Β, Γ, Δ denote the radicals arranged in order of a hitherto undetermined quantity, K—any replacement of one of the radicals by a new one which changes the order as regards K alters the sense of the optical activity. Suppose, for instance, that as seen from Α the values of K for Β, Γ, Δ are in ascending order. If we substitute for Γ an atom whose K is greater than that of Δ, the order will now be Β, Δ, Γ′, which is left-handed if that of Β, Γ, Δ was right-handed. Thus (assuming that increase of *mass* is accompanied by increase of K), he finds that in a number of such compounds the alteration indicated produces a substance in which the direction of rotation of the plane of polarization of light is reversed.—Dr. H. R. Mill read a paper on the mean level of the surface of the solid earth, in which he showed that, from Dr. John Murray's calculations, the general level of the lithosphere was at a depth of 1400 fathoms beneath mean sea-level. More recent explorations show that the oceanic depths are deeper and more extensive than was formerly supposed, so that the mean sphere-level—the surface of a shell which cuts the slope between the elevated and depressed region of the earth's surface in such a manner as to leave a volume of elevated material above it exactly equal to the volume of the depressed region beneath it—appears to lie close to a depth of 1700 fathoms. The contour line of 1700 fathoms of ocean depth divides the earth's surface into two equal areas—one of depression, the other of elevation. This remarkable coincidence shows that the portions of the elevated half of the lithosphere projecting above mean sphere-level would just suffice to fill the hollow beneath mean sphere-level of the depressed half. Dr. Mill also pointed out that round the edge of the great three-armed northern elevated mass the slope to the depressed area was so steep that the outlines of the 1000 and 2000 fathom contours follow the coast-line very closely; but the Antarctic elevation rises from the bed of the depression with an extremely gentle slope.—Mr. J. Crockett communicated an account of Weierstrass's contributions to the calculus of variations.—Mr. John Anderson gave accounts of the recent Louisville tornado, and showed a barometric record made in its neighbourhood. The barometer fell suddenly to the extent of about one-tenth of an inch, and again instantly rose as the tornado passed.

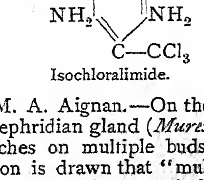
PARIS.

Academy of Sciences, June 16.—M. Hermite in the chair.—On the ordnance survey of France, by M. Maurice Lévy. In commenting upon the work undertaken by the Geodetical Commission, M. Lévy notes that two kinds of documents will be published—one to contain an account of the methods of calculation and corrections which have been employed, as well as the description of the instruments used; the other to be a graphical *répertoire* of the levels determined—and in presenting an account of the first series of operations, some explanation of the work is given.—Theory of the permanent movement produced near the widened opening of a fine tube; application to the second series of Poiseuille's experiments, by M. J. Boussinesq.—Calculation of the successive temperatures in an indefinite homogeneous and athermanous medium which is in contact with a source of heat, by the same author.—On the various isomeric inosites and their heat of transformation, by M. Berthelot. The author finds that in dextrorotatory inosite, dehydrated and having the formula $C_6H_{12}O_6$, dissolved in 300 c.c. of water at 17°·9, the heat absorbed by 1 molecule is − 2·05 calories. Lævorotatory inosite similarly treated gives − 2·03 calories. On mixing the two liquids no rise or fall of temperature was observed. It is therefore concluded that two symmetrical inosites do not show any signs of combination when in solution. Four grammes of neutral inosite were dissolved in 300 c.c. of water at 18°. The rate of solution was slower than that of the active inosites, and the heat developed was − 3·87 calories. This heat of solution is negative and greater than that of either of the active inosites. The three corresponding tartaric acids give the same results.—Variation of the elasticity of glass and of crystal with temperature, by M. E. H. Amagat. It appears from the experiments that the variation increases with the temperature; it is a little greater for glass between 100° and 200° than between 100° and 0°, and considerably greater in the case of crystal; and it seems probable that the variation would increase more and more in value with still higher temperatures.—On a new property of luminous waves, by M. Gouy.—Characteristic equation of hydrogen, by M. Ch. Antoine.—On the variation of temperature with altitude in cyclones and anticyclones, by M. Marc Dechevrens. From a series of observations the law is formulated that "at sea-level and in air, at an altitude less than 1000 or 1200 metres, the temperature in a vortex varies inversely as the pressure, whilst in air at greater altitudes it varies directly as the pressure. In the latter case the temperature has a minimum along the axis of the cyclone and a maximum on the perimeter of the depression; a maximum also occurs along the axis of an anticyclone."—On the combinations and reactions of the gases ammonia and phosphoretted hydrogen with the halogen compounds of arsenic, by M. Besson. The compounds $AsBr_3.3NH_3$; $AsCl_3.4NH_3$; $AsI_3.4NH_3$; and $2AsF_3.5NH_3$ are described, and the products of their decomposition indicated.—On a new method of forming crystalline oxychlorides of the metals; researches on the oxychlorides of copper, by M. G. Rousseau. Among other bodies of the same type, the author has succeeded in obtaining crystallized *atacamite* by his method.—On the combination of phosphorus pentafluoride with nitrogen tetroxide, by M. Emile Tassel. The body formed, $N_2O_4.PF_5$ reacts with water in accordance with the equation—

$$3(N_2O_4, PF_5) + 14H_2O = 2NO + 4HNO_3 + 3H_3PO_4 + 15HF,$$

thus differing essentially from the corresponding compound containing chlorine.—Heat of formation of uric acid and the alkaline urates, by M. C. Matignon.—Chloralimide and its isomeride; a reversible isomeric transformation, by MM. Béhal and Choay. A mixture of chloralimide (B.P. 169°) and isochloralimide (B.P. 103°–104°) is obtained by the action of heat upon chloralammonia; the method of separation and purification of each of these bodies is described. Both bodies possess the same molecular weight, Raoult's method with benzine for solvent yielding the figures—for chloralimide 430, for isochloralimide 434–435. Each body may be transformed by suitable means, given in detail, into its isomeride. The following formulæ are proposed by the authors as representations of these bodies:—


Chloralimide.


Isochloralimide.

—On an adulteration of linseed oil, by M. A. Aignan.—On the ear gland (*Paludina vivipara*) and the nephridian gland (*Murex brandaris*), by M. L. Cuénot.—Researches on multiple buds, by Mr. William Russell. The conclusion is drawn that "multiple buds, one springing from another and being vascularly

connected therewith, ought to be considered as normal ramifications."—On the influence exercised by the time of cutting upon the production and development of shoots from the stocks in underwood, by M. E. Bartet.—Influence of the peritoneal transfusion of the blood of the dog upon the evolution of tuberculosis in the rabbit, by MM. J. Héricourt and Ch. Richet.—On the antiseptic and antipeptic doses of various substances, by M. Andrea Ferranini.

BERLIN.

Physiological Society, June 6.—Prof. du Bois-Reymond, President, in the chair.—Dr. Hagemann gave an account of his experiments on proteid metabolism during pregnancy and lactation; they were conducted upon two dogs supplied with a constant nitrogenous diet. During the first half of the period of pregnancy more nitrogen was excreted than was taken with the food, so that the nitrogen requisite for the growth of the fœtus must have been derived from the tissue-proteids of the mother. After this period the nitrogenous excretion sank to a condition of equilibrium in the middle of pregnancy, and then fell further until the birth of the offspring. Immediately after parturition there was a very marked increase in the excretion of nitrogen, followed by a sudden fall which led to the output being, during four weeks' lactation, less than the in-take.—Prof. Zuntz made a further communication respecting the intestinal fistulæ which he described at the previous meeting of the Society. As regards the absorption of fats and fatty acids, he found that even the finest and most uniform emulsions were not absorbed either alone or with the addition of bile. When saponified, a marked absorption of the soaps took place, but to a much less extent than in normal animals; neither was it increased by the addition of glycerine. The results obtained were, on the whole, negative. The speaker put forward the view that the absorption of fat in the intestine is dependent upon some at present unknown function of the pancreas.

AMSTERDAM.

Royal Academy of Sciences, May 31.—Mr. Max Weber pointed out the characters of a true adult hermaphroditic finch (*Fringilla cœlebs*), caught in the neighbourhood of Amsterdam. The right side of the bird has the plumage of the adult male, the left that of the adult female. The striking difference in the colouring of the plumage on the two sides corresponds to an internal co-existence of ovary and testis: the latter is, on the male-coloured (right), the former on the female-coloured (left) side. Both sexual glands, compared, also microscopically, with the testis and ovary of normal finches, are anatomically wholly normal, and able to produce male and female sexual elements. The case seems to be an illustration of the dependence of sexual colouring upon the nature of the sexual gland.—Mr. van Bemmelen stated that Mr. Molengraaff had sent him a white substance found in the high moor of Drenthe (Netherlands), denominated by the moor-diggers as *White Klien*. It consists of 87 per cent. carbonate of oxydulated iron, 6 per cent. carbonate of lime, and 8 per cent. vegetable matter.—Prof. Hubrecht gave a description of the early developmental stages in the shrew. In the two-layered blastocyst the mesoblast makes its appearance: (*a*) from the hypoblast under the anterior portion of the epiblastic shield; (*b*) from the primitive streak and its anterior prolongation; (*c*) from an annular zone of hypoblast below, but just outside the border of the epiblastic shield. The mesoblast from these three sources very soon fuses into one continuous plate. There appears to be considerable agreement between the facts as presented by the shrew and those which Bonnet has described for the sheep. The gastrulation process in the Mammalia was then comparatively considered, and a theoretical interpretation put forward, differing considerably from E. van Beneden's latest hypothesis.

STOCKHOLM.

Royal Academy of Sciences, June 11.—Spiders from the Nicobar Islands and other parts of Southern Asia, mostly collected during the voyage of the Danish war-ship *Galatea* in the years 1845–47, described by Prof. T. Thorell.—On the remains of a fish preserved since the year 1289 in the cathedral of Wisby, and often mentioned in the old chronicles as a remarkable curiosity, by Prof. F. A. Smitt.—Étude des conditions météorologiques à l'aide de cartes synoptiques répresentant la densité de l'air, par Dr. N. Ekholm.—On an expedition which has just started for Spitzbergen, by Baron A. E. Nordenskiöld. This expedition consists of some young Swedish naturalists who propose to make geological and zoological researches.—On the fungi of Omberg and its neighbourhood in Ostrogothia, by Herr L. Romell.—On the different kinds of vegetation on the surface of the peat bogs of Southern Sweden, by Herr G. Andersson.—Dendrological studies made in several Swedish provinces, by Herr F. Laurell.—On the vegetation of Norrbotten, by Dr. A. Lundström.—Botanical rambles in the south-west of Jemtland in the summer of 1889, and description of some Hieracia and Carices found, by Dr. M. Elfstrand.—On the oxidation of the phenyl-methyl-triazol-carbon acid, i., by Dr. J. A. Bladin.—On some ammoniacal platina combinations, by Dr. O. Carlgren.—Critical remarks on the history of the vegetation of Greenland, by Prof. A. G. Nathorst.—Studies on the Turbellaria and Nemertinæ of the northern countries, by Dr. D. Bergendal.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Catalogue of Stars observed at the U.S. Naval Observatory during the Years 1845–77, 3rd edition: Prof. M. Yarnall and Prof. E. Frisby (Washington).—Father Perry, F.R.S.: A. L. Cortie (Catholic Truth Society).—Travels in Africa during the Years 1875–78: Dr. W. Junker, translated by A. H. Keane (Chapman and Hall).—Nitrogen: its Uses and Sources in Agriculture: C. M. Aikman (Glasgow, Wright).—A Handy Guide to the Birds in the Bootle Museum: J. J. Ogle (Bootle).—Record of Experiments in the Production of Sugar from Sorghum in 1889: H. W. Wiley (Washington).—A Revised Account of the Experiments made with the Bashforth Chronograph to find the Resistance of the Air to the Motion of Projectiles: F. Bashforth (Cambridge University Press).—Selected Subjects in Connection with the Surgery of Infancy and Childhood: E. Owen (Baillière).—The Triumph of Philosophy: J. Gillespie (Dumfries).—A Hand-book of Descriptive and Practical Astronomy; III. The Starry Heavens, 4th edition: G. F. Chambers (Oxford, Clarendon Press).—Die Pflanzen und Thiere in den Dunklen Räumen der Rotterdamer Wasserleitung: H. de Vries (Jena, Fischer).—Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere: Dr. O. Hertwig (Jena, Fischer).—Oxford and Modern Medicine: Sir H. W. Acland (Frowde).—The Quarterly Journal of Microscopical Science, June (Churchill).

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THURSDAY, JULY 3, 1890.

LIFE OF SEDGWICK.

I.

The Life and Letters of the Reverend Adam Sedgwick, LL.D., D.C.L., F.R.S., Fellow of Trinity College, Cambridge, Prebendary of Norwich, Woodwardian Professor of Geology, 1818–73. By John Willis Clark, M.A., F.S.A., and Thomas McKenny Hughes, M.A., F.R.S. Two Volumes. (Cambridge: University Press, 1890.)

BETTER late than never! Geologists have waited for seventeen years for a life of Sedgwick, though the biographies of Murchison, Lyell, and Darwin, two of whom survived him, have all been published. The delay, as is admitted in the preface, requires some explanation: whether that is really furnished may be doubted. This at least is clear, that it has not been due to Mr. Clark, since he only undertook his portion of the work, and that the major one, in 1886. The delay is the more to be regretted because not a few of those who could remember Sedgwick in the days of his full vigour have passed away, and, as Mr. Clark observes, "a number of interesting letters which he is known to have written, and which were long carefully preserved, have either been destroyed or cannot now be traced. These remarks apply specially to the earlier years." Still, Mr. Clark has had at his disposal a large amount of material, from which he has drawn a picture no less vivid than accurate—as we feel sure those who knew the original will admit—of a man of remarkable genius and almost unique personality. He has told us the story of Sedgwick's life, he has woven into it Sedgwick's letters, and the result is a book which is worthy to be classed with the two best biographies, at any rate of recent date, of distinguished sons of Cambridge—those of Charles Kingsley and Charles Darwin.

This book has its value as a chronicle of the development of geology into a distinct and independent branch of science, but this is not its only interest. True, it is a record of a life comparatively uneventful. It was not often that Sedgwick's geological studies conducted him beyond the limits of the British Isles. His Continental journeys were restricted to the western half of Europe, and did not include Spain or Scandinavia, but his friends were numerous and notable. His life extended over a period of our national history of unusual interest. He remembered vividly the great incidents of the "struggle for life and death with France." He heard the death peal rung for Nelson and for Wellington: he had shared in the domestic strife of the Reform Bill, and had witnessed the blunders of the Crimea and the peril of India. His sympathies were as quick as they were wide, and he was not only a frequent letter writer, but also a master of that almost forgotten art. Hence these volumes contain much that will be interesting to others than geologists. They are the record, not of a life devoted solely to one special study, but of a man of varied interests and rare enthusiasm, of unusual eloquence and exceptional descriptive powers. Not the least valuable part of the work is Sedgwick's own account (extracted from a privately printed pamphlet) of the manners and customs of the dalesmen of the Sedbergh district, among whom he was born, whither he constantly returned, and which he loved to the last hour of his life.

This book brings before us Sedgwick as a man and as a geologist, a division which corresponds with the work of its joint authors. Though the two characters made up the one personality, and a distinction between them must be to some extent arbitrary, this may be adopted, as a matter of convenience, in endeavouring to give some idea of the varied contents of these volumes.

Adam Sedgwick was born in the year 1785, the fourth child of the Rev. Richard Sedgwick, vicar of Dent, an old-world village, by a tributary of the Lune, among the great hills of Western Yorkshire. He was a member of one of the families of "statesmen" which had been settled in Dent for more than three centuries. Till he was sixteen years old he was taught at the Grammar School, partly by his father; then he was sent to school at Sedbergh; thence he went, in his twentieth year, to Trinity College, Cambridge, after a few months' tuition by John Dawson, a country surgeon (he had ushered Sedgwick into the world) who had become eminent as a mathematician, as teacher no less than as investigator. Sedgwick's work at Cambridge was interrupted by an attack of typhoid fever which nearly proved fatal, but, notwithstanding this, he obtained a scholarship in his College, and the fifth place among the Wranglers in the Mathematical Tripos of 1808. Private pupils and reading for his Fellowship employed him for the next two years, the latter being obtained in the year 1810. The double work proved a severe strain to Sedgwick's constitution. The great importance which has always been attached at Trinity College to the examination for fellowships has its advantages and its disadvantages; the one as affording an opportunity for remedying ill-fortune at the time of the degree and widening the field of choice; the other as giving an advantage to the wealthy, and pressing heavily on those who must combine work for a living with study for an examination. Not a few of the latter have paid for success by permanent injury to health. Among these, it appears, Sedgwick must be reckoned. During the next three years he was out of health, and in 1813 came a complete breakdown. Consumption was apprehended; but at last his naturally strong constitution triumphed, and he was able to return to Cambridge and take part in the regular tuition of the College. In 1816 he was ordained. Three years later came the great crisis of his life—the Woodwardian Professorship of Geology, hitherto little more than a sinecure, became vacant, and Sedgwick declared himself a candidate. His prospects of success at first did not seem great, for he had little, if any, knowledge of the subject, and was opposed, not only by a member of his own College, but also by the Rev. G. Gorham, of Queens' College, who was reputed to have devoted much attention to geology, though he does not appear to have published anything. But the opponent from within the walls of Trinity retired, and then Sedgwick had an easy victory over the other. Cambridge—perhaps Oxford also—has often been rather eccentric in her elections to professorships, and prone to act on the maxim "*Omne ignotum pro magnifico.*" But on this occasion the leap in the dark was more than justified.

Neither of Sedgwick's opponents afterwards made any name as geologists, though the second of them lived to fight a battle for religious freedom in the Church of England.

At once Sedgwick threw himself heart and soul into his subject. Geology at that time signified little more than an excrescential growth from mineralogy, which became the less scientific the further it departed from its support. Still, the Geological Society of London had already been founded full ten years; and the men were now hard at work who were to roll away the reproach from geology, and lay its foundations on the sure ground of observation and induction. Neptune had failed to extinguish the torch of Pluto, and the Wernerians were retreating before the Huttonians; William Smith had already published his wonderful maps, and had set in order, almost single-handed, the newer rocks of England; but below the base of the Carboniferous system a great field for research still remained, in which the generation of Sedgwick's more immediate contemporaries were destined to win their laurels.

Sedgwick's first geological journeys were in Derbyshire and Staffordshire, in the Isle of Wight, and on the coast of Suffolk. But the learner quickly became an investigator. Even in 1818 he began to attack the problems presented by the older rocks of the south-west of England; thence he turned aside to examine Eastern Yorkshire and Durham. Difficult problems seemed from the first to have for Sedgwick a peculiar fascination, and in 1822 he grappled with those presented by the Lake District. In 1827 began his association with Murchison, whom he accompanied in a geological tour to Scotland, and joined in a paper on the results. The following summer saw them companions in their notable researches in Germany and the Tyrol, which produced another joint communication. By this time Sedgwick's merits had been recognized by his election to the Presidency of the Geological Society.

The year 1831 brought two important crises in Sedgwick's life—the one the offer of a valuable living from the Lord Chancellor, the other the beginning of his work in North Wales. That offer he declined, making a mistake, as several of his friends thought—an opinion to which his biographer inclines. Probably Sedgwick would have been a healthier man in a country rectory—for the climate of Cambridge was not suitable to him—and a happier man in married life. But science, we think, would have lost. It might not have been so with some men, but it was Sedgwick's nature to throw himself with all his heart into whatever work he undertook; so that in all probability the interest felt for his parishioners and his home circle would gradually have extruded geology from his thoughts. In this case science would have had to wait some time for the unravelling of much complicated stratigraphy; the collections of the Woodwardian Museum might have remained in a comparatively impoverished condition, and the University would have lost the quickening action of Sedgwick's influence on generations of its students.

Next year Sedgwick took a new departure in authorship. A College Commemoration sermon, which he had been asked to print, increased under his hands, with a prefatory head and a commentarial tail, till the "Discourse on the Studies of the University of Cambridge" expanded into a book, and became, as he phrased it, "a grain of wheat between two millstones." In 1834 he was made a Prebendary of Norwich, a preferment which, though its duties often seriously interrupted his scientific work, was a welcome addition to his income, which up to that time had hardly sufficed for the numerous calls upon it.

For the next six years his work in the field was less and his papers rather fewer; henceforth interruptions obviously became more frequent. The rearrangement of his fine geological collections, for which at last a museum had been provided; political incidents, in which he took an active interest; visits to and from distinguished friends, which became more numerous as his fame increased—all these proved, as they always prove, detrimental to work which requires steady and continuous application. But as the scientific interest of the book wanes a little, its general interest increases. Graphic sketches of notable personages appear more frequently in Sedgwick's letters, which come nearer to being a journal of his life. They bring out also—for many of them are written to young folks—all the tenderness of his nature: they intersperse fatherly advice with accounts of his doings, now grave, now comic. One moment he pulverizes a scientific foe; the next, gives his niece a ludicrous lesson on the pronunciation of Welsh. The election of Prince Albert to the Chancellorship of the University of Cambridge, in which Sedgwick took a leading part, still further interfered with his devotion to science, for it led to his acting as the Prince's secretary in Cambridge, and holding a place on a Commission for the Reform of the University. This, however, is a gain to the book, for his private letters give many interesting details of the Royal visit to Cambridge, and especially of the home life of the Queen and Prince Albert at Osborne.

After a time, about the year 1851, the Silurian question, presently to be noticed, spurred Sedgwick into renewed activity in his old field of work, but led to the unhappy result of his alienation from Murchison and his estrangement from the Geological Society. The burden of years, however, was now beginning to make itself felt, for in 1855, when he reviewed the controversy in his introduction to McCoy's "Description of British Palæozoic Fossils," he attained the age of threescore and ten. Henceforth the path of his life became sadder; one by one friends passed away, the infirmities of age increased, and though at times the old fire flashed up, and for a while the racy phrase and eloquent speech would return, he now felt, as most must feel, something of the *pœna diu viventibus*. Still he was able, up to about 1863, to take occasionally an active part in passing events, though more and more he was compelled to avoid excitement and fatigue, and thus his life at Cambridge was often lonesome. During the last ten years he sometimes suffered severely; almost he might have described himself as "sans teeth, sans eyes," sans ears, though happily not "sans everything," for the mind, as his letters show, continued unclouded, though, of course, sometimes that memory, once so marvellously retentive, failed a little. No part of the book is more tender, none more sympathetic, than the account of Sedgwick's last years. Early in 1873 came the closing scene, in the rooms in Trinity, which had been for so many years his chief home.

"There was no change till about midnight," writes his niece, "and then we saw the shadow of death come softly over his face, and we knew that he had passed into the dark valley, and that the end was near; but there was no pain; only quiet sleep. His breathing again grew more faint and soft; and without a sigh, just as the clock in the great court of Trinity chimed a quarter past one, his spirit returned to God."

Sedgwick's original scientific work will be sketched in another notice. This may conclude with a word on the man himself. A stalwart figure with rugged features and brown complexion, a flashing eye, and a grand pose of the head, which always reminde me of an eagle. He called himself—men called him—ugly. This I never could understand. Few were better tellers of a story: his memory of striking details, his sense alike of humour and of pathos, were so strong. As a lecturer he was discursive, but suggestive—one who stimulated and fertilized rather than who trained. His speeches were marked by a curious play of fancy, unexpected transitions from grave to gay, and occasional bursts of eloquence, which our greatest orators might have been glad to own. As a writer he was often diffuse, sometimes laboured—the results of hurried work or unsystematic arrangement; yet he broke out occasionally into passages of singular force and vigour. For instance, the concluding paragraphs of his preface to the "Catalogue of Cambrian and Silurian Fossils"—his last contribution to literature—are worthy, in my judgment, of a place among the best extracts from English literature. He was sometimes strong and even narrow in his prejudices, as will appear hereafter; he was impetuous in temper, fierce in the fray, positively ripping up an incompetent antagonist; yet he was commonly the most genial and placable of men; he was tender as a woman to those who sorrowed and who suffered, and was the idol of little children.

We may close the present notice with the words with which Mr. Clark concludes his own part of the biography—the words of one of Sedgwick's intimate friends:—

"He was transparent and straightforward—the very soul of uprightness and honour—tender and affectionate—most generous and kind. He had a hatred of all duplicity and meanness. He was entirely unsuspicious of evil, unless it was forced upon his notice; and he expected and believed everyone to be as straightforward and truthful as he was himself. I do not think that any man was so beloved by his friends as he was."

T. G. BONNEY.

(*To be continued.*)

GÉRARD'S "ÉLECTRICITÉ."

Leçons sur l'Électricité, professées à l'Institut Électrotechnique Montefiore annexé à l'Université de Liége. Par Eric Gérard, Directeur de cet Institut. (Paris: Gauthier-Villars, 1890.)

THE author of this book says in his preface that when he took charge of the classes in electric technology at Liége he felt the want of a text-book which would give a clear and definite account of electrical phenomena without requiring more extensive mathematical knowledge than his pupils might be expected to possess. We think that in this respect the experience of most teachers of electricity will coincide with that of M. Gérard. There are very few text-books on electricity in which the happy mean between utter vagueness and methods requiring the use of high mathematical knowledge has been hit; this, however, has been done so successfully in the book before us, that we think the difficulty to which we have just alluded will be almost removed. In this book we have the main outlines of electricity explained in language at once intelligible and precise, and without introducing more mathematics than every student of the subject ought to be competent to follow. In a subject like electricity, where forces have to be compared, the geometrical properties of bodies of various shapes utilized, &c., it is evident that if any numerical results at all are to be attained, some mathematics must be introduced; the question as to how much mathematical knowledge should be expected of students who, as a working hypothesis, may be assumed not to have any special aptitude for that study is one on which opinions will differ. For our part, we think that, even regarding it solely from the point of view of the engineer or physicist, such students ought to be advised to acquire an elementary knowledge of the differential and integral calculus; the possession of this knowledge will make many parts of the subject easy which without it would be difficult, and the time spent in acquiring the mathematics will be much more than saved in the time spent over the physics. In the book before us the mathematics are as plain and straightforward as possible. At the same time, M. Gérard, very wisely we think, does not scruple to use the elements of the differential and integral calculus.

The work contains more than 500 pages, of which about 200 are devoted to the theory of Dynamos. The remainder consists of an exceedingly clear and accurate description of electrical phenomena, the subject throughout being treated from Maxwell's point of view. The book is brought well up to date, and contains an account of most of the recent researches in electricity and magnetism; we think, however, it would have been improved by references to the places of publication of the original papers in which these researches are described, so that a student who wishes for a more detailed description than could be given in an elementary text-book might be able to refer to the original authorities for himself.

A most excellent feature of the book is that M. Gérard does not treat the subject as if an investigation was complete when it had led to a relation between a number of symbols. He applies the equations he gets to actual cases, and thus familiarizes the student with the magnitude of the quantities with which he is dealing. He commences the book with Sir William Thomson's maxim, which is so excellent in physics, though its application to other subjects might possibly cause consternation, that "we cannot understand a phenomenon until we can express it in numbers," and he acts up to the spirit of this maxim all through the book.

The book is well and clearly printed, and the author has realized the fact that it is more important that the diagrams in a text-book of physics should be explanatory than that they should be elegant.

There are one or two points which we think might be corrected in a new edition, which we are sure will soon be required. The deformation of dielectrics under elec-

tric forces, which is cited as a proof of Maxwell's theory of stress in the medium, is rather an obstacle than a support to the theory, as some dielectrics are strained in one way, and others in the opposite, while, on Maxwell's theory, the strain should all be of one kind. The statement on p. 97, that the sparking distance increases very much more rapidly than the increase in the difference of potential between the electrodes, should have been limited to the case where the electrodes are pointed; it is not true when the dimensions of the electrodes are large compared with the sparking distance. The proof of the expression for the electromotive force due to induction, on p. 170, does not seem to us to be sound; and the method of measuring the coefficient of self-induction of a coil was really invented by Maxwell, and given by him in his paper on the "Dynamics of the Electric Field," though it is not in the "Electricity and Magnetism."

We must, in conclusion, congratulate the author on having written one of the best treatises on elementary physics which it has ever been our good fortune to read.

J. J. T.

THE ART OF PAPER-MAKING.

The Art of Paper-Making. By Alexander Watt. (London: Crosby Lockwood and Son, 1890.)

THE author of this work, in the preface, expresses his thanks to certain gentlemen who have been good enough to conduct him through their mills and explain to him the various operations performed therein. From this we gather that the author is not only not a practical paper-maker, but that, up to the time of writing the book, he had but a limited and general knowledge of the subject. These conclusions are amply justified by a perusal of the book. This want of practical knowledge can hardly be wondered at, as the writer is already an authority on such widely different subjects as soap-making, leather manufacture, electro-metallurgy, electro-deposition, &c.

On the other hand, there is evidence that on the whole the author has devoted some considerable time to the reading up of his subject, though in many cases he has not consulted the latest authorities. For example, in speaking of the properties of cellulose, he quotes the opinions expressed by Mr. Arnot in his Cantor Lectures for 1877, since which time several additions have been made to our knowledge. We should have preferred to see more space devoted to this branch of the subject, as on the proper understanding of the properties of cellulose the scientific manufacture of paper depends.

Some of the statements with regard to cellulose are inaccurate and misleading, as for example, that "hydrochloric acid converts it into a fine powder without altering its composition," and again, that "nitric acid forms substitution products of various degrees, according to the strength of acid employed." As a matter of fact, ordinary nitric acid does not form nitro-substitution products with cellulose.

Under the head of the "Recognition of Vegetable Fibres by the Microscope," esparto—perhaps the most important raw material used in this country—is not even mentioned. The author's descriptions of the various mechanical appliances used in paper-making are, with one or two exceptions, accurate and fairly complete. In describing the chemical processes involved, however, the author occasionally gets out of his depth. For instance, he recommends certain qualities of rags to be boiled with 30 per cent. of caustic soda. At first we thought this was a misprint for 3 per cent., but on referring to the source of the information, we found that the author had quoted correctly. Again, we are told that the neutralization of chlorine in pulp by hyposulphite, which the author says is sometimes called thiosulphite, is effected when the liquor ceases to redden litmus paper.

In giving directions for the sizing of paper, the author appears to have left out a number of decimal points. According to him 100 parts of pulp require 10–12 parts of rosin, and 20–30 parts of starch, and from 30–50 parts of kaolin. In the interest of the consumer it is satisfactory to know that such numbers are impossible.

In the chapter containing directions for the testing of alkalies, alum, &c., the following extraordinary statement occurs: "There are two principal methods of analyzing or assaying alkalies by means of the test acid—namely, volumetric, or by volume, and gravimetric, or by weight, in which a specific gravity bottle, capable of holding exactly 1000 grains of distilled water, is used."

Another instance of looseness of style occurs in the statement that "the proportion of caustic soda per cwt. of rags varies to the extent of from 5 to 10 per cent. of the former to each cwt. of the latter."

The general plan of the book also shows want of careful preparation; for example under the head of "Action of Acids on Cellulose," the author discusses the action of the strongly alkaline solution of cuprammonium.

In speaking of the origin of the wood-pulp process, the author champions the right of his father to be regarded as the pioneer. Similarly, with regard to electrolytic bleaching, we are told that the modern Hermite process, which has been successfully applied to the bleaching of paper pulp, is the outcome of an invention patented by his brother in 1851. It is perfectly true that in this patent the electrolysis of chlorides was claimed, but this in no way diminishes the credit due to those who have based on this principle a practical and successful manufacturing process.

OUR BOOK SHELF.

A Contribution to the Natural History of Scarlatina, derived from Observations on the London Epidemic of 1887–88. By D. Astley Gresswell, M.A., M.D. Oxon. (Oxford: Clarendon Press, 1890.)

THIS volume constitutes Dr. Gresswell's dissertation for the degree of Doctor of Medicine at Oxford, and is published "as a mark of distinction" by the University. It is the result of some six months clinical work at the South-Western Fever Hospital of the Metropolitan Asylums Board, and the author is to be congratulated alike upon the large number of carefully recorded observations which he has made, and upon the evidence his book affords of his careful study of the literature of scarlatina.

Between September 1887 and February 1888 Dr. Gresswell had charge of nearly 600 fever patients, and the statistical tables with which his treatise abounds are thus based on no inconsiderable number of cases. After some

general considerations with respect to the disease, certain special symptoms are passed in review, particular attention being devoted to their relation to season. Then follows an exhaustive discussion of scarlatinal albuminuria. Perhaps the most striking fact brought out in the book is the contrast presented in regard to this symptom between the patients admitted during October and November, and those admitted during December and January. While albumen was discovered previous to "getting up for the first time" in only rather more than 50 per cent. of the latter group of cases, it was found in every such case investigated in the two earlier months. This universal occurrence of albuminuria in the first three weeks of the disease, during the height of the epidemic, is eminently noteworthy; as Dr. Gresswell says, it could not have been "a mere casual incident of pyrexial origin," nor could he account for it by differences of sex, age, stage of illness on admission, or treatment. All observers of the scarlatina of the latter half of 1887 seem to have been impressed with the unusually frequent occurrence of albumen in the urine. Dr. Sweeting referred it to overcrowding; Dr. Gresswell inclines to consider it as accounted for by the change in the character of the disease during the progress of the epidemic.

Although the chapter on "postural albuminuria" is of considerable interest, much of its subject-matter is not immediately connected with the natural history of scarlatina, while an important question like secondary sore throat is very briefly dealt with. Two cases of "reversio eruptionis" are quoted, but in one, as Dr. Gresswell admits, there is but scant evidence that the child had scarlatina on admission.

Attention is particularly devoted throughout to the variations in the phenomena of the disease in their relation to season, and the concluding section of the work contains some interesting suggestions with regard to this topic. The author upholds the view that variations in the life-history of the micro-organism of scarlatina lie at the root of the matter, but surely he goes rather far afield when he alludes to the possibility of the "interfertilization of different kinds of microbes."

The hope may be entertained that Dr. Gresswell will not lack imitators in selecting this particular branch of study as the subject of dissertation for the M.D. degree. There is abundant scope for research at the Asylums Board hospitals, and if the work be as full of interest as it is in the example before us, it cannot fail to redound to the credit of the worker.

Le Soleil; les Étoiles. By Gabriel Dallet. (Paris: Firmin-Didot et Cie., 1890.)

THE chapters on the constellations, in this work, are of a very comprehensive character. That devoted to a description of instruments of observation contains a fair amount of useful information, whilst tables of parallaxes and proper motions, double and variable stars, and other interesting objects visible in our hemisphere, compiled from the British Association Catalogue, *Connaissance des Temps*, and *L'Annuaire du Bureau des Longitudes*, are plentifully and properly distributed throughout, and render the work what it purports to be, an "Astronomie Pratique." The author is, however, evidently not at home when writing on spectroscopy, and is considerably behind the recent developments in that branch of astronomy. As an example of this deficiency we would cite his assertion that the spectrum of the Orion nebula consists of three bright lines, as discovered by Dr. Huggins in 1864, although recent observations have increased the number visible to nine, and the photographic spectrum shows many times this amount.

The author seems also to have very vague ideas as to the origin of the universe. He says:—"Notre soleil et ses planètes ont dû se trouver au centre d'une nébuleuse, mais la matière cosmique qui la formait comprenait une variété considérable d'éléments chimiques qui ne se présentent pas dans nébuleuses proprement dites"; a conclusion which leads him to write:—"Nous pouvons dire avec M. Huggins que les nébuleuses à spectre gazeux sont des systèmes ayant une structure et une organisation à part, et qui sont d'un ordre différent de celui dont notre soleil, avec ses planètes, faisait partie dans la nébuleuse primitive"; although in justice to Dr. Huggins it should be said that he has now rejected the conviction that "the nebulæ which give a gaseous spectrum are systems possessing a structure, and a purpose in relation to the universe, altogether distinct and of another order from the great group of cosmical bodies to which our sun and the fixed stars belong."

Little spectroscopy other than this is included in the work, observations of sun-spots and prominences being mainly considered from a pictorial point of view. There is no doubt, however, that the twelve maps of the heavens will be of service to amateur astronomers, and that the ninety-three illustrations are in general well chosen. We should be glad, therefore, to see the slight inaccuracies that we have indicated eliminated in a future edition.

Father Perry, F.R.S. By Aloysius L. Cortie, S.J. (London: The Catholic Truth Society, 1890.)

THE author of this little book was a friend of the late Father Perry, and is, therefore, most capable of writing a sketch of his life and work, and few lives could afford more of the material which makes such a sketch interesting.

The programme of work undertaken by the deceased astronomer ten years ago at Stonyhurst College Observatory was comprehensive. It included the daily drawing of the sun when possible, the measurement of the depth of the chromosphere, the heights of prominences, and observations of sun-spot spectra, and this programme was faithfully adhered to up to the time of his death. The method of obtaining the drawings of sun-spots which have appeared in the Memoirs of the Royal Astronomical Society is described, and the reproduction of two of the largest spots shows how much can be effected by means of the pencil. These drawings are of great importance, and supplement solar photographs. The main object in making them was to throw light upon the theories of the mode of formation of spots, and to find, if possible, the clue to the connection between terrestrial magnetism and solar activity. This discussion however, was cut short by death.

A copy of the photograph of the solar corona, from the observation of which Father Perry was carried to his death-bed, testifies more than volumes of words to the character of the man whose life is before us, and the long list of published papers and the expeditions in which he took part speak of his industry. A few of his notes on faculae and veiled spots are appended, and render this volume of 112 pages something more than a biography.

Prodomus Faunæ Mediterraneæ sive Descriptio Animalium maris Mediterranei incolarum quam comparata silva rerum quatenus innotuit adjectis locis et nominibus vulgaribus eorumque auctoribus in commodum Zoologorum congessit Julius Victor Carus. Vol. I. Pars II., Vol. II., Pars I. (Stuttgart: E. Schweizerbart'sche Verlagshandlung.)

SOME five years ago we welcomed the appearance of the first part of Prof. J. Victor Carus's "Prodomus Faunæ Mediterraneæ" (NATURE, vol. xxxi. p. 201); since then, two additional parts have been published. The second completes vol. i., and contains the Arthropods; it was published early in 1885. The third part, the first of vol. ii., was published late in last year, and contains the Brachiostomata and Mollusca.

Beyond the record of the appearance of these parts, and the expression of our hope that the author will speedily hasten the completion of his work, the usefulness of which will be greatly increased thereby, we have nothing to add to our previous notice.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Spiny Plants in New Zealand.

In Mr. Wallace's recent work on "Darwinism," reference is made to the absence of spiny and prickly plants in oceanic islands in disproof of Prof. Geddes's theory that spines are an indication of the ebbing vitality of a species. Mr. Hemsley's remarks on the subject are quoted, and an explanation of the occurrence of spines in our only species of *Rubus* and in *Aciphylla* is given. In regard to the former it is stated (p. 433, colonial edition):—"In New Zealand the prickly *Rubus* is a leafless trailing plant, and its prickles are probably a protection against the large snails of the country, several of which have shells from two to three and a half inches long." The explanation seems to me to be a very unsatisfactory one, and indeed to be quite incorrect. The snails referred to (*Placostylis bovinus, Paryphanta Busbyi,* and *P. Hochstetteri*) are very uncommon; I do not know that they occur at all in the South Island. The *Rubus*, on the other hand, is everywhere a most abundant and aggressive plant, springing up especially in bush clearings, whether made by fire or by the axe alone. It is also incorrect to speak of it as a *leafless* trailing plant. Sir Joseph Hooker, who is the first authority on the New Zealand flora, has united all the forms of *Rubus* found in these islands into one polymorphic species, and even the most inveterate species-makers have never yet successfully disputed his dictum. It must, however, be acknowledged that *four* if not *five* very distinct varieties are included under the common name of *Rubus australis.* Of these only the variety *cissoides* of the "Flora Novæ-Zealandiæ" is leafless, its leaves being reduced to prickly midribs. All the other forms are leafy, some densely so, and these are by far the most abundant.

The true explanation of the prickles is most probably that they serve as climbing organs. No doubt all the developments of the epidermis in the larger species of the genus *Rubus* served primarily for protection against grazing animals. This is evidently the case in the common raspberry. But even in the various forms of the European blackberry or bramble, the prickles seem to help the plants in their scrambling growth to overtop those shrubs among which they grow. This is very evidently the case with our New Zealand bramble, or "bush-lawyer," as it is suggestively termed. It is a plant which grows especially at the edges of the bush or in clearings, and it quickly climbs over the plants among which it lives. If we take hold of a petiole (the stems have no prickles) we find it provided on the under side with a line of strong prickles, all curved downwards so as to give them good holding power. Their catch is further improved by the sharp bend in a direction opposite to their curvature which the petiole and petiolules take. One has only to attempt to pull a "lawyer" down from the plant on which it is climbing to see that the snail-hypothesis is not the correct one.

Any explanation of the formidably spinous leaves of *Aciphylla* is at the best hypothetical. Perhaps the theory that they may have gained their spines to prevent them from being trodden down or eaten by the Moas, is as good as any other. In a paper on the origin of the New Zealand flora, published in vol. xiv. of the Transactions of the N.Z. Institute, I have made reference (p. 496) to the scarcity of spiny and prickly plants. It is there shown that in cases where such defensive modifications occur the plants are either wide-spread in their distribution, having probably, before spreading into these islands, acquired their characters in other regions where they were of service; or that they belong to genera having extensive distribution. I have also pointed out that in pungent-leaved plants, such as species of *Leucopogon, Archeria,* &c., the strictly endemic species have lost the pungent tips. The same remark holds as to the barbed spines of *Acæna*, which serve to distribute the seeds, probably by mammalian agency. Of the seven described species two have a wide distribution outside of these islands, and have strong barbs. Two endemic species have the barbs not so well developed, while in the other three species—also endemic—they are wanting altogether.

Can anyone offer any suggestion as to the formidable nature of the stinging-hairs of our common nettle—*Urtica ferox?* In *U. incisa* and *U. australis* the stinging-hairs are few in number and feeble in their urticating properties. But *U. ferox* is a species confined, as far as I know, to these islands, and it has developed a formidable array of large and very poisonous hairs. It is worthy of remark that though so strongly protected in one direction it is particularly liable to insect attacks, it being often very difficult to find a perfect leaf. I cannot suggest any adequate explanation.

Geo. M. Thomson.

Dunedin, N.Z., May 14.

Drowned Atolls.

As Captain Wharton speaks of the Macclesfield Bank as the so-called "drowned atoll" of the China Seas, it may be interesting to note that in the recent survey of it there were found no less than 15 genera, including 27 species, of living corals growing in depths from 21 to 44 fathoms, the dredge at each haul always bringing up living specimens, and of these only four were found growing on the more shallow rim of the Tizard Bank.

P. W. Bassett-Smith.

As my opinion is that all the submerged atolls are in vigorous growth, I concur, of course, in Mr. Bassett-Smith's view, in the paragraph above, that the term "drowned," as indicating "dead," is a misnomer; and I inserted the words "so-called" to express this. The examination of the Macclesfield and Tizard Banks strongly supports this view.

W. J. L. Wharton.

The Essex Bagshots.

Mr. H. W. Monckton has done good service in calling the attention of geologists to the section through Brentwood Common (Nature, vol. xlii. p. 198); and I am glad to say that I entirely agree with the interpretation of the section which he has suggested. The classification of all these beds as "Lower Bagshot" is in fact but a repetition of the error committed in former years in the Newbury country (see *Q.J.G.S.*, vol. xliv. pp. 178, 179). Lithological and palæontological evidence now concur to prove what seemed to me in the highest degree probable when the discovery of fossils in the Bagshot Beds at Frierning was announced in the new edition of the "Geology of London" last year, and what I suggested on general grounds three years ago (see *Geological Magazine*, March 1887, p. 115); namely, that in the Essex area there is an attenuation of the lower sands implying a transgressive relation of the "Bagshots" to the London Clay, such as has been shown by me (*Q.J.G.S.*, vols. xliii. and xliv.) to occur along the northern margin of the Bagshot area from Englefield Green to Farley Hill, south of Reading.

A. Irving.

Wellington College, Berks, June 30.

A Remarkable Appearance in the Sky.

The remarkable appearance in the sky noted by your correspondent in Nature of June 26 (p. 198), as observed in Sussex, on night of 17th inst., was also well seen here. I enclose sketches which afford an approximate idea of the phenomenon as observed on both the 17th and 25th inst. The former was the first conspicuous occurrence here this season of those "luminous boreal night-clouds," of which sketches have been forwarded to Nature by the writer for some years past, but from another locality of residence at a higher elevation. This may account for failure of earlier observation during the present year. The luminous forms have become less definite, the outlines being faint and nebulous, as contrasted with the bright and definite cirro-form cloudlets seen when first noted, at considerably higher altitudes above the northern horizon.

Kensington, June 28.

D. J. Rowan.

DARKEST AFRICA.[1]

IT would be out of place in these pages to discuss Mr. Stanley's remarkable narrative of a remarkable expedition so far as the main purpose of that expedition is concerned. It is nearly four years since the interest in the position and fate of Emin Pasha reached its height in this country. The pages of NATURE and the columns of the daily press of the time will afford evidence of the universality and intensity of that interest, and of the reality of the belief that Emin and his people were in imminent danger of being exterminated by the Mahdists. Mr. Stanley insists on the ideal of Emin's conduct and character which was universally accepted at the time, as those of a hero who, in the face of danger and at the risk of death, loyally clung to his post and remained faithful to his duty and the people who regarded him as their leader and chief. As a man who had during his twelve years' sojourn in the Equatorial provinces made large contributions to science, the scientific world was naturally interested in his safety. Substantial evidence of what Emin has done for science may be seen in our own Natural History Museum. To rescue and relieve the pioneer of science and of civilization was the one object of the Expedition with the leadership of which Mr. Stanley was entrusted. It is evident from his narrative that the object was ever before his eyes, and that all else was subordinate. Through dangers innumerable and sufferings that might have daunted all but the boldest and truest spirits, the purpose for which the Expedition was organized was accomplished. Emin and all of his people who cared to accompany him were rescued, and that just in time; for, according to the latest reports, the Mahdists are now swarming on the shores of Albert Nyanza. That Emin presented himself to Mr. Stanley in a light somewhat different from the ideal; that the Governor was reluctant to leave; that most of his people were disloyal and demoralized wretches who might have been left to the tender mercies of the Mahdi, with whom they could easily have made terms; that there were other features about the expedition that may leave room for criticism, do not affect the general result. Mr. Stanley has once more proved his supremacy as a man of action, as a leader whose single aim is to accomplish what he undertakes. Even were Emin as full of blemishes as he is represented in Mr. Stanley's narrative, no one need regret the Expedition sent to his relief; it has helped to keep alive the sentiments of chivalry and humanity in the midst of a civilization in danger of becoming too materialistic, and given opportunity for the exercise of those noble qualities which make us proud of our race.

The truth is that no two men could be more dissimilar in character and conduct than are Emin and Mr. Stanley. They seem quite incapable of understanding each other; the one has no sympathy with the other's pursuits. Stanley is, before everything, a man of action, who goes direct to the accomplishment of whatever purpose he undertakes; Emin is a student of Nature, a man of science, who, by force of circumstance, had become ruler of a province, and military leader. As a man of science he may be too much given to making allowances to be fitted for a post where quickness of decision and rapidity of action were necessary, and where he had to deal with people with whom force was the only remedy. Whatever may be his weaknesses, science at least cannot regret the rescue of one of her most devoted disciples. With all Mr. Stanley's apparent contempt for science and her students, he himself has been one of her most successful pioneers.

It is hard to say whence Mr. Stanley has obtained his notion of the character of scientific men; and we are not disposed to take his verdict too seriously, when we remember the circumstances under which his book was written, and the many irritating conditions to which he had without doubt been subjected by the conduct of Emin and his people. The most satisfactory feature about the passage in which he flouts at science and its votaries is its inconsistency. This and other passages in his book, in which Mr. Stanley deals with science, only show that he is not equally strong all round; that, notwithstanding the valuable contributions he has made to science in this and his previous writings, he himself is not largely endowed with the scientific spirit.

While we are disposed to be critical, may we refer to one or two other passages in Mr. Stanley's book which seem to us to show that he had not quite recovered that equanimity which was so bitterly tried, even down to the arrival at Bagamoyo? Speaking of the fine race of the Wahuma, he scoffs at "some philological *nidderings*" for classing them and many other tribes in Central and South Africa under the common name of *Bantu*, which, as he truly tells us, simply means men. If Mr. Stanley intends by this to protest against the implication that, because a variety of peoples speak a certain type of language, therefore they must all be of common descent, he is quite justified. But that the languages over a large area of Central and South Africa have all a certain family likeness there can be little doubt; and the term Bantu is quite as useful as any other to express this fact. Possibly Mr. Stanley might be able to suggest a better term. In his chapter on the tribes of the grass land, where the fine Wahuma race is dominant, Mr. Stanley has some most suggestive and interesting remarks on the various types of African peoples, and on the immigration which must have taken place at an early period from Asia into Africa. In its ethnology, as in so many other respects, this strange continent presents many puzzles for the student of science to solve. Mr. Stanley's recent journey was, to a large extent, through the borderland which forms a sort of meeting-ground for various types of peoples; and the contributions he has made to ethnology will cover a multitude of flouts at science and its votaries. The many interesting details he gives concerning the pigmies that pestered his column so much on its march through the forest form one of the most prominent features of his narrative. Prof. Flower has so recently (NATURE, vol. xxxviii. pp. 44 *et seq.*) fully discussed the whole subject of pigmy races, that we need only refer the reader to Mr. Stanley's pages in confirmation of Prof. Flower's views. In his own graphic and peculiar way, Mr. Stanley claims a high antiquity for these tiny folks; and in this he is supported by the evidence adduced by Prof. Flower. Mr. Stanley himself, on his greatest journey of all, heard of these pigmies about the great bend of the Congo; on his last journey he found the forest swarming with them. Outside the sunless gloom of the forest they pine and die. They are naturally timid, and continually on the offensive against all comers. But when treated kindly they become devoted to their benefactors, and serve them faithfully even to their own hurt. Full details of the various dimensions of these pigmies are given from Emin's notes (vol. ii. p. 150); but we may quote here what Mr. Stanley says about the first of these pigmies whom he had an opportunity of inspecting. A man and a woman were brought to him at the Avatiko plantation, on the Ituri. The man was apparently about 21. Mr. Bonny conscientiously measured him, with the following result:—

"Height, 4 ft.; round head, $20\frac{1}{4}$ in.; from chin to back top of head, $24\frac{1}{2}$ in.; round chest, $25\frac{1}{2}$ in.; round abdomen, $27\frac{3}{4}$ in.; round hips, $22\frac{1}{2}$ in.; round wrist, $4\frac{1}{2}$ in.; round muscle of left arm, $7\frac{1}{2}$ in.; round ankle, 7 in.; round calf of leg, $7\frac{3}{4}$ in.; length of index finger, 2 in.; length of right hand, 4 in.; length of foot, $6\frac{1}{4}$ in.; length of leg, 22 in.; length of back, $18\frac{1}{2}$ in.; arm to tip of finger, $19\frac{3}{4}$ in.

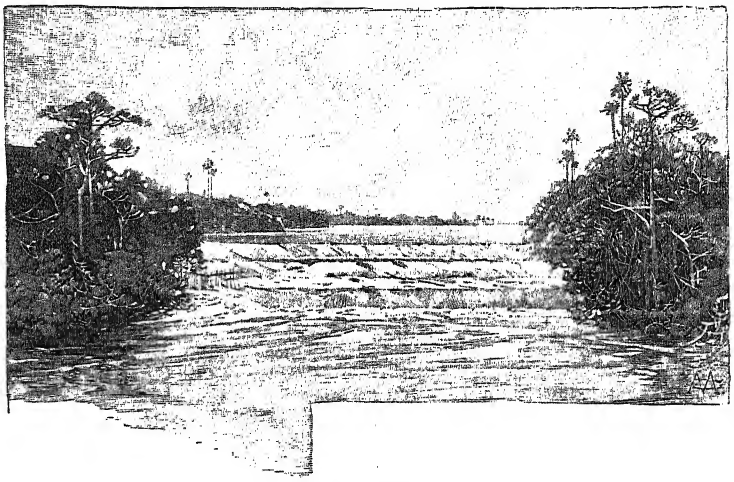
"This was the first full-grown man we had seen. His

[1] "In Darkest Africa: or the Quest, Rescue, and Retreat of Emin, Governor of Equatoria." By Henry M. Stanley. Two Vols. (London: Sampson Low and Co., 1890.)

colour was coppery; the fell over the body was almost furry, being nearly half an inch in length. His head-dress was a bonnet of a priestly form, decorated with a bunch of parrot feathers; it was either a gift or had been stolen. A broad strip of bark covered his nakedness. His hands were very delicate, and attracted attention by their unwashed appearance."

The chapter on the forest in the second volume, abounds with information concerning the various tribes which inhabit the forest region. There is a family likeness among all the varieties. With regard to the pigmies, Mr. Stanley maintains there are two distinct types (Batwa and Wambutti), which differ as much from each other as a Turk would from a Scandinavian. The Batwa have longish heads and long narrow faces, reddish small eyes, set close together, which give them a somewhat ferrety look, sour, anxious, and querulous. The Wambutti have round faces, gazelle-like eyes, set far apart, open foreheads, which give one an impression of undisguised frankness, and are of a rich, yellow, ivory complexion. The Wambutti occupy the southern half of the Ituri region, the Batwa the northern, and extend south-easterly to the Awamba forest on both banks of the Semliki river, and east of the Ituri. The women are agriculturists and the men hunters. Though their nomad habits are often annoying to the forest tribes of larger make, yet the latter find the pigmies exceedingly useful as scouts who give warning of the approach of the enemy. As might have been expected, the forest peoples are all of lighter complexion than the inhabitants of the open grass lands. With regard to the poison of the arrows of these pigmies, Mr. Stanley does not insist so strongly on its insect origin as he did in his letter to the Royal Geographical Society. The poison, he states, seems to be made from a species of arum. It is evident from these allusions that Mr. Stanley's contributions to a knowledge of the ethnology of the region are of great interest; indeed, they entitle him to be classed among those students of science whom he professes to despise.

The great forest, in which so much of the time of the Expedition was spent, and which entailed upon it so much suffering and so many losses, pervades the whole work; and one might even trace its depressing influences upon Mr. Stanley's style in the earlier chapters of the book. There are several points of great scientific interest connected with the forest. Mr. Stanley refers to Prof. Drummond in terms unnecessarily severe, because in his book on "Tropical Africa" he describes the type of African



Cascades of the Nepoko.

forest as quite different from that of Brazil. But Prof. Drummond can only be taken as referring to that part of Africa with which he is perfectly familiar, the Lake Nyassa region, where as in East Africa generally the "forest" is of the open park-like character, with dense patches here and there. But the fact is, we are apt to forget that Africa is a great continent covering some 11 million square miles, and that its surface presents a great variety of features. Here is how Mr. Stanley puts it:—

"Nyassaland is not Africa, but itself. Neither can we call the wilderness of Masai Land, or the scrub-covered deserts of Kalahari, or the rolling grass land of Usukuma, or the thin forests of Unyamwezi, or the ochreous acacia-covered area of Ugogo, anything but sections of a continent that boasts many zones. Africa is about three times greater than Europe in its extent, and is infinitely more varied. You have the desert of deserts in the Sahara, you have the steppes of Eastern Russia in Masai Land and parts of South Africa, you have the Castilian uplands in Unyamwezi, you have the best parts of France represented by Egypt, you have Switzerland in Ukonju and Toro, the Alps in Ruwenzori—you have Brazil in the Congo basin, the Amazon in the Congo River, and its immense forests rivalled by the Central African forest which I am about to describe.

"The greatest length of this forest, that is from near Kabambarré in South Manyuema to Bagbomo on the Welle-Makua in West Niam-niam, is 621 miles; its average breadth is 517 miles, which makes a compact square area of 321,057 square miles. This is exclusive of the forest areas separated or penetrated into by campo-like reaches of grass land, or of the broad belts of timber which fill the lower levels of each great river basin like the Lumani, Lulungu, Welle-Mubangi, and the parent river from Bolobo to the Loika River.

"The Congo and the Aruwimi Rivers enabled us to penetrate this vast area of primeval woods a considerable length. I only mean to treat, therefore, of that portion which extends from Yambuya in 25° $3\frac{1}{2}'$ E. L. to Indesura, 29° 59′ = $326\frac{1}{2}$ English miles in a straight line."

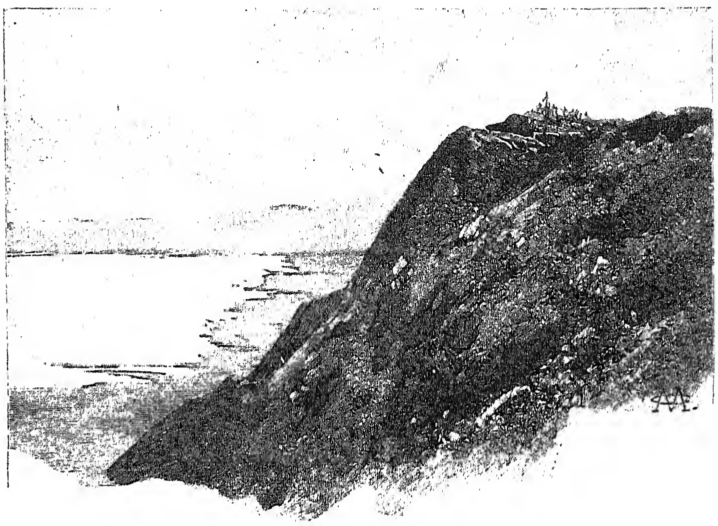
Mr. Stanley's description of an African tropical forest is also worth quoting:—

"Imagine the whole of France and the Iberian peninsula closely packed with trees varying from 20 to 180 feet high, whose crowns of foliage interlace and prevent any view of the sky and sun, and each tree from a few inches

to four feet in diameter. Then from tree to tree run cables from two inches to fifteen inches in diameter, up and down in loops and festoons and W's and badly-formed M's; fold them round the trees in great tight coils, until they have run up the entire height, like endless anacondas; let them flower and leaf luxuriantly, and mix up above with the foliage of the trees to hide the sun, then from the highest branches let fall the ends of the cables reaching near to the ground by hundreds with frayed extremities, for these represent the air roots of the Epiphytes; let slender cords hang down also in tassels with open thread-work at the ends. Work others through and through these as confusedly as possible, and pendent from branch to branch—with absolute disregard of material, and at every fork and on every horizontal branch plant cabbage-like lichens of the largest kind, and broad spear-leaved plants—these would represent the elephant-eared plant—and orchids and clusters of vegetable marvels, and a drapery of delicate ferns which abound. Now cover tree, branch, twig, and creeper with a thick moss like a green fur. Where the forest is compact as described above, we may not do more than cover the ground closely with a thick crop of phrynia, and amoma, and dwarf bush; but if the lightning, as frequently happens, has severed the crown of a proud tree, and let in the sunlight, or split a giant down to its roots, or scorched it dead, or a tornado has been uprooting a few trees, then the race for air and light has caused a multitude of baby trees to rush upward—crowded, crushing, and treading upon and strangling one another, until the whole is one impervious bush.

"But the average forest is a mixture of these scenes. There will probably be groups of fifty trees standing like columns of a cathedral, grey and solemn in the twilight, and in the midst there will be a naked and gaunt patriarch, bleached white, and around it will have grown a young community, each young tree clambering upward to become heir to the area of light and sunshine once occupied by the sire. The law of primogeniture reigns here also."

What is the real extent of the continuous forest area? Is the forest of Mr. Du Chaillu in the Ogowé region, and that in which Livingstone wandered between Tanganyika and Nyangwé, really part of the same great forest through which the Ituri flows? The two slave-raiding parties which Mr. Stanley met on the Ituri, and which had come



View of the South End of Albert Nyanza.

north from Kibongé on the Upper Congo, journeyed through dense forest the whole way, meeting with not a patch of open grass. That the forest may be almost continuous from about Nyangwé to the Ituri, and for some distance northwards, is probable enough. But that there is one continuous forest from the Lower Ogowé to the plateau above Lake Albert is highly improbable. Indeed, from the observations of De Brazza and of Mr. Stanley himself on the Lower Congo, and in the country between that and the Ogowé, we know that there does exist much open country there. Even in the region with which Mr. Stanley specially deals—the region along the Ituri and to the north and south—it must be remembered that it has been traversed only along one or two lines. Considering the close network of rivers which characterize the region, it is probable enough that over a very great area we have one dense forest. Readers of Schweinfurth, Emin, and Junker, will remember the "gallery" forests which they describe to the north and north-east of Mr. Stanley's route; forests lining the banks of the rivers, and stretching for several miles from their banks. It may be, then, that in the Ituri region, with its many rivers, we have a series of gallery forests which have coalesced or overlapped into one continuous forest. With the rapidly progressing opening-up of Africa, this is a problem that cannot remain long unsolved. At the same time it should be remembered that even in the Amazonian basin the forest is by no means continuous, but gives way in many places to great stretches of open land.

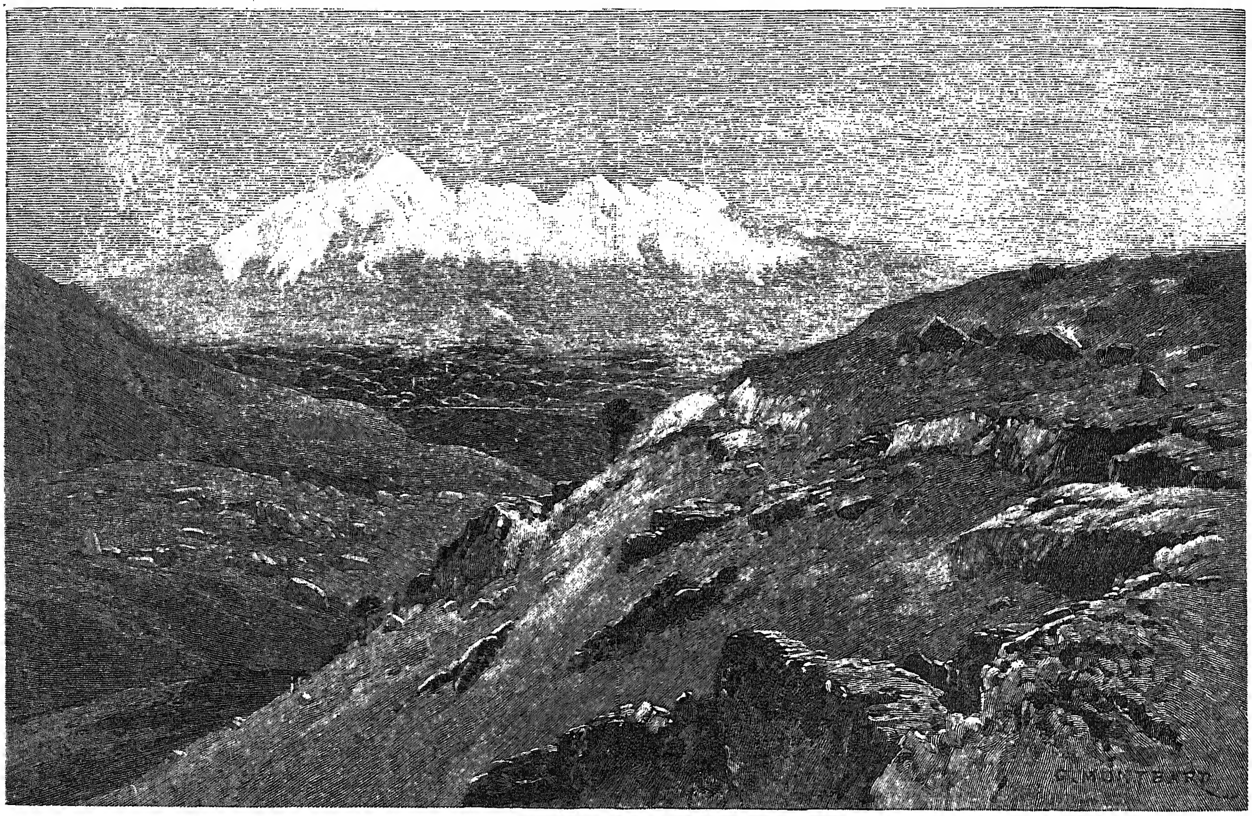
Mr. Stanley was here in what is probably the rainiest region of all Africa. We were at first disposed to believe that most of the moisture found its way westwards from the Indian Ocean. But this is a point on which Mr. Stanley made many careful observations, and his conclusion that the great rain-bearing winds come from the Atlantic must meantime be accepted. At the same time it is to be hoped that the Government of the Congo Free State will establish a series of observing-stations over as wide an area as possible, and so collect data which will be useful not only to science, but of service to its own economic interests. One great service rendered by Emin Pasha was the daily series of observations which he carried on at Lado for several years, and which render that station the one place in Central Africa for which we have trustworthy meteorological data. Emin carried on his observations during the whole period he was with Mr. Stanley,

down to Bagamoyo, and it is to be hoped that these will one day be given to the world.

About the prevailing plateau character of the whole country traversed by Mr. Stanley there can be no doubt. Some of its remarkable features are well brought out in many of the fine illustrations which adorn the book. The importance of this feature in the opening up of the centre of the continent under the guidance of Europeans is evident. The magnificent grassy plateau between Lake Albert and the edge of the forest, where the Expedition lived many weeks while waiting for Emin and his people, seems really a fine country from this point of view. What could be accomplished by partially clearing the forest may be seen from the planting operations at Fort Bodo, where Lieutenant Stairs and Dr. Parke lived for many months, and where they grew large crops of maize, bananas, tobacco, and other cultures. Here is a description of the country as seen from the plateau above Lake Albert:—

"Yesterday Jephson and I had examined the summits of the hills, and in one of the hollows we had discovered tree ferns, standing eight feet high, with stalks eight inches in diameter. We also brought with us a few purple flowering heliotropes, aloes, and rock ferns for the Pasha. All this has inspired him with a desire to investigate the flora for himself.

"These hills have an altitude varying from 5400 to 5600 feet above the sea. The folds and hollows between these hills are here and there somewhat picturesque, though on account of the late grass burnings they are not at their best just now. Each of the hollows has its own clear water rillet, and along their courses are bamboos, tree ferns, small palms, and bush, much of which is in flower. From the lively singing of the birds I heard



Ruwenzori: from Karimi. (From a Photograph.)

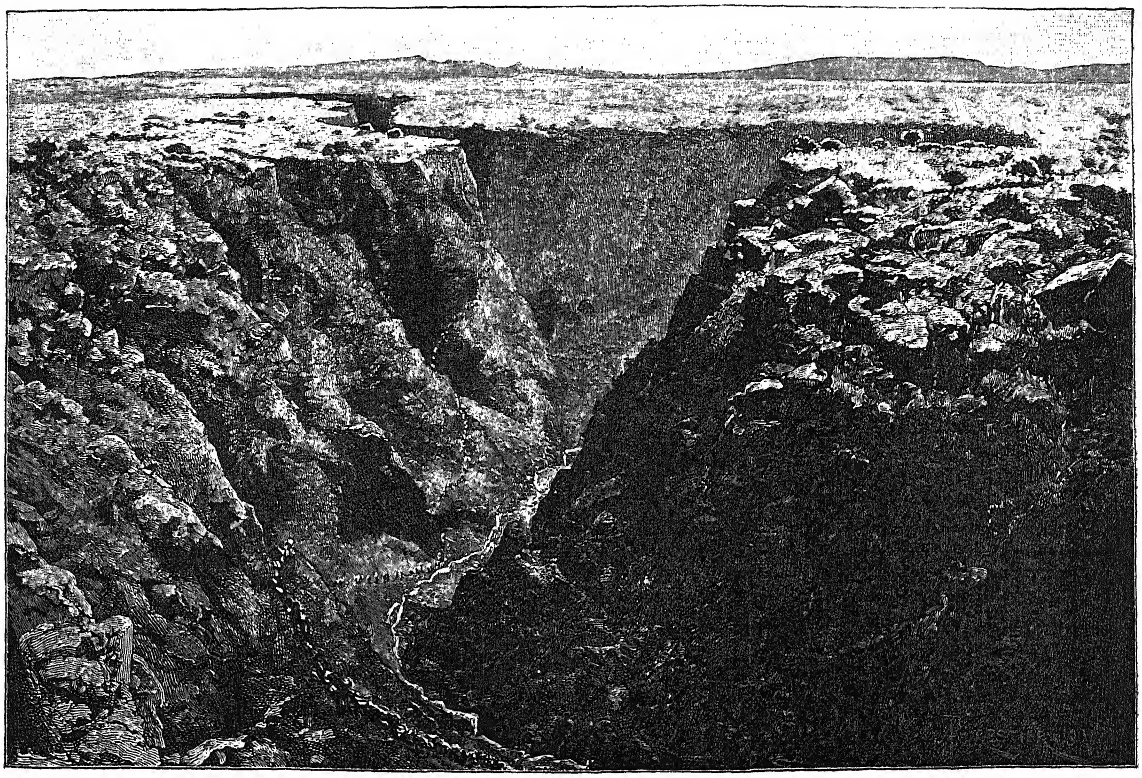
yesterday, it was thought likely this insatiable collector might be able to add to his store of stuffed giant-larks, thrushes, bee-eaters, sun-birds, large pigeons, &c. Only four specimens were obtained, and the Pasha is not happy.

"In a bowl-like basin, rimmed around by rugged and bare rocks, I saw a level terrace a mile and a half long by a mile wide, green as a tennis lawn. Round about the foot of this terrace ran a clear rivulet, through a thick bank of woods, the tops of which just came to the level of the terrace. It has been the nicest site for a mission or a community of white men that I have seen for a long time. The altitude was 5500 feet above the sea. From the crest of the rocky hills encircling it we may obtain a view covering 3000 square miles of one of the most gloriously beautiful lands in the world. Pisgah, sixty miles westward, dominates all eminences and ridges in the direction of the forest world; Ruwenzori, 18,000 to 19,000, white with perpetual snow, eighty miles off, bounds the view south; to the east the eye looks far over the country of Unyoro; and north-east lies the length of the Albert Nyanza."

The instructive map prepared by Stanford from Mr. Stanley's observations affords an excellent idea of the physical features of the region traversed, and especially of the new features to the south of Lake Albert. This is even more strikingly brought out in the bird's-eye view of the region presented in one of the plates. The sudden fall from the plateau down to the level of Lake Albert, over 2000 feet, is remarkable. It is not quite so marked on the southern lake, which is not so much of the nature of a ravine; it may be because the lake has much diminished in size. That both lakes have greatly shrunk is evident; but that they ever formed one lake is a point

that can only be established by a series of careful observations. The same may be said as to the real nature of the change of level; is it permanent, or is it only of the nature of an oscillation of level, as is the case with other African lakes? What, again, are the forces which have been at work to produce these lake-chasms, and raise the magnificent mountain-mass of Ruwenzori? It may well be that the same forces have been at the bottom of both features, though possibly not in the precise fashion that Mr. Stanley would seem to indicate. There is here, evidently, a splendid field for geological research, and science has therefore every reason to wish that all this region may soon be restored to civilizing influences—included, if possible, within the sphere of the British East African Company. The volcanic mountain-mass of Ruwenzori, with its many snow-covered peaks and deeply scored sides, really covers a considerable area, with outlying peaks, like Gordon Bennett and Mackinnon, east and north-east. Both it and Lake Albert Edward are surrounded by a range or escarpment, 5500 to 6500 feet high. Stretching all the way south-east almost to the borders of Lake Victoria Nyanza, the table-land is much cut up by ravines, sometimes assuming a cañon-like shape, and marked here and there with peaks like Mfumbiro, 10,000 feet. On the Semliki itself, which joins Lakes Albert and Albert Edward, we find a forest, very similar to that on the Ituri, stretching some little distance up the lower slopes of Ruwenzori. The following description of the Semliki forest is worth quoting:—

"About a mile from Mtarega the grassy strip to which we had clung in preference was ended, the forest had marched across the breadth of the Semliki Valley, and had absorbed the Ruwenzori slopes to a height of seven thousand feet above us, and whether we would or no, we



Expedition winding up the Gorge of Karya-muhoro.

had to enter the doleful shades again. But then the perfection of a tropical forest was around us. It even eclipsed the Ituri Valley in the variety of plants and general sappiness. There were clumps of palms, there were giant tree-ferns, there were wild bananas, and tall, stately trees all coated with thick green moss from top to root, impenetrable thickets of broad-leafed plants, and beads of moisture everywhere, besides tiny rillets oozing out every few yards from under the matted tangle of vivid green and bedewed undergrowth. It was the best specimen of a tropical conservatory I had ever seen. It could not be excelled if art had lent its aid to improve nature. In every tree-fork and along the great horizontal branches grew the loveliest ferns and lichens; the elephant-ear by the dozen, the orchids in close fellowship, and the bright green moss had formed soft circular cushions about them, and on almost every fibre there trembled a clear water-drop, and everything was bathed by a most humid atmosphere. The reason of all this was not far to seek; there were three hot-water springs, the temperature of which was 102°. This tract of forest was also in the cosiest fold of the snow mountains, and whatever heat a hot sun furnished on this place was long retained."

Mr. Stanley may be said in this expedition to have put the final touch to the definite delimitation of the Congo and the Nile basins. It looks only a few steps from the sources of the Ituri and its feeders, which go to swell the Congo, to the edge of the escarpment whose feet are lapped by the waters of Lake Albert, which sends its tribute to the Nile. That the Southern Muta Nzige (Lake Albert Edward) belongs to the Nile and not to the Congo system is finally proved. Mr. Stanley, who seems to have been still in a fighting mood while he was writing his book at Cairo, severely castigates the

map-makers for ignoring the work of the cartographers of ancient times. We need not quarrel with his manner of opening up a subject of great interest. What were the relations of Egypt, and therefore of ancient Europe, with Central Africa? It is certainly a noteworthy fact that, even in the oldest maps (whatever was the real nature of these maps), we find the Nile coming out of two lakes, and we find a range of mountains somewhere in the neighbourhood, called the Mountains of the Moon. It is not to be supposed that the people of Africa were less restless in ancient times than they are now, nor that the Egyptians did not make efforts to find out where their great river came from. Expeditions into the heart of Africa we read of in Herodotus and elsewhere. However the knowledge came—probably obtained through traders or from natives brought down as slaves to Egypt—there can be no doubt that the Ptolemaic maps of Africa bear some distant resemblance to reality. But all became much exaggerated as time went on. The maps of Africa became overcrowded with features the authority for which it was impossible to find out; the "Mountains of the Moon" stretched themselves right across the continent. That snowy Ruwenzori, Kilimanjaro, and Kenia formed the original nucleus out of which these mountains were evolved there can be little doubt. But modern discoveries have proved how unlike the maps of Africa, before d'Anville swept them clean, were to the reality, and how essential it was to make a new start. No one has done more than Mr. Stanley himself to fill the map of Africa with authentic features.

Mr. Stanley's pages teem with facts and suggestions of interest to science; we have only touched upon a few of

South-west Extremity of Lake Victoria Nyanza.

the more prominent topics referred to in the work. Here, for example, are some curious data with regard to the distribution of malaria:—

"On the plateau of Kavalli and Undussuma, Messrs. Jephson, Parke, and myself were successively prostrated by fever, and the average level of the land was over 4500 feet above the sea.

"On descending to the Nyanza plain, 2500 feet lower, we were again laid up with fierce attacks.

"At Banana Point, which is at sea-level, ague is only too common.

"At Boma, 80 feet higher, the ague is more common still.

"At Vivi, there were more cases than elsewhere, and the station was about 250 feet higher than Boma, and not a swamp was near it.

"At Stanley Pool, about 1100 feet above sea-level, fever of a pernicious form was prevalent.

"While ascending the Congo with the wind astern we were unusually exempted from ague.

"But descending the Upper Congo, facing the wind, we were smitten with most severe forms of it.

"While ascending the Aruwimi we seldom thought of African fever, but descending it in canoes, meeting the wind currents, and carried towards it by river-flow and paddle, we were speedily made aware that acclimatization is slow.

"Therefore it is proved that from 0 to 5000 feet above the sea there is no immunity from fever and ague; that over forty miles of lake water between a camp and the other shore are no positive protection; that a thousand miles of river course may serve as a flue to convey

malaria in a concentrated form; that if there is a thick screen of primeval forest or a grove of plantains between the dwelling-place and a large clearing or open country there is only danger of the local malaria around the dwelling, which might be rendered harmless by the slightest attention to the system; but in the open country neither a house nor a tent is sufficient protection, since the air enters by the doors of the house, and under the flaps, and through the ventilators to poison the inmates.

"Hence we may infer that trees, tall shrubbery, a high wall or close screen interposed between the dwelling-place and the wind currents will mitigate their malarial influence, and the inmate will only be subjected to local exhalations.

"Emin Pasha informed me that he always took a mosquito curtain with him, as he believed that it was an excellent protector against miasmatic exhalations of the night.

"Question, might not a respirator attached to a veil, or face screen of muslin, assist in mitigating malarious effects when the traveller finds himself in open regions?"

As a matter of fact, we believe a veil or a mosquito curtain is found a useful preventive in malarial regions.

Mr. Stanley gives some natural history notes which he obtained from Emin. Here, for example, is a statement which he gives in Emin's own words, but which notwithstanding is somewhat astounding:—

"The forest of Msongwa is infested with a large tribe of chimpanzees. In summer-time, at night, they frequently visit the plantations of Mswa Station to steal the fruit. But what is remarkable about this is the fact that they use torches to light the way! Had I not witnessed this extraordinary spectacle personally, I should never have credited that any of the Simians understood the art of making fire.

"One of these same chimpanzees stole a native drum from the station, and went away pounding merrily on it. They evidently delight in that drum, for I have frequently heard them rattling away at it in the silence of the night."

The importance of this fact with regard to fire-using (it is not stated that they are fire-making) chimpanzees need not be pointed out. We cannot doubt the accuracy of Mr. Stanley's report, nor the trustworthiness of Emin's observation; but we should like to have more details.

Great expectations have been formed of Mr. Stanley's narrative of one of the most remarkable African expeditions on record. These expectations have not been disappointed. The reader who merely seeks for the excitement of adventure will find what he seeks in almost every page. We have written enough to prove that the student of science and the geographer will find the narrative teeming with novel and suggestive facts. There are no doubt a few marks of haste and fatigue on the part of the author; but the work is altogether worthy of Mr. Stanley's brilliant record, and entitles him, let us once more say, to be ranked among the foremost pioneers of science in "Darkest Africa."

By the courtesy of Messrs. Sampson Low and Co. we are able to give a few specimens of the 150 illustrations which add so much to the beauty and value of the book, which from the point of view of get-up is entirely creditable to all concerned. J. S. K.

PROBLEMS IN THE PHYSICS OF AN ELECTRIC LAMP.[1]

II.

AT this stage it will perhaps be most convenient to outline briefly the beginnings of a theory proposed to reconcile these facts, and leave you to judge how far the subsequent experiments confirm this hypothesis. The theory very briefly is as follows:—From all parts of the incandescent carbon loop, but chiefly from the negative leg, carbon molecules are being projected which carry with them, or are charged with, negative electricity. I will in a few moments make a suggestion to you which may point to a possible hypothesis on the manner in which the molecules acquire this negative charge. Supposing this, however, to be the case, and that the bulb is filled with these negatively-charged molecules, what would be the result of introducing into their midst a conductor such as this middle metal plate which is charged positively? Obviously, they would all be attracted to it and discharge against it. Suppose the positive charge of this conductor to be continually renewed, and the negatively-charged molecules continually supplied, which conditions can be obtained by connecting the middle plate to the positive electrode of the lamp, the obvious result will be to produce a current of electricity flowing through the wire or galvanometer, by means of which this middle plate is connected to the positive electrode of the lamp. If, however, the middle plate is connected to the negative electrode of the lamp, the negatively-charged molecules can give up no charge to it, and produce no current in the interpolated galvanometer. We see that on this assumption the effect must necessarily be diminished by any arrangement which prevents these negatively-charged molecules from being shot off the negative leg or from

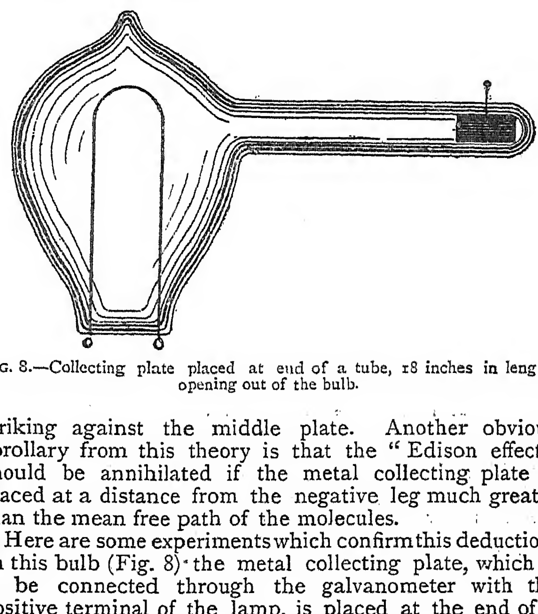


FIG. 8.—Collecting plate placed at end of a tube, 18 inches in length, opening out of the bulb.

striking against the middle plate. Another obvious corollary from this theory is that the "Edison effect" should be annihilated if the metal collecting plate is placed at a distance from the negative leg much greater than the mean free path of the molecules.

Here are some experiments which confirm this deduction. In this bulb (Fig. 8) the metal collecting plate, which is to be connected through the galvanometer with the positive terminal of the lamp, is placed at the end of a long tube opening out of and forming part of the bulb. We find the "Edison effect" is entirely absent, and that the galvanometer current is zero. We have, as it were, placed our target at such a distance that the longest range molecular bullets cannot hit it, or, at least, but very few of them do so. Here again is a lamp in which the plate is placed at the extremity of a tube opening out of the bulb, but bent at right angles (Fig. 9). We find in this case, as first discovered by Mr. Preece, that there is no "Edison effect." Our molecular marksman cannot shoot round a corner. None of the negatively charged molecules can reach the plate, although that plate is placed at a distance not greater than would suffice to produce the effect if the bend were straightened out. Following out our hypothesis into its consequences would lead us to conclude that the material of which the plate is made is without influence on the result, and this is found to be the case. Many of the foregoing facts were established

[1] Friday Evening Discourse delivered at the Royal Institution by Prof. J. A. Fleming, M.A., D.Sc., on February 14, 1890. Continued from p. 201.

by Mr. Preece as far back as 1885, and I have myself abundantly confirmed his results.

We should expect also to find that the larger we make our plate, and the nearer we bring it to the negative leg of the carbon, the greater will be the current produced in a circuit connecting this plate to the positive terminal of the lamp. I have before me a lamp with a large plate placed very near the negative leg of the carbon of a lamp, and we find that we can collect enough current from these molecular charges to work a telegraph relay and ring an electric bell. The current which is now working this relay is made up of the charges collected by the plate from the negatively charged carbon molecules which are projected against it from the negative leg, across the highly perfect vacuum. I have tried experiments with lamps in which the collecting plate is placed in all kinds of positions, and has various forms, some of which are here, and are represented in the diagrams before you; but the results may all be summed up by saying that the greatest effects are produced when the collecting plate is as near as possible to the base of the negative end of the loops, and, as far as possible, incloses, without touching, the carbon conductor. Time will not permit me to make more than a passing reference to the fact that the magnitude of the current flowing through the galvanometer when connected between the middle plate and the positive terminal of the lamp often "jumps" from a low to a high value, or *vice versâ*, in a remarkable manner, and

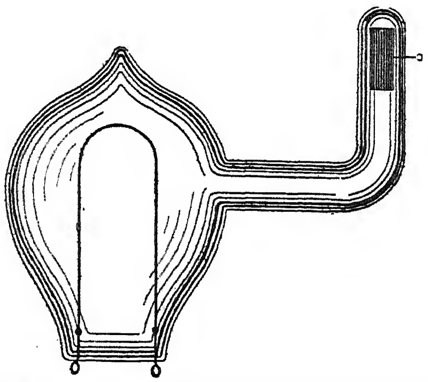


Fig. 9.—Collecting plate placed at end of an elbow tube opening out of the bulb.

that this sudden change in the current can be produced by bringing strong magnets near the outside of the bulb.

Let us now follow out into some other consequences this hypothesis that the interior of the bulb of a glow-lamp when in action is populated by flying crowds of carbon atoms all carrying a negative charge of electricity. Suppose we connect our middle collecting plate with some external reservoir of electric energy, such as a Leyden jar, or with a condenser equivalent in capacity to many hundreds of Leyden jars, and let the side of the condenser which is charged positively be first placed in connection through a galvanometer with the middle plate (see Fig. 10), whilst the negative side is placed in connection with the earth. Here is a condenser of two microfarads capacity so charged and connected. Note what happens when I complete the circuit and illuminate the lamp by passing the current through its filament. The condenser is at once discharged. If, however, we repeat the same experiment with the sole difference that the negatively charged side of the condenser is in connection with the middle plate then there is no discharge. The experimental results may be regarded from another point of view. In order that the condenser may be discharged as in the first case, it is essential that the negatively charged side of the condenser shall be in connection with some part of the circuit of the incandescent carbon loop. This experiment with the condenser discharged by the lamp may be then looked upon as an arrangement in which the plates of a charged condenser are connected respectively to an incandescent carbon loop and to a cool metal plate, both being inclosed in a highly vacuous space, and it appears that when the incandescent conductor is the negative electrode of this arrangement the discharge takes place, but not when the cooler metal plate is the negative electrode of the charged condenser. The negative charge of the condenser can be carried across the

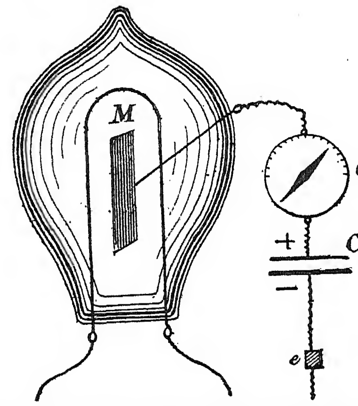


Fig. 10.—Charged condenser C discharged by middle plate M, when the positively charged side of condenser is in connection with the plate and other side to earth *e*.

vacuous space from the hot carbon to the colder metal plate, but not in the reverse direction.

This experimental result led me to examine the condition of the vacuous space between the middle metal plate and the negative leg of the carbon loop in the case of the lamp employed in our first experiment. Let us return for a moment to that lamp. I join the galvanometer between the middle plate and the negative terminal of the lamp, and find, as before, no indication of a current. The metal plate and the negative terminal of the lamp

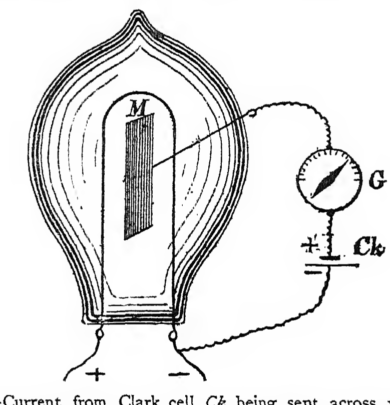


Fig. 11.—Current from Clark cell *Ck* being sent across vacuous space between negative leg of carbon and middle plate M. Positive pole of cell in connection with plate M through galvanometer G.

are at the same electrical potential. In the circuit of the galvanometer we will insert a single galvanic cell having an electromotive force of rather over one volt. In the first place let that cell be so inserted that its negative pole is in connection with the middle plate, and its positive pole in connection through the galvanometer with the negative terminal of the lamp (see Fig. 11). Regarding the circuit of that cell alone, we find that it consists of the cell itself, the galvanometer wire, and that half-inch of highly vacuous space between the hot carbon conductor

and the middle plate. In that circuit the cell cannot send any sensible current at all, as it is at the present moment connected up. But if we reverse the direction of the cell so that its positive pole is in connection with the middle plate, the galvanometer at once gives indications of a very sensible current. This highly vacuous space, lying between the middle metal plate on the one hand, and the incandescent carbon on the other, possesses a kind of unilateral conductivity, in that it will allow the current from a single galvanic cell to pass one way but not the other. It is a very old and familiar fact that in order to send a current from a battery through a highly rarefied gas by means of metal electrodes, the electromotive force of the battery must exceed a certain value. Here, however, we have indication that if the negative electrode by which that current seeks to enter the vacuous space is made incandescent the current will pass at a very much lower electromotive force than if the electrode is not so heated.

A little consideration of the foregoing experiments led to the conclusion that in the original experiment, as devised by Mr. Edison, if we could by any means render the middle plate very hot, we should get a current flowing through a galvanometer when it is connected between the middle plate and the negative electrode of the carbon. This experiment can be tried in the manner now to be shown. Here is a bulb (Fig. 12) having in it two carbon

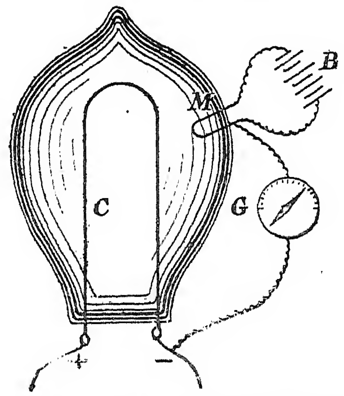


FIG. 12.—Experiment showing that when the "middle plate" is a carbon loop rendered incandescent by insulated battery B, a current of negative electricity flows from M to the positive leg of main carbon C across the vacuum.

loops; one of these is of ordinary size, and will be rendered incandescent by the current from the mains. The other loop is very small, and will be heated by a well-insulated secondary battery. This smaller incandescent loop shall be employed just as if it were a middle metal plate. It is, in fact, simply an incandescent middle conductor. On repeating the typical experiment with this arrangement, we find that the galvanometer indicates a current when connected between the middle loop and either the positive *or* the negative terminal of the main carbon. I have little doubt but that if we could render the platinum plate in our first-used lamp incandescent by concentrating on it from outside a powerful beam of radiant heat we should get the same result.

A similar set of results can be arrived at by experiments with a bulb constructed like an ordinary vacuum tube, and having small carbon loops at each end instead of the usual platinum or aluminium wires. Such a tube is now before you (see Fig. 13), and will not allow the current from a few cells of a secondary battery to pass through it when the carbon loops are cold. If, however, by means of well-insulated secondary batteries we render both of the carbon loop electrodes highly incandescent, a single cell of a battery is sufficient to pass a very considerable current across that vacuous space, provided the resistance of the rest of the circuit is not large. We may embrace the foregoing facts by saying that if the electrodes, but especially the negative electrode, which form the means of ingress and egress of a current into a vacuous space are capable of being rendered highly incandescent, and if at that high temperature they are made to differ in electrical potential by the application of a very small electromotive force, we may get under these circumstances a very sensible current through the rarefied gas. If the electrodes are cold a very much higher electromotive force will be necessary to begin the discharge or current through the space. These facts have been made the subject of elaborate investigation by Hittorf and Goldstein, and more recently by Elster and Geitel. It is to Hittorf that I believe we are indebted for the discovery of the fact that by heating the negative electrode we greatly reduce the apparent resistance of a vacuum.

Permit me now to pave the way by some other experiments for a little more detailed outline of the manner in which I shall venture to suggest these negative molecular charges are bestowed. This is really the important matter to examine. In seeking for some probable explanation of the manner in which these wandering molecules of carbon in the glow-lamp bulb obtain their negative charges, I fall back for assistance upon some facts discovered by the late Prof. Guthrie. He showed some years ago new experiments on the relative powers of incandescent bodies for retaining positive and negative charges. One of the facts he brought forward[1] was that

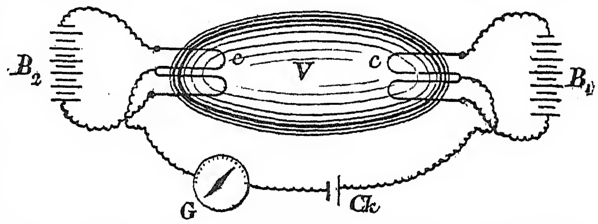


FIG. 13.—Vacuum tube having carbon loop electrodes, *c c*, at each end rendered incandescent by insulated batteries, B_1 B_2, showing current from Clark cell, *Ck*, passing through the high vacuum when the electrodes are incandescent.

a bright red-hot iron ball, well insulated, could be charged negatively, but could not retain for an instant a positive charge. He showed this fact in a way which it is very easy to repeat as a lecture experiment. Here is a gold-leaf electroscope, to which we will impart a positive charge of electricity, and project the image of its divergent leaves on the screen. A poker, the tip of which has been made brightly red-hot, is placed so that its incandescent end is about an inch from the knob of the electroscope. No discharge takes place. Discharging the electroscope with my finger, I give it a small charge of negative electricity, and replace the poker in the same position. The gold leaves instantly collapse. Bear in mind that the extremity of the poker, when brought in contiguity to the knob of the charged electroscope, becomes charged by induction with a charge of the opposite sign to that of the charge of the electroscope, and you will at once see that this experiment confirms Prof. Guthrie's statement, for the negatively charged electroscope induces a positive charge on the incandescent iron, and this charge cannot be retained. If the induced charge on the poker is a negative charge, it is retained, and hence the positively charged electroscope is not discharged, but the negatively charged electroscope at once loses its charge. Pass in imagination from iron balls to carbon molecules. We may ask whether it is a legitimate assumption to suppose the same fact to hold good for them, and that a hot

[1] "On a New Relation between Electricity and Heat," *Phil. Mag.*, vol. xlv. p. 308 (1873).

carbon molecule or small carbon mass just detached from an incandescent surface behaves in the same way and has a greater grip for negative than for positive charge? If this can possibly be assumed, we can complete our hypothesis as follows:—Consider a carbon molecule or small collection of molecules just set free by the high temperature from the negative leg of the incandescent carbon horseshoe. This small carbon mass finds itself in the electrostatic field between the branches of the incandescent carbon conductor (see Fig. 14). It is acted upon inductively, and if it behaves like the hot iron ball in Prof. Guthrie's experiment it loses its positive charge. The molecule then being charged negatively is repelled along the lines of electric force against the positive leg.

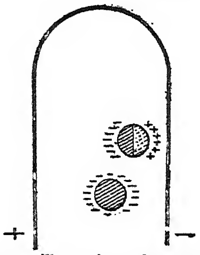


FIG. 14.—Rough diagram illustrating a theory of the manner in which projected carbon molecules may acquire a negative charge.

The forces moving it are electric forces, and the repetition of this action would cause a torrent of negatively-charged molecules to pour across from the negative to the positive side of the carbon horseshoe. If we place a metal plate in their path, which is in conducting connection with the positive electrode of the lamp carbon, the negatively charged molecules will discharge themselves against it. A plate so placed may catch more or less of this stream of charged molecules which pour across between the heels of the carbon loop. There are many extraordinary facts, which as yet I have been able only imperfectly to explore, which relate to the sudden changes in the direction of the principal stream of these charged molecules, and to their

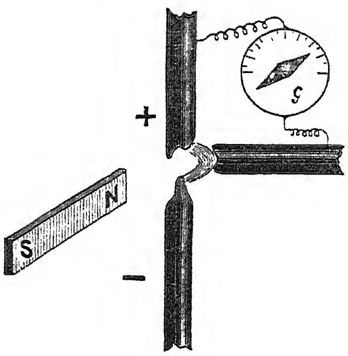


FIG. 15.—Electric arc projected by magnet against a third carbon, and showing a strong electric current flowing through a galvanometer, G, connected between the positive and this third carbon.

guidance under the influence of magnetic forces. The above rough sketch of a theory must be taken for no more than it is worth, viz. as a working hypothesis to suggest further experiments.

These experiments with incandescence lamps have prepared the way for me to exhibit to you some curious facts with respect to the electric arc, and which are analogous to those which we have passed in review. If a good electric arc is formed in the usual way, and if a third insulated carbon held at right angles to the other two is placed so that its tip just dips into the arc (see Fig. 15), we can show a similar series of experiments. It is rather more under control if we cause the arc to be projected against the third carbon by means of a magnet. I have now formed on the screen an image of the carbon poles and the arc between them, in the usual way. Placing a magnet at the back of the arc, I cause the flame of the arc to be deflected laterally and to blow against a third insulated carbon held in it. There are three insulated wires attached respectively to the positive and to the negative carbons of the arc, and to the third or insulated carbon, the end of which dips into the flame of the arc projected by the magnet. On starting the arc this third carbon is instantly brought down to the same electrical potential as the negative carbon of the arc, and if I connect this galvanometer in between the negative carbon and the third or insulated carbon, I get, as you see, no indication of a current. Let me, however, change the connections and insert the circuit of my galvanometer in between the positive carbon of the arc and the middle carbon, and we find evidence, by the violent impulse given to the galvanometer, that there is a strong current flowing through it. The direction of this current is equivalent to a flow of negative electricity from the middle carbon through the galvanometer to the positive carbon of the arc. We have here, then, the "Edison effect" repeated with the electric arc. So strong is the current flowing in a circuit connecting the middle carbon with the positive carbon that I can, as you see, ring an electric bell and light a small incandescent lamp when these electric-current

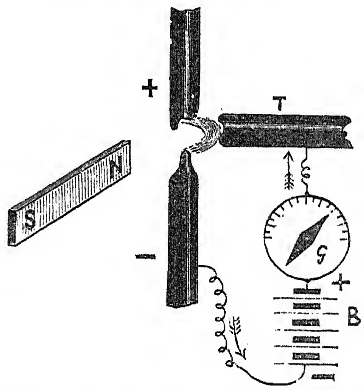


FIG. 16.—Galvanometer G and battery B inserted in series between negative carbon of electric arc and a third carbon to show unilateral conductivity of the arc between the negative and third carbons.

detectors are placed in connection with the positive and middle carbons.

We also find that the flame-like projection of the arc between the negative carbon possesses a unilateral conductivity. I join this small secondary battery of fifteen cells in series with the galvanometer, and connect the two between the middle carbon and the negative carbon of the arc. Just as in the analogous experiment with the incandescent lamp, we find we can send negative electricity along the flame of the arc one way but not the other. The secondary battery causes the galvanometer to indicate a current flowing through it when its negative pole is in connection with the negative carbon of the arc (see Fig. 16), but not when its positive pole is in connection with the negative carbon. On examining the third or middle carbon after it has been employed in this way for some time, we find that its extremity is cratered out and converted into graphite, just as if it had been employed as the positive carbon in forming an electric arc.

Time forbids me to indulge in any but the briefest remarks on these experiments; but one suggestion may be made, and that is that they seem to indicate that the chief movement of carbon molecules in the electric arc is *from* the negative *to* the positive carbon. The idea suggests itself that, after all, the cratering out of the positive carbon of the arc may be due to a sand-blast-like action

of this torrent of negatively charged molecules which are projected from the negative carbon. If we employ a soft iron rod as our lateral pole, we find that, after enduring for some time the projection of the arc against it, it is converted at the extremity into *steel*.

Into the fuller discussion as to the molecular actions going on in the arc, the source and nature of that which has been called the counter-electromotive force of the arc, and the causes contributing to produce unsteadiness and hissing in the arc, I fear that I shall not be able to enter, but will content myself with the exhibition of one last experiment, which will show you that a high vacuum, or, indeed, any vacuum, is not necessary for the production of the "Edison effect." Here is a carbon horseshoe-shaped conductor, not inclosed in any receiver (see Fig. 17). Close to the negative leg or branch, yet not touching it, we have adjusted a little metal plate. The sensitive galvanometer is connected between this metal plate and the base of the other or positive leg of this carbon arch.

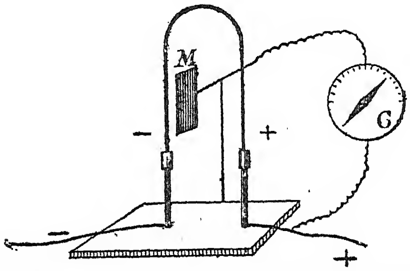


FIG. 17.—"Edison effect" experiment shown with carbon in open air.

On sending a current through the carbon sufficient to bring it to bright incandescence, the galvanometer gives indications of a current flowing through it, and as long as the carbon endures, which is not, however, for many seconds, there is a current of electricity through it equivalent to a flow of negative electricity from the plate through the galvanometer to the positive electrode of the carbon. The interposition of a thin sheet of mica between the metal plate and the negative leg of the carbon loop entirely destroys the galvanometer current.[1]

These experiments and brief expositions cover a very small portion of the ground which is properly included within the limits of my subject. Such fragments of it as we have been able to explore to-night will have made it clear that it is a region abounding in interesting facts and problems in molecular physics. The glow-lamp and the electric arc have revolutionized our methods of artificial lighting, but they present themselves also as subjects of scientific study, by no means yet exhausted of all that they have to teach.

NOTES.

PROF. E. RAY LANKESTER, F.R.S., has been elected Deputy Linacre Professor of Human and Comparative Anatomy, Oxford.

TO-DAY a meeting will be held at the Mansion House in support of the International Congress of Hygiene and Demography, which is to be held in London in 1891. The Prince of Wales has consented to act as President of the forthcoming Congress; and it is expected that the meetings will be "of great magnitude and importance." Many delegates have already been appointed, and other nominations are being received daily. It is necessary, therefore, that a definite organization should be formed, and that a fund should be raised for the defraying of expenses.

[1] This last experiment is due to my assistant, Mr. A. H. Bate.

ON Wednesday next, July 9, Prof. T. McKenny Hughes F.R.S., will deliver a lecture in the saloon of the Mansion House on the question, "Is there coal in the south-east of England?" The Lord Mayor will preside, and will be supported by the "Coal Search Committee." This Committee has been formed for the purpose of taking steps to discover whether there really are good coal-fields in the south-east of England, and, if so, to what extent. It consists of scientific and commercial men, and their services are gratuitous. Among the members are Prof. Boyd Dawkins, Colonel Godwin-Austen, Prof. A. H. Green, Dr. Henry Hicks, Mr. W. H. Hudleston, Prof. Edward Hull, Dr. A. Irving, Prof. J. W. Judd, Sir John Lubbock, Prof. Meldola, Mr. F. W. Rudler, Prof. W. J. Sollas, Mr. J. J. H. Teall, Mr. W. Topley, Mr. W. Whitaker, and Dr. H. Woodward.

A NEW scientific Society (Die Deutsche zoologische Gesellschaft, on the lines of the Anatomische Gesellschaft) has just been founded. Early in May, nine representatives of zoology in German Universities issued a circular inviting brother zoologists to unite in forming a Zoological Society. On May 28 a preliminary meeting was held at Frankfurt, and zoologists from nearly a dozen German Universities were present. At present there are fifty-four members, and the next meeting is to be held on August 1, when a President will be chosen. The invitations to this meeting will be issued early in July. Applications for membership may be sent to Prof. Bütschli (Heidelberg), Prof. Victor Carus (Leipzig), or Prof. Spengel (Giessen). The foundation of this Society is a step in the right direction, and it is to be hoped that the new Zoologische Gesellschaft will speedily become as cosmopolitan as the sister anatomical one.

THE Norwegian Storthing, by 73 votes against 39, has voted a grant of 200,000 kroner for Dr. Nansen's North Pole Expedition.

THE third summer meeting of University Extension and other students will be held at Oxford in August next. The meeting will be divided into two parts. The first part of the meeting will begin with an inaugural address by Prof. Max Müller at 8.30 p.m. on Friday, August 1, and will end on Tuesday evening, August 12. The second part of the meeting will begin on Wednesday morning, August 13, and end on Tuesday evening, September 2. This period will be devoted to quiet study. The courses of lectures will be longer than those delivered during the first part of the meeting, and will deal in greater detail with the subjects then introduced.

SIR HENRY W. ACLAND has published a letter on "Oxford and Modern Medicine." It is addressed to Dr. James Andrew, and was printed originally for private circulation. In the preface the author expresses an earnest hope that "the broad and yet precise study of material science and of nature may prosper at Oxford, as part of the whole range of University thought, and that in the haste for technical education our physicians may not be relegated as some now desire into a professional class or clique by themselves, but be as formerly a living part of the whole of the scientific and literary University."

THE third International Shorthand Congress will be held at Munich from August 7 to 17. The centenary of F. X. Gabelsberger, the originator of modern German shorthand, will be celebrated by those who attend the meetings, and a bronze statue of him will be unveiled.

ACCOUNTS which have reached the *Times* from the Weatherby district, in Yorkshire, agree as to the occurrence of distinct earthquake shocks on Wednesday, June 25, about 10.30 p.m., and again on Thursday morning about four o'clock. Mr. John Emmet, of Boston Spa, sixteen miles from Leeds, states that shocks were experienced, not only at Boston Spa, but at Wighill, Clifford, Thorp Arch, Weatherby, and other places, and he

adds, "Crockery was heard to shake, and some of it was broken. Those who had retired to bed felt their houses and beds shaking, and rushed into the street. . . Those who were abroad in the street had to lay hold of something to keep them from falling."

Messrs. Macmillan and Co. have nearly ready for publication two works of great interest to students of ornithology, both of American origin. The first is a treatise on the "Myology of the Raven," intended as an introduction to the study of the muscular system in birds, by Dr. R. W. Shufeldt, of the Smithsonian Institution. The second is a revised re-issue, in one volume of convenient size, of the very valuable monographs on field ornithology and on general ornithology which were prefixed to Dr. Elliott Coues's monumental "Key to North American Birds." Part I., on field ornithology, contains the necessary instructions for the observation and collection of birds in the field, and for the preparation and preservation of specimens for scientific study. Part II. is a technical treatise on the classification, the zoological characters, and the anatomical structure of the class of Birds, in which the examples cited in illustration of the principles of ornithology have for the most part been re-drawn by the author from British instead of American birds.

Messrs. D. Marples and Co., Liverpool, have issued the presidential address delivered by the Rev. Henry H. Higgins at the meeting of the Museums Association, lately held at Liverpool. In the course of the address he gives an interesting account of the principal kinds of fittings and apparatus used in the Liverpool Museum.

Messrs. Mawson, Swan, and Morgan propose to issue a lithographed facsimile of an old manuscript volume of apothecaries' lore and household recipes, which was discovered some years ago amongst the papers belonging to the old firm of Gilpin and Company, chemists, Pilgrim Street, Newcastle. Careful examination, in which some of the curators of the British Museum have assisted, shows that the manuscript dates from about the time of Queen Elizabeth, additions having been made from time to time, in various handwritings, up to the middle of last century.

Under the auspices of the Royal Dublin Society, and partially aided by the Government, a scientific investigation of Irish fishing grounds is now being carried on upon the south-west and west coasts of Ireland. The Rev. W. Spotswood Green, Her Majesty's Inspector of Fisheries, Dublin, and Prof. A. C. Haddon, of Dublin, organized the expedition, which is expected to last four or five months. The screw steamer *Fingal*, of Glasgow, 160 tons register, chartered for the cruise, left Queenstown on May 7, having on board Mr. Green, Prof. Prince, Mr. T. H. Poole, of Cork, special surveyor to the expedition, and a crew of seamen experienced in trawl, net, and line fishing. Prof. Prince, who has conducted elaborate investigations upon the embryology of food-fishes at St. Andrews, and later on, Mr. E. W. L. Holt, also of St. Andrews Marine Laboratory, superintended the zoological department until Prof. Haddon was able to join the steamer. Dr. R. Scharff, of the Science and Art Museum, Dublin, and other gentlemen have temporarily assisted on board. The *Fingal* has been specially fitted up for the work. Several beam trawls (including patent forms), a quantity of mackerel nets, thirteen miles of long lines, large tow-nets (after Prof. McIntosh's pattern), microscopes and instruments for zoological and physical research, are included among the appliances. The coast from Cape Clear to Killybegs Bay (Donegal) has already been traversed, and about thirty stations have been tested and results of value obtained. In the open sea and in inshore waters the eggs and larval stages of mackerel, ling, gurnard, haddock, turbot, witch, and other species of food-fishes have been obtained, and a great variety of invertebrates, including some rare echinoderms, annelids, molluscs, &c., have been brought up in the dredge and trawl, the greatest depth tested up to this time being about 100 fathoms. The estuary of the Kenmare river, Dingle Bay, Smerwick, Birterburg, and Roundstone Bays, and the harbour of Clifden, proved to be very rich in invertebrate forms, specimens of *Synapta inhærens*, being abundant, while *Bonellia*, *Priapulus*, and many rare molluscs, *Lyonsia*, *Philine*, and various nudibranchs were procured. Copepods, larval crustaceans, medusæ, echinoderms, and ascidians occurred in such quantities as to frequently cause great inconvenience. A fine example of *Orthagoriscus mola*, nearly 9 feet in dorso-ventral measurement, was shot by Mr. Green and secured, and the rare Pleuronectid, *Arnoglossus gröhmanii*, was obtained in Clifden Harbour, the second specimen captured in British seas. Deep-sea dredgings will be taken, and it is expected that the reports, to be presented at the end of the cruise to the Royal Dublin Society, to the Irish Fishery Department, and the Government, will be of unusual scientific interest.

Some valuable notes on the progress of the coloured people of Maryland since the American civil war have been printed in the series of "Johns Hopkins University Studies in Historical and Political Science." The author is Dr. J. R. Brackett. He takes a more favourable view of the subject than is adopted by many American observers, and deprecates the idea of good citizens allowing "the coloured people to be condemned before the testimony is all in, at a fair, unbiassed trial." There are now a good many schools for coloured children, and conventions of coloured teachers are held. The most discouraging fact in connection with the progress that has been made is that everything has been gained by the energy of a few leaders. The coloured people, according to one of themselves, are "too spasmodic"; they are "too prone to grow tired in well doing."

The Town Gardening Committee of the Manchester Field Naturalists' Society, to which we lately alluded, has been vigorously prosecuting its work. The esplanade in front of the Manchester Infirmary and Albert Square, in which the Town Hall stands, are now decorated with seventy-five beautiful specimens of holly and aucuba, whose bright green leaves show up with good effect against the darkened stone of the neighbouring buildings. The plants have been placed in substantial but movable boxes 3 feet square and 4½ feet in height. The Parks Committee of the Manchester Corporation, of which Mr. Chesters-Thompson is chairman, has shown a laudable anxiety to carry out the plans suggested, and has contributed the greater part of the £500 already spent by the two committees on plants. It is hoped next year to carry on tree-planting on a large scale in the open spaces and streets of Manchester, and with a view to ensuring success under the extremely unfavourable atmospheric conditions peculiar to the city, the Town Gardening Committee is occupied in collecting all information relating to the subject, and will shortly issue a pamphlet of recommendations to those actually engaged in the work. Dr. Bailey, of Owens College, will contribute an essay on the effect of noxious gases on plants, and Dr. Poisson, of the Museum in Paris, will send a detailed account of the progress and experience gained in tree-planting in French towns. Several of our most distinguished botanists have also consented to act as corresponding members of the committee. It is probable that the movement will spread rapidly over the north of England, as the committee has already received official and unofficial requests for information about the work from Liverpool, Carlisle, Leek, and many other towns. The honorary secretary, Mr. C. J. Oglesby (16 Kennedy Street, Albert Square, Manchester), will be glad to receive additional information from anyone who may have had experience in the cultivation of trees and plants in manufacturing towns.

AMONG the papers read at the closing meeting of the Royal Society, was one by Prof. Ewing, of the Dundee College, entitled, "Contributions to the Molecular Theory of Induced Magnetism," in which experiments of a novel and curious kind were described, leading to an important conclusion. Prof. Ewing has examined experimentally Weber's theory of molecular magnets, according to which the molecules of iron are always magnets, which point anyhow in an unmagnetized piece, but are turned round to point one way when the iron is magnetized. It is well known that in the development of this theory by Maxwell and others there has been much difficulty in reconciling the results of the theory with what is known about the magnetic quality of iron and steel, and many arbitrary assumptions have been suggested in order to make the theory fit the facts. Prof. Ewing's experiments have removed this difficulty, showing that no arbitrary assumptions are necessary, and that the known character of the magnetizing process may be deduced from the molecular theory in its simplest form. The experiments were made by means of a model in which a large number of small pivoted permanent magnets are grouped to represent the molecular structure of iron. When a magnetic field is applied, the action of the small magnets on one another makes them behave in a way that exactly agrees with the observed behaviour of a bar of solid iron when it is magnetized. The model exhibits all the variations of susceptibility which are known to take place, and explains how magnetic hysteresis occurs without anything like friction among the molecules.

ACCORDING to the *Ceylon Observer*, Mr. A. T. W. Marambe, of Kandy, the translator of "Gulliver's Travels" and the author of "A Practical Synopsis of Ceylon History," has in preparation a little work on the Veddah language. Many have attempted this task before, but without success. Besides Veddah songs, a description of habits and customs, &c., the book will have a completer list of words than has hitherto appeared.

AN exceptionally pretty and instructive series of new experiments, upon the action of carbon heated to whiteness in the electric arc on various gaseous compounds, are described in the current number of the *Berichte* by Prof. Lepsius, of Frankfurt. Perhaps the most important are a group of four experiments illustrating the relative combining powers of the four elements iodine, sulphur, phosphorus, and carbon. The apparatus employed consists of a specially modified Hofmann eudiometer, one limb of which is 40 mm. in diameter and 300 mm. long, and the other longer limb narrower and furnished with a mercury reservoir at its upper end. The wider limb, which is the reaction-tube, is furnished with a stop-cock at the top, and just below this are two tubuli through which the adjustable carbon poles are inserted. At the base of the wider limb a second stop-cock is placed so as to permit of the adjustment of the mercury. The gas to be experimented upon is introduced into the apparatus at the upper stop-cock by allowing mercury to run out at the base. Four such eudiometers are arranged in a row, and 100 c.c. of gas introduced into each. Into the first, hydriodic acid is introduced; into the second, sulphuretted hydrogen; into the third, phosphuretted hydrogen; and into the fourth, marsh-gas. The gases thus stand at the same level in each of the four reaction-tubes. The current from a battery whose electromotive force should amount to 60–80 volts is then allowed to pass between the carbon poles, which are, of course, in contact at first, and then gradually drawn away until the maximum arc is obtained. Each reaction may be performed separately, or all four may be allowed to proceed simultaneously by adopting an arrangement in multiple arc. In hydriodic acid the brilliant arc light is tinted a magnificent purple and the whole space above the mercury becomes filled with violet vapour of iodine. Notwithstanding the considerable heating effect of the discharge, the volume of gas perceptibly diminishes, the liberated iodine rapidly depositing in minute crystals upon the walls of the tube. So rapid, indeed, is the diminution in volume, that mercury requires to be poured into the reservoir to prevent the entrance of air into the reaction-tube. In a very few minutes the reaction is complete, and the mercury ceases to rise. In sulphuretted hydrogen the light is coloured blue, and copious clouds of sulphur are produced, which settle upon the walls in the form of a white transparent coating. The volume of gas is considerably augmented, owing to the expansion by heat, and the reaction is likewise completed in a very brief space of time. In phosphuretted hydrogen the arc glows with a dazzling red light; the volume visibly augments at a rapid rate, and red clouds of phosphorus are thrown off, the glass walls being covered with red phosphorus, among which are to be found notable quantities of the ordinary yellow variety. The mercury attains its maximum height in the narrow limb in a minute at most from the moment of switching on the current. In the case of marsh-gas, the whiteness of the arc appears at first to be rendered more intense, and is surrounded by dense black clouds of carbon, which form a striking background. The upper part of the vessel, however, soon becomes covered with an opaque deposit, which perceptibly diminishes the brilliancy of the light. The volume appears to increase by leaps and bounds, and in a few seconds attains its maximum. At the end of the experiment, after cooling, the volume of hydrogen left in the first case is 50 c.c., in the second 100 c.c., in the third 150 c.c., and in the fourth case 200 c.c., thus showing in a most striking manner that an atom of iodine combines with one atom of hydrogen, sulphur with two, phosphorus with three, and carbon with four atoms of hydrogen.

THE additions to the Zoological Society's Gardens during the past week include a Bosman's Potto (*Perodicticus potto*) from West Africa, presented by Mr. P. S. S. Radcliffe; a Harnessed Antelope (*Tragelaphus scriptus* ♂), a —— Antelope (*Cervicapra* sp. in. ♂), two Marabou Storks (*Leptoptilus crumeniferus*) from Gambia, West Africa, presented by Dr. Percy Rendall; an English Wild Bull (*Bos taurus*, var.) from Chartley, Staffordshire, presented by the Earl Ferrers; a Ring-tailed Coati (*Nasua rufa* ♀) from Buenos Ayres, presented by Mr. C. W. Blacklock; two Tigers (*Felis tigris* ♂ ♀) from India, presented by H.R.H. the Duke of Clarence and Avondale; a Wedge-tailed Eagle (*Aquila audax*) from Australia, presented by Captain Salvin; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Alexander Finlay; two Nightingales (*Daulias luscinia*), British, presented by Mr. J. Young, F.Z.S.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on July 3 = 16h. 47m. 29s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4373	—	Greenish-blue.	17 58 19	+66 37
(2) 387 Birm.	5 6	Reddish-yellow.	16 40 33	+ 8 47
(3) π Herculis	3	Yellow.	17 11 12	+36 56
(4) ε Herculis	3·4	Bluish-white.	16 56 6	+31 5
(5) 202 Schj.	8	Very red.	17 23 15	−19 24
(6) V Boötis	Var.	Reddish-yellow.	14 25 20	+39 21

Remarks.

(1) This is the planetary nebula famous in the history of astronomy as the first nebula which was examined by the spectroscope. The nebula, though small, is remarkably bright, and the lines in its spectrum are at least as bright as those in the nebula of Orion. The three principal lines in the green, and the hydrogen line at G, are seen without any difficulty.

Vogel mapped two faint lines near λ 518 and 554, but these require confirmation. With a 10-inch refractor I have had no great difficulty in glimpsing these fainter lines, but I was unable to confirm their positions. The line at 518 is very suggestive of carbon, and that at 554 of manganese, and, if possible, comparisons with these substances should be made where a large aperture is available. I have very little doubt also, from my own observations, that there are many lines between F and G. Another observation of importance will be that of the character of the brightest line. Observers differ very considerably on this point, some maintaining that it is perfectly sharp on both edges, aud others that it is softened off on the more refrangible edge. For this observation it is not desirable to use high dispersion. In the General Catalogue the nebula is described thus:—"A planetary nebula; very bright; pretty small; suddenly brighter in the middle to a very small nucleus." Webb compares the telescopic appearance of the nebula with that of a star out of focus.

(2) Vogel describes the spectrum of this star as a very fine one of the solar type (Class II.*a*), whereas Dunér calls it Group II. According to the latter observer the banded spectrum is feebly developed, 2, 3, and 7 being very narrow, and the remaining bands appearing only as lines. From these observations it is not possible to say whether the star belongs to an early species of the group or a late one. In either case the bands would be narrow, but if the star be at an early stage the bright carbon flutings ought to be very manifest, and if at a late stage, there ought to be dark lines in addition to the narrow bands. Vogel may have mistaken the narrow bands for lines.

(3 and 4) These are stars of the solar type and of Group IV. respectively. The usual detailed observations are required in each case.

(5) This star, according to Dunér, has a well-marked spectrum of Group VI., the blue zone, however, being very feeble. The green and yellow zones are separated by a wide and dark band; the bands 4 and 5 are not visible. Further details or peculiarities should be looked for.

(6) This variable has a well-marked spectrum of Group II. (Dunér). The range is but small—7·0–9·4 in a period of 266 days—and it will be interesting to ascertain whether the bright hydrogen lines appear at maximum as in stars of greater range. Dunér states that though the spectrum is not a very bright one, its characteristics are by no means difficult to observe. There will be a maximum about July 6.

A. Fowler.

Annular Eclipse of June 17.—The number of the *Comptes rendus* for June 23 contains observations of this eclipse made at various Observatories. The Emperor of Brazil took the time of second contact at Nice Observatory; MM. Charlois, Javelle, and Perrotin those of first and last contact. At Lyons Observatory, M. Gonnessiat made some measures of the position-angle of the shadow. M. Trépied at Algiers succeeded in taking 26 photographs, the times of first and last contact also being noted. The maximum of the eclipse was indicated on the curves of a self-registering thermometer by a fall in temperature of 1°·4. Clouds prevented good observations at Meudon, but four photographs were taken by M. Trouvelot. M. de la Baume went from Meudon Observatory to Canea to observe the eclipse, and a telegram was received from him by M. Janssen stating that the weather was favourable, and that he had been able to obtain photographs of the ring and of its spectrum. M. Janssen also noted that one of the objects of the expedition to Canea was to obtain a photographic spectrum of the annulus, in order to see if the spectrum of the extreme edge of the sun's disk showed the bands of oxygen, and from the telegram received it seems probable that the question will be settled. The photographs obtained at Meudon show the granular structure of the solar surface so well visible during an eclipse, and the granulation can be traced right up to the edge of the moon, thus affording another proof of the excessive rarity of the lunar atmosphere.

Yarnall's Star Catalogue.—The Catalogue of stars observed at the United States Naval Observatory during the years 1845 to 1877, and prepared for publication by Prof. M. Yarnall, has been revised and corrected, and the stars renumbered by Prof. Edgar Frisby. In preparing this edition a re-examination of all annonymous stars has been made; the named stars have been compared with those of existing catalogues, the names being changed whenever necessary, and new names that existed previous to the publication of the Catalogue have been supplied. The errata in previous editions, pointed out by Profs. Holden, Krueger, and Millosevich, and Dr. Peters, have also been corrected, and the many notes to the Catalogue referring to the mistakes in the second edition, and the changes that have been made, indicate that the task of revision has not been a light one. As the object of the revision was merely for the purpose of correcting mistakes, no observations have been added or any unfinished observation completed, excepting such as were observed but omitted from the Catalogue, the apparent additions being found in some of the published volumes or in an unfinished state in the observing-books. The stars in the Catalogue have all been compared with standard catalogues as far as possible, and Prof. Frisby confidently believes that most of the mistakes have been corrected.

Photographs of the Surface of Mars.—Prof. W. H. Pickering, in the June number of the *Sidereal Messenger*, makes some remarks on fourteen photographs of the planet Mars taken by Mr. Wilson. Seven negatives were taken on April 9, between 22h. 56m. and 23h. 41m. G.M.T., and seven more were taken on April 10, between 23h. 20m. and 23h. 32m. Thus the same face of the planet was presented in both cases. Distinct and identifiable spots and markings are well shown in all the photographs, but in those taken on the latter date the white spot surrounding the south pole is seen considerably larger. It has been known for some years that the size of these polar spots varied gradually from time to time, apparently diminishing in the summer and increasing in the winter of their respective hemispheres. This, however, appears to be the first time that the precise date and approximate extent of one of these accessions has been observed. The appearances described are said to be so conspicuous upon each of the fourteen photographs that no one who had once seen them would have any difficulty in deciding on which of the dates any particular plate was taken.

Lightning Spectra.—Mr. W. E. Woods, of Washington, has used a Browning's pocket spectroscope to study the spectrum of lightning during a thunderstorm (*Sidereal Messenger*, June 1890). In several instances he observed what appeared to be bright lines superposed on a faint continuous spectrum; and in each case, when the continuous spectrum was bright enough to be seen, shaded flutings were visible. It is, however, much to be regretted that no diagram or statement as to the approximate position of the lines and fluting is given.

THE MARINE BIOLOGICAL ASSOCIATION.

At the annual general meeting of the Marine Biological Association, held at the rooms of the Royal Society on Wednesday, June 25, the following Report was submitted by the Council, and unanimously adopted. We omit only the list of those who went as a deputation to the Chancellor of the Exchequer on May 15.

The Council has met nine times during the past year, and the attendance has been fully up to the average of previous years.

The business transacted by the Council has had reference—

(1) To the maintenance and general efficiency of the Laboratory.

(2) To the prosecution of special investigations on economic subjects.

(3) To the financial position of the Association.

(1) It was found necessary to alter the communications between the storage reservoirs and the pumps of the Laboratory at Plymouth, and orders were given to Messrs. Leete, Edwards, and Norman, to supply a new valve-box, connection-pipes, &c. The cost of these alterations has been considerable, but it is satisfactory to note that the results have been very beneficial, and have produced a marked improvement both in the working of the pumps and in the water in circulation.

The Director reports that there was some little trouble over the sea-water in June and July 1889, during the hot weather, and during the alterations to the supply-pipes, which prevented more than one of the storage reservoirs being in use; but that since then, and especially after the alterations were completed, the water has been of admirable quality, and all the animals have done remarkably well.

Great improvement has lately been effected in the Aquarium at a very trifling cost, by hanging curtains between the top of the

fronts of the tanks and the ceiling, so that all the light reaching the spectator must pass through the tanks. Previous to this there appears to have been an excess of light in the tanks, and the fishes now appear to be much more comfortable, and keep nearer to the glass fronts.

The following fishes, molluscs, and crustacea have spawned in the tanks during the past year:—

The Plaice (*Pleuronectes platessa*).
The Flounder (*Pleuronectes flesus*).
The Pouting (*Gadus luscus*).
The Poor Cod (*Gadus minutus*).
The Rockling (*Motella tricirrata*).
The Lucky Proach (*Cottus bubalis*).
The Spotted Dog-fish (*Scyllium canicula*).
Chiton cinereus.
The Whelk (*Buccinum undatum*).
The Purple (*Purpura lapillus*).
The Sea-hare (*Aplysia punctata*).
The Sea-lemon (*Archidoris tuberculata*).
Goniodoris nodosa.
The Lobster (*Homarus vulgaris*).
The Crawfish (*Palinurus vulgaris*).
The Shrimp (*Crangon vulgaris*).
The Prawn (*Palæmon serratus*).
Idotea tricuspidata and *emarginata*,

as well as other species not so well known.

The *personnel* of the staff and servants remains unchanged, with the exception of the fisherman, W. Roach, who left in October. His place has been filled by E. G. Heath, a trawl fisherman of great experience.

The Council sanctioned the purchase, in July 1889, at a cost of £250, of a small steam-launch, the *Firefly*, which has been of great service. Being half-decked, and only 38 feet long, this launch is only suitable for local expeditions, and its purchase in no wise diminishes the necessity for a sea-going steam-vessel for carrying on investigations on food-fishes. The *Firefly* is very economical in coal and water, and has entailed no extra expense in working. The Association now possesses three boats, the *Firefly*, the *Mabel*, a three-ton hook and line fishing-boat presented by Mr. Bourne, and the *Anton Dohrn*, a rowing-boat bought in 1889.

Trawling, dredging, surface netting, and shore hunting have been carried on continuously during the year, and examples of interesting species, many of which are new to the district, have been added to the list since the last Report.

The standard collection of species is making good progress, the collection of Decapod Crustacea being remarkably complete.

(2) The researches on food-fishes and crustacea carried on under the direction of the Council have made considerable progress.

The Director of the Association, Mr. G. C. Bourne, has continued his observations on the pelagic fauna in the neighbourhood of Plymouth, and was also able, through the courtesy of Captain Aldrich, R.N., to make an expedition off the south-west coast of Ireland in H.M.S. *Research* in July last, for the purpose of comparing the surface fauna at the entrance of the Channel with that of the Channel itself. Some interesting observations have been made in connection with the presence of multicellular floating algæ in spring months and the presence of mackerel, which it is hoped may lead to practical results.

The Director has made observations and collected notes on the destruction of immature fish in various localities, and has been able, with the kind co-operation of the medical staff of the Deep Sea Mission to Fishermen, to arrange an extensive inquiry into the presence of immature fish in deep waters in the North Sea, their movements and destruction by beam trawling. This inquiry is in progress, and promises to be full of interest.

In connection with the destruction of immature soles in the estuary of the Thames, the Director has been making arrangements for keeping young soles in inclosed ponds with the view of rearing them to a marketable size, as is done in the Adriatic. For various reasons these experiments have been delayed, and are not yet in progress.

Experiments are also being made on the possibility of cultivating soles in fresh water, and it has been proved that the adult sole may be kept in fresh water.

In conjunction with Dr. G. H. Fowler, the Director has studied the natural history of the oyster, and through the kindness of Lord Revelstoke he has been able to arrange a series of practical inquiries on the natural history and propagation of the oyster in the River Yealm.

The Naturalist of the Association, Mr. J. T. Cunningham, has been chiefly occupied during the past year with a treatise on the common sole, which is now ready for publication.

Mr. Cunningham also has gathered much valuable information about the occurrence of the anchovy in English waters, and the possibility of an English anchovy fishery. A full account of the anchovy is given in the last number of the Journal, vol. i. No. 3.

In the early spring of this year, Mr. Cunningham made several expeditions to procure the ova of soles and other flat-fishes. He was able to secure and artificially fertilize a much larger number of soles' ova than on any previous occasion, and the fertilized ova were successfully hatched and the larvæ reared, up to the period of the absorption of the yolk-sac, in the aquarium.

On March 13 this year the plaice in the aquarium were found to be breeding. The Director and Mr. Cunningham collected a large number of their fertilized ova and transferred them to suitable hatching apparatus. The ova hatched out by March 18, and the larvæ were kept alive in specially isolated tanks till April 2. By this time the yolk-sac was completely absorbed, but the larvæ, although apparently healthy, could not be induced to feed. They died off very suddenly, evidently for want of food, on April 3 and 4, having lived fifteen days after hatching.

A second batch of ova was procured on March 28, and the eggs were hatched out on April 3 and 4. These larvæ were placed in a tank and fed with the pelagic organisms caught in the tow-net. They paid no attention to this food, so on April 22 they were fed with crushed crab, which they appeared to like, for on the following day their intestines could be seen full of food. In spite of this they began to die on April 24, and all were dead by the 26th.

Thus in the second experiment the larvæ were kept alive twenty days after hatching, a considerably longer period than in previous experiments at Plymouth, and, what is more important, they were induced to feed. These experiments show that some steps have been made towards success. None of the larvæ underwent metamorphosis, but Mr. Cunningham has procured some young plaice, flounders, and brill, already "flattened," and these are thriving in the tanks and feeding regularly.

Arrangements have been made with the Fishery Board for Scotland for carrying on an investigation on the food of the common sole, in connection with the work done by the Board on the food of other fishes.

Mr. W. Bateson was working on the sense-organs and habits of fishes, with the view of showing the possibility of using artificial or preserved baits in sea-fishing, from April to October 1889. The results of Mr. Bateson's investigations have been published in the Journal, vol. i. No. 3.

Mr. Weldon continued his investigations on the artificial rearing of lobsters last year. His experiments were apparently turning out successfully, when an accident caused the loss of his larvæ and apparatus. This year the artificial rearing of lobsters is being proceeded with by means of a different form of apparatus suggested by Dr. Fowler's successful method of raising the young of *Idotea*.

In addition to his experiments on lobsters, Mr. Weldon is engaged on important scientific investigations on the variation and natural history of the Decapod Crustacea, his expenses being, as before, met by the grant of £150 from the Government Grant Fund of the Royal Society, intrusted in 1887 by the Government Grant Committee to the President of the Association, the Hon. Secretary, Prof. Moseley, and Mr. Sedgwick.

The following gentlemen and ladies have been engaged on independent scientific researches in the Laboratory since the date of the last Report:—

Dr. G. H. Fowler (Studies in Descent), Mr. M. C. Potter (Marine Algæ), Mr. S. F. Harmer (Development of *Polyzoa*), Mr. T. T. Groom (*Cirrhipedia*), the Rev. Canon A. M. Norman, D.C.L. (Crustacean Fauna), Mr. A. O. Walker (*Amphipoda*), Prof. T. Johnson (*Floridea*), Mr. A. E. Shipley (*Gephyrea*), Dr. Hans Driesch, Jena (Heliotropism in *Hydroidea*), Mr. P. C. Mitchell (Histology of *Tunicata*), Mr. T. H. Riches (Nephridia of *Mollusca* and *Crustacea*), Mr. Herbert Thompson (Development of *Crustacea*), Miss Marion Greenwood, Newnham College, Cambridge (Physiological Studies), Miss L. Ackroyd, Newnham College, Cambridge (Morphology of *Nebalia*).

(3) Among the receipts of the past year the Council have to acknowledge the following subscriptions and donations:—£100 from Lord Revelstoke; £100 from Sir Henry Thompson; £100 from the Grocers' Company; £200 from the Fishmongers' Company (annual grant for five years); £500 from H.M. Treasury (annual grant for five years).

From annual subscriptions and compositions £143 was received, £61 interest on investments, and £150 for rent of tables and sale of specimens.

The expenditure, as shown in the Treasurer's account presented herewith, amounted to £2992, of which £398 was paid to Mr. Inglis for balance of his fees as engineer, £417 for structural alterations and additions, £112 for bait investigation, and £250 for a steam-launch.

The Association now has in hand, in cash and invested, £1398 2*s.* 11*d.*

The Council have great pleasure in acknowledging the generous assistance which has lately been afforded to the Association by the Fishmongers' Company, by Mr. J. P. Thomasson, M.P., and Mr. Frank Crisp.

The Fishmongers' Company, in addition to substantial grants which they have already made to the Association, have undertaken to contribute £400 per annum to the funds of the Association for a period of five years from the present date.

Mr. J. P. Thomasson has kindly offered a sum of £250, to enable the Council to retain the services of the Naturalist, Mr. J. T. Cunningham, for another year.

Mr. Frank Crisp has kindly given a sum of £120 (£60 per annum for two years) to meet the expenses of special investigations on the culture of sea fishes in inclosed ponds. The Council take this opportunity of placing on record their appreciation of the interest and confidence shown in the work of the Association by these liberal donations.

The thanks of the Association are due to Prof. Haeckel for a copy of his work on the *Siphonophora*; to Colonel Richardson, R.A., for a number of ichthyological works from the library of the late Sir J. Richardson; to Mr. J. W. Clark for back numbers of the Philosophical Transactions of the Royal Society and other books; to Messrs. J. and A. Churchill for the current numbers of the *Quarterly Journal of Microscopical Science*; and to Messrs. Agassiz, Giard, Marion, the United States Fish Commission, the Naples Zoological Station, the officers of the Norwegian North Atlantic Expedition, and other individuals and societies for copies of their publications.

The Council desire to express the indebtedness of the Association to the Council of the Royal Society for kindly permitting the Association to hold the periodical meetings of the Council and Association in their rooms.

In July and August 1889, the Council was in correspondence with the Fishery Board for Scotland and the Fisheries Department of the Board of Trade, with reference to the possibility of procuring scientific information on the alleged destruction of immature fish by beam trawling in deep waters.

Subsequently the Council determined to make an application to H.M. Treasury for a further grant of money in aid of special researches on food-fishes. The Chancellor of the Exchequer kindly consented to receive a deputation on the subject on May 15. . . .

The Council regret to have to announce that Prof. Huxley, who since the foundation of the Association has been its President, has found it necessary to withdraw from the office which he has held with so much honour and advantage to the Association. The Council desire to express their warm appreciation of the eminent services rendered by Prof. Huxley to the Association, and their great regret that he should be unable to continue his office.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Victoria University.—Last Saturday was Degree-day; the ceremony, presided over by Principal Rendall, the Vice-Chancellor, took place in the Manchester Free-Trade Hall. In the course of his speech, the Vice-Chancellor gave the following details as to the progress of the University:—

"A three-fold scheme for certificates, technical, commercial, and literary, has replaced the narrower project for technical certificates alone, and will be the means of giving University direction and attachment to numerous organizations which have hitherto lacked clearness of aim or recognition of results. The Manchester Chamber of Commerce has entrusted the examinations for its commercial certificate to the University. The local lectures scheme continues to thrive vigorously. In the last three sessions 21 courses, with an average attendance of 130, the large majority in or near Manchester, have been delivered under University auspices. The three colleges of the University are taking action, more or less concerted, for the establishment of day training colleges for primary teachers under the provisions of the new Education Code. Thus step by step the University is comprehending her mission and entering upon her heritage. Those who are forwarding the work may feel that impatience for quick returns which comes of convictions confident and energetic, but the observer and the historian will agree that in content and scope Victoria University has advanced with unparalleled rapidity. In all the colleges of the University building is in progress or in contemplation. At University College the Victoria Building for the arts department is advancing towards completion; at Yorkshire College funds have been raised for the erection of a medical department and other needed extensions; at Owens College further enlargement of the Medical School buildings is now under consideration."

As at Cambridge, the women students have done remarkably well this year, three out of four "first classes" in the B.A. honours schools and the Thomasson Prize for English Essay falling to their share.

St. Andrews University.—A Scholarship of the value of £30 a year has just been placed at the disposal of Prof. Percy Frankland at University College, St. Andrews University, by Miss E. F. Forster, of London. It is intended that the student holding the same shall devote the whole of his time to the prosecution of original research. The Scholarship, which will be known as "The Forster Research Scholarship," has been awarded for this year to Mr. John MacGregor, M.A.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 12.—"On the Position of the Vocal Cords in Quiet Respiration of Man, and on the Reflex-Tonus of their Abductor Muscles." By Felix Semon, M.D., F.R.C.P., Assistant Physician in charge of the Throat Department of St. Thomas's Hospital, and Laryngologist to the National Hospital for Epilepsy and Paralysis, Queen Square. Communicated by Prof. Victor Horsley, F.R.S.

The final conclusions arrived at by the author are as follows:—

(1) The glottis in man is wider open during quiet respiration (inspiration and expiration) than after death or after division of the vagi or recurrent laryngeal nerves.

(2) This wider opening during life is the result of a permanent activity of the abductors of the vocal cords (posterior crico-arytænoid muscles), which therefore belong not merely to the class of accessory, but of regular respiratory, muscles.

(3) The activity of these muscles is due to tonic impulses, which their centres receive from the neighbouring respiratory centre in the medulla oblongata. It is very probable that these impulses rhythmically proceed to the respiratory centre from the stimulation of certain afferent fibres contained mainly, but not exclusively, in the trunks of the pneumogastric nerves, and that they are in the respiratory centre changed into tonic impulses. The regular activity of the abductors of the vocal cords during life, therefore, belongs to the class of reflex processes. The permanent half-contraction of these muscles, in which form their tonic innervation is manifested, can be further increased, in concord with the general laws of the mechanism of respiration, by either volition or other reflex influences.

(4) In spite of their extra-innervation, the abductors of the vocal cords are physiologically weaker than their antagonists.

(5) These antagonists, the adductors of the vocal cords, have primarily nothing at all to do with respiration, and ordinarily serve the function of phonation only. Their respiratory functions are limited to—

(*a*) Assistance in the protection of the lower air passages against the entry of foreign bodies.

(*b*) Assistance in the modified and casual forms of expiration known as cough and laughing.

Physical Society, June 6.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Mr. H. Tomlinson, F.R.S.. read a paper on the effect of change of temperature on the Villari critical points of iron. This, he said, was a continuation of the paper he read before the Society on March 21, and the method employed was the same as then described (see *Phil. Mag.*, vol. xxviii. p. 394). Since then, however, he has made experiments at various temperatures up to 285° C., the temperature being determined from the resistance of a platinum wire whose temperature coefficient was carefully determined. The following table shows some of the results obtained with a well-annealed iron wire 1 mm. in diameter, which had been repeatedly heated up to 300° C., and cooled to the temperature of the room until the temporary permeability with various loads attained constant values at both temperatures.

Magnetizing force in C.G.S. units.	Load in kilogrammes for which permeability is the same as for unloaded wire at temperature				
	12° C.	76° C.	167° C.	244° C.	285° C.
2·84	4·7	5·0	5·3 and 12	5·7 and 10	None
3·70	2·5	3·2	3·6	4·2 ,, 11·5	4·7 and 9·9
4·8	1·8	2·5	2·7	—	3·1 ,, 12·3
7·69	None	None	None	None	None
10·40	,,	,,	,,	,,	,,
15·32	,,	,,	,,	,,	,,

Curves from which these numbers were obtained are given in the paper, and in these the load in kilogrammes, and percentage change of temporary permeability are plotted. From these curves and table it will be seen that if the first points in which the curves cut the load-line be considered, then at all temperatures the Villari values increase as the load decreases. If, however, the second points be taken, the critical values increase both with load and temperature. In both cases the Villari value is increased by rise of temperature. From the curves it follows that rise of temperature reduces the total variation of permeability produceable by loading. A table showing the temporary permeability of the unloaded wire at the various temperatures accompanies the paper.—A paper on the diurnal variations of the magnet at Kew, by W. G. Robson and S. W. J. Smith, was communicated by Prof. Rücker. In some preliminary remarks the Professor pointed out the great advisability of having the results of magnetic observations at various Observatories reduced and published in the same manner, and for the same periods. In order that this may be effected, the methods of reduction must be trustworthy, but not very elaborate. The Greenwich plan is too laborious to be generally adopted, but the method suggested by Dr. Wild (Rep. Brit. Ass., 1885, p. 78), in which the mean diurnal variation is obtained from measurements on five quiet days in each month, is feasible. With a view to further testing the trustworthiness of this method, the work described in the paper was undertaken. Mr. Whipple had made a comparison of the two methods for the years 1870–71–72, with the result shown in the following table:—

Minutes of arc.

$$K_s - K_w = 0{\cdot}7$$
$$G - K_s = 1{\cdot}2$$
$$G - K_w = 1{\cdot}6$$

where K_s is the mean diurnal range at Kew as obtained by Sabine's method, K_w that obtained by Wild's method, and G that obtained at Greenwich by the Greenwich method. He also found that the mean hourly differences followed some definite law. The authors undertook the reduction of the Kew observations according to Wild's method for the years 1883, 1886–87; the first was chosen as being a year of maximum sun-spots. The results give—

Minutes of arc.

1883 ... $G - K_w = 1{\cdot}5$
1886 ... $G - K_w = 1{\cdot}2$
1887 ... $G - K_w = 1{\cdot}9$

There is thus a difference of nearly two minutes in the variations at the two places, and this cannot all be accounted for by the method of reduction. Another peculiarity is that the range, as calculated by Wild's method, is greater by about 0′·5 than that obtained by the Greenwich method, although the latter includes days of moderate disturbance. The total range at both places has diminished by about 1′·6 between 1883 and 1887. The paper is accompanied by tables and curves plotted from the differences in the mean hourly readings at Greenwich and Kew for each of the above 6 years, and a marked similarity exists between all of them. The mean of the 6 curves differs in no case by more than 0′·4 from the curve for any year. It is thus possible to calculate the Greenwich values from the Kew numbers; and as these latter are published about two years sooner than the former, this fact may be very important. Referring to the reduction of results, Prof. Rücker said that the Stonyhurst observers and Prof. Mascart were willing to adopt Wild's method; Falmouth, he hoped, would follow suit, and Greenwich had been asked to publish their results in both ways. Mr. Whipple said that, before recording-instruments were available, and the numbers were obtained from separate experiments, the labour involved was considerable, and a single large disturbance or magnetic storm might vitiate the result of a whole year's work. Methods were therefore adopted to eliminate these disturbances; of these, that used by Sabine may be particularly mentioned. Although declination records have now been obtained for a considerable number of years, the cause of the variations still remains unknown. They do not seem to be dependent on temperature or on astronomical facts. He considered it valuable to obtain magnetic data from different parts of the earth, but comparisons were only possible when all are published on the same plan. This, he hoped, would result from the efforts of Profs. Rücker and Adams. When this is accomplished, the observations on magnetic force will need treatment; this work will be laborious, and the aid of volunteers like Messrs. Robson and Smith would be of great service. Prof. W. G. Adams said he was glad to see the satisfactory nature of the work which had just been brought before the Society. Usually, the mass of figures to be dealt with was so large that the mere reduction was a great undertaking. If, however, the difference between results obtained by the Greenwich and Wild's method was not more than 0′·4, it may be possible to make out the causes of the variations from observations reduced on Wild's plan. He himself would put more faith in horizontal force observations, and wished they could be worked out by some ready method. He hoped the one adopted in America, of obtaining mean curves by photography, might prove satisfactory. Prof. Perry asked if a machine could not be made to do the work. Mr. Whipple said such machines had been used by the Meteorological Office, but they were so elaborate and expensive that clerical work was just as cheap. The method of photographing mean curves had been tried at Kew, but it was open to the objection that accidental disturbances, such as those produced by the movement of iron in the vicinity and the approach of cabs, &c., were not eliminated. Mr. Boys, referring to the use of integrators, said that, for an harmonic analyzer, his disk-cylinder pattern was preferable to the ball-disk-cylinder integrators of J. Thomson, for it was much cheaper, and had less inertia. The President said the movement initiated by Prof. Rücker would be of great service if it resulted in the numbers obtained at the various magnetic Observatories being published in the same way. It was a great advantage to have such men, who were not permanently attached to an Observatory, to take up the subject and suggest improvements. The heads of such institutions were usually too much employed in making the necessary reductions to have time for devising improved methods. In his opinion, greater freedom should be allowed to the chiefs of Observatories, for it should be borne in mind that the object of observations is not to produce volumes of figures, but to increase our knowledge. Referring to the reduction of observations, he thought the voluntary services of senior physical students should be more generally accepted, and to this end he suggested that properly recommended persons should be allowed to spend some time in Observatories as honorary assistants. This would be of great use to the students themselves, and an advantage to the Observatories, for the reduction of observations could then be expedited. As regards the accidental disturbances referred to by Mr. Whipple, he contended that regulations should be adopted to render them impossible.

Zoological Society, June 17.—W. T. Blanford, F.R.S., in the chair.—Mr. Sclater exhibited and made remarks on a mounted head of a Pallah Antelope, obtained by Captain F. Cookson, on the Cunene river, in South-western Africa, which was distinguished by its black face from the ordinary form of the Cape Colony.—Mr. Sclater also exhibited a large photograph of Grévy's Zebra (*Equus grevyi*), taken from the specimen in the

Natural History Museum at Paris by Mr. Gambier Bolton.—A specimen of Pallas's Plover (*Ægialitis asiatica*), obtained in May last near Great Yarmouth, and now in the Norwich Museum, was exhibited; and a note upon its occurrence by Mr. T. Southwell was read to the meeting.—A communication was read from Prof. F. Jeffery Bell containing some notes received from Mr. Edgar Thurston, of the Madras Museum, on the habits of the Pennatulids of the genus *Virgularia*.—A communication was read from M. P. A. Pichot, containing exact particulars of the locality on the Lower Rhone in which the Beaver is still found in its native state.—Mr. W. Bateson read a paper on some cases of repetition of parts in animals, and exhibited a series of specimens illustrative of this subject.—Mr. Henley Grose Smith gave an account of the Diurnal Lepidoptera collected by Mr. W. Bonny, of the Emin Relief Expedition, on the river Aruwimi, Central Africa.—A communication was read from Mr. W. L. Distant, containing descriptions of some Hemiptera collected by Mr. W. Bonny during the same expedition.—A communication was read from Mr. H. W. Bates, F.R.S., on some of the Coleoptera collected by Mr. W. Bonny during the same expedition.—Mr. Herbert Druce read the descriptions of ninety-five new species of Lepidoptera Heterocera from Central and South America.—Mr. G. A. Boulenger pointed out the secondary sexual characters in the South African Tortoises of the genus *Homopus*.—A communication was read from Mr. W. L. Sclater, containing a series of critical notes on the Indian species of the family Muridæ.—A communication was read from Mr. J. T. Cunningham, containing some notes on the secondary sexual characters of the genus *Arnoglossus*. The author showed that the so-called *Arnoglossus laterna* is only the female of *A. lophotes*.—Mr. R. Bowdler Sharpe read the sixth part of his series of notes on the Hume Collection of Birds. The present communication treated of the Coraciidæ of the Indian region, and contained descriptions of three new species.—A communication was read from Miss E. M. Sharpe, containing an account of a collection of Lepidoptera made by Mr. Edmund Reynolds on the rivers Tocantins and Araguaya, and in the province of Goyaz, Brazil.—Mr. Edmund S. Hall gave an account of the occurrence of a persistent right posterior cardinal vein in a Rabbit.—This meeting closes the present session. The next session (1890–91) will commence in November 1890.

PARIS.

Academy of Sciences, June 23.—M. Hermite in the chair.—On the partial eclipse of the sun on June 17, by M. J. Janssen.—Theory of the motion of fluids near to the wide opening of a delivery pipe, where the liquid threads have not acquired their normal inequalities of velocity, by M. J. Boussinesq.—Comparison of the theoretical figure of a storm given in the *Comptes rendus* of June 9 with the facts known to navigators, by M. H. Faye.—The work and progress of the Arago Laboratory in 1890, by M. de Lacaze-Duthiers.—On the visible and photographic spectrum of the great nebula of Orion, by Dr. W. Huggins.—On the distribution of *Salmo quinnat* on the Mediterranean coasts of the south-east of France, by MM. A. F. Marion and F. Guitel.—On the glycolytic power of blood and of chyle, by MM. R. Lépine and Barral.—Observations of Brooks's comet (March 19, 1890) made at Bordeaux Observatory, by MM. G. Rayet, Picart, and Courty. Observations of position are given extending from May 19 to June 20, being in continuation of those published in the *Comptes rendus* of March 31, April 8, and May 19.—Elements and ephemeris of the new minor planet (293) discovered at Nice Observatory on May 20, by M. Charlois.—Partial eclipse of the sun of June 17, in the morning, observed at Nice, by M. Perrotin.—Observation of the eclipse of the sun of June 16–17, made with the Brunner equatorial of Lyons Observatory, by M. Gonnessiat.—On the partial eclipse of June 16–17 (Algiers Observatory), by M. Ch. Trépied.—The solar eclipse of June 17, by M. E. L. Trouvelot. (For eclipse observations see Our Astronomical Column.)—On the international zero of altitude, by M. Ch. Lallemand.—On a direct-reading dynamometer, by M. G. Trouvé.—Reciprocal action of alkaline haloid salts and mercurous salts, by M. A. Ditte.—On some phosphates of lithium, beryllium, lead, and uranium, by M. L. Ouvrard. A number of double phosphates formed by the action of molten alkaline phosphates upon the carbonates of lithium and glucinum and the oxides of lead and uranium are described; among them occurs a double phosphate of beryllium and sodium corresponding in composition with the recently discovered mineral beryllonite.—Combinations of double chlorides of phosphorus and iridium with arsenious chloride, by M. G. Geisenheimer. By heating the constituents in a sealed tube to 250°, ruby-red prismatic crystals of $2(Ir_2P_3Cl_{15}).5AsCl_3$ are formed. — On the sub-fluoride of silver, by M. Guntz. The existence of a sub-fluoride of silver was indicated by the analyses of a precipitate produced on the negative pole when subjecting a hot saturated solution of silver fluoride to electrolysis, employing a very strong current and silver electrodes. The pure salt is obtained in quantity by heating finely divided silver with a saturated solution of silver fluoride on a bath to a temperature of from 50°–90°. Analyses of the product prove it to be the sub-fluoride of silver Ag_2F.—A contribution to the study of ptomaïnes, by M. Œchsner de Coninck.—On the preparation of wine ferments, by M. A. Rommier.—On the sense of smell in star-fishes, by M. Prouho. The author concludes that star-fishes excited by the presence of a bait are guided by sensations perceived by the extremities of their arms. The sense of smell is not diffuse in star-fishes, but is localized in the limbs useless for locomotion at the back of the ocellary plate.—The photographic registration of the chlorophyll function by the living plant, by M. Timiriazeff.—On the hypersthene andesites and labradorites of Guadaloupe, by M. A. Lacroix.—On the vertical circulation in the deep ocean, by M. J. Thoulet.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Three Years in Western China: A. Hosie (Philip).—Encyclopædia of Photography, Part 1: W. E. Woodbury (Iliffe).—Advanced Physiography (Physiographic Astronomy): J. Mills (Chapman and Hall).—A Manual of Orchidaceous Plants, Part 6 (Veitch).—Text-book of Physiological and Pathological Chemistry: G. Bunge, translated by Dr. C. L. Wooldridge (K. Paul).—In Darkest Africa, 2 vols.: H. M. Stanley (S. Low).—The Aborigines of Tasmania: H. Ling Roth (K. Paul).—Ostéologie Ropuch (Bufo Laur.): Prof. Dr. F. Bayer (V. Praze).—Uhlonosné Útvary v. Tasmanii; Prof. Dr. O. Feistmantel (V. Praze).—Abhandlungen der Math. Naturw. Classe der K. B. Gesellschaft der Wissenschaften, 1889-90, vii. Folge, 3 Band (Prag).—Annales de l'Observatoire de Moscou, ii. Série, vol. 2, Livre 1 and 2 (Moscou).—Annales de l'Observatoire de Nice, Tome 3, Texte et Atlas.—Sun-dial, adjustable for all Latitudes (Philip).—A Theory of the Sun's Radiation of Heat: W. Goff (Stanford).—Publication of the Leander McCormick Observatory of the University of Virginia, vol. 1, Part 4, Double Stars 1885-86 (Virginia).—Mind and Matter: O. Barnard (J. Heywood).—Proceedings of the Society for Psychical Research, June (K. Paul).—Journal of the Royal Microscopical Society, June (Williams and Norgate).—Records of the Geological Survey of India, vol. xxiii. Part 2.—Annalen des K. K. Naturhistorischen Hofmuseums, Band 5, No. 2 (Wien, Hölder).

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THURSDAY, JULY 10, 1890.

LIFE OF SEDGWICK.[1]

II.

The Life and Letters of the Reverend Adam Sedgwick, LL.D., D.C.L., F.R.S., Fellow of Trinity College, Cambridge, Prebendary of Norwich, Woodwardian Professor of Geology, 1818-73. By John Willis Clark, M.A., F.S.A., and Thomas McKenny Hughes, M.A., F.R.S. Two Volumes. (Cambridge: University Press, 1890.)

THE main results of Sedgwick's geological work, as stated in these volumes, are briefly as follows. Passing over several contributions, often of permanent value, to the geology of the crystalline rocks of Cornwall and of the Carboniferous system, especially in the north of England, we come first to his monograph on the Magnesian Limestone and lower portion of the New Red Sandstone series. Of this Prof. Hughes justly says: "It is at once broad and minute: broad in its generalizations—for it places in order a complex group of rocks, which, until it was written, were in complete confusion; and minute in working out, through the whole of the district selected, from Nottingham to the southern extremity of Northumberland, the boundaries of the different formations and their relations to each other." We are not, however, prepared to follow Prof. Hughes, if we understand him rightly, in his objection to the name Permian as designating the lower part of this series, for the break between that formation and the so-called Trias is probably more important than at first sight appears, and New Red Sandstone is a name obviously provisional.

Sedgwick undertook a task of unusual difficulty in investigating the rock masses which enter into the "Cumbrian mountains," and ascertaining their relations with the strata in adjacent regions, but it was so successfully accomplished that subsequent observers have made few if any important changes, though of course they have amplified many details, in his results.

His work also among the Palæozoic rocks in Devonshire and Cornwall, in which, after a time, he was joined by Murchison, was a long and arduous task, at first productive of much controversy. In this, others, as it is said, "helped with facts or with useful criticism," but it seems a fair statement of the result to say that "the now received classification of the Devon rocks remains as Sedgwick and Murchison left it: Culm measures (Carboniferous) above and Devonian below; the base of the Devonian being there unknown."

Sedgwick's work in Wales commenced in the year 1831. It was a task from which the boldest geologist might well have recoiled. The country was comparatively difficult of access, the maps were not good, little help was to be obtained from palæontology; the "greywacke" rocks were a vast *terra incognita*. But the "stiffer" the problem, the greater its attraction for Sedgwick. In that and the following years he unravelled the complicated structure of North Wales, and placed in order the great rock masses, from the base which he established in the neighbourhood of the Menai Straits, under the great group named after the town of Harlech, to the top of that which he called the Bala group, clearly distinguishing the latter from the Denbigh Grits and other rocks, which are now universally recognized as Silurian. Of this part of Sedgwick's work, Prof. Hughes gives a lucid history, of which the following is a brief outline.

Sedgwick spent the summer of 1831 in North Wales, and established the succession of the rocks from his base line upwards, across Snowdon and the Merionethshire axis. Short accounts of his results were laid before the Cambridge Philosophical Society and the British Association in June 1832. Next month he sent from Wales sections which illustrate the stratigraphical succession from the Menai Straits to the Berwyns. Thus, to quote Prof. Hughes's words:—

"Sedgwick, by 1832, had explained the geological structure of North Wales; had sketched out the leading subdivisions of the Cambrian rocks, and had established the correct sequence of the Arenig and Bala series, and placed them in true relation with what were afterwards known as the Silurian (Upper Silurian of Murchison) in Central Wales and the borders."

Later in the autumn of this year we find that he "had ascertained the exact position of the Wenlock limestone south of Llandovery," and drawn a rough section, "correct as far as it goes," from the Lower Bala beds, in the valley of the Towy, to the Old Red Sandstone. Further communications, as the result of this prolonged labour in the field, were laid before the British Association and the Cambridge Philosophical Society in 1834. From time to time during the next twelve years important details were worked out; perhaps the most marked advance being made in the difficult region of Central Wales, between the Towy and the sea, where Sedgwick succeeded in establishing the general succession of the strata, obscure and almost unfossiliferous as they are. In 1851 he practically proved that the name Caradoc which had been used by Murchison in "The Silurian System" (published in 1839, from work begun in 1831) must cover two distinct groups, one containing Bala fossils, the other clearly underlying the Wenlock group, with fossils similar to those in the latter, but without those characteristic of the former. This is actually demonstrated in a paper published in 1852.

Soon afterwards began, at any rate openly, the difference regarding the limits of the Cambrian and Silurian systems which unhappily estranged him from his friend and from the Geological Society. The immediate cause appears to have been the publication, by the Geological Survey, of a map of North Wales, on which the colours used to distinguish Silurian rocks were extended over a large part of those hitherto described by Sedgwick as Cambrian. Why this was done, it is now difficult to understand. Between the base of the Cambrian and that of the Old Red only one well-marked physical break exists—that at the base of the May Hill Sandstone (Upper Llandovery). Below this is only a palæontological break, which at that date had not been clearly recognized. Accordingly,

[1] Continued from p. 219.

as time went on, the lower limit of the Silurian system descended, like a stone sinking in the mire, till at last "Lower Silurian" actually included the Menevian rocks, as may be seen to this day at the Museum in Jermyn Street. This being so, one would have thought that, even if Murchison had preceded Sedgwick in the publication of the results of his work—which was not the case—the two vital errors in his reading of the beds between the base of the Wenlock and that of the Arenig ought to have deprived him of any claim on account of prior nomenclature. Sedgwick had placed the beds of his Cambrian system in right order from base to top—that is, to beneath the so-called Upper Llandovery. These facts appear to be fully proved, and thus Sedgwick had good cause to feel aggrieved. Into the more personal aspect of the controversy it is needless to enter. One cannot greatly wonder that when once a rift opened in the lute it quickly became a rent, for the two men were so unlike, both in their excellencies and their defects. Nor can it be denied that Murchison had his grievances. Sedgwick was vexatious as a coadjutor in the preparation of papers, for he was unpunctual and unready; he was also slow in duly publishing the results of his own labours, contenting himself too much with informal communications to the British Association and the Cambridge Philosophical Society, instead of laying carefully written memoirs before the Geological Society. But it must be remembered that his time was much occupied. His fellowship, his professorship, his prebend—all entailed duties which were often heavy; and Sedgwick was too honourable a man not to give "a full pennyworth" to those who bought from him. He had to work to live, for he had no private means. It cannot, however, be denied that he interpreted too literally the precept, "Whatsoever thy hand findeth to do, do it with all thy might." Social engagements, political contests, University disputes, too often turned his attention from the main work of his life, and gave some ground for Lyell's severe remark:—"He has not the application necessary to make his splendid abilities tell in a work. Besides, everyone leads him astray; . . . to become great in science, a man must be nearly as devoted as a lawyer, and must have more than mere talent." Still it must be remembered that Sedgwick's health, notwithstanding the great age to which he attained, was far from good, and his constitutional ills were those which make continued sedentary work extremely trying. He was also unlucky in the way of accidents: a dislocated wrist, a broken arm, bad falls, an eye permanently injured, make up a large catalogue of damages for a Cambridge Professor.

Still, although I take Prof. Hughes's view as to the rights of the case, I cannot, under existing circumstances, agree with his condemnation of the proposal to give a new name —Ordovician—to the beds between the base of the May Hill Sandstone and that of the Arenig. "One shell is given to Sedgwick, another to Murchison, but who gets the oyster?" A smart remark, doubtless, but like many such rather misleading. There is no question of shell or oyster in the matter. Each part is equally edible—or indigestible. Granted that Sedgwick has the better title, possession, in questions where the right is not wholly on one side, counts for something with practical men. Cambrian also, as defined by Sedgwick, is rather disproportionately large, and the palæontological break beneath the Arenig is more marked than that which severs the Cambrian from the Silurian. I venture to think that, apart from personal questions, a tripartite division would be pronounced most in accordance with the principles of geological classification, and should not be surprised, if this be repudiated by Sedgwick's defenders, at the ultimate disappearance of Cambrian in the omnivorous maw of Silurian.

Sedgwick's permanent estrangement from the Geological Society I venture to think a mistake. Doubtless he had good cause for indignation at his treatment by its Council, and the well-meant, but arbitrary, action of one of its officers. But a Council is only a temporary aggregate of individuals, and the offence after a time should have been condoned. Its members did not really understand the question at issue; they were evidently actuated, not by any desire to be unjust, but by a nervous anxiety to keep the peace, and forgot, as men so often do, that when a sore is hidden under a plaster, it commonly festers. So the event proved in this case: molluscous amiability met with its usual reward. If the combatants had "fought it out," fairly and honourably, there would have been more chance of an ultimate reconciliation.

These interesting volumes enable us better than ever to estimate Sedgwick's place among the geologists of his generation. His especial strength lay in stratigraphy. In his power of unravelling a complicated district by attention mainly to physical evidence he has never been equalled. He was a patient and unwearied collector of facts, with a wholesome dread of viewing them "through the distorting medium of an hypothesis." Yet it must be admitted that his judgment was often warped by prejudice, using the word in its technical sense. His great power is best displayed when he attacks a problem which is completely novel; for, reformer though he was in politics, his mind, in scientific matters, had a distinctly "conservative" bias, and was too much influenced by ideas which had no better authority than tradition. Of this defect the book records several instances. It will suffice to mention his opposition to Lyell's "Principles of Geology" and to Darwin's "Origin of Species." It is of course possible to overstate the doctrine of uniformity and misuse the hypothesis of evolution; but the progress of knowledge has not justified Sedgwick's attacks on the main arguments of these works, and it must be admitted that he was inferior to their authors in power of inductive generalization. Perhaps no better example could be found of Sedgwick's strength and weakness than his well-known paper "On the Structure of Large Mineral Masses," where a magnificent co-ordination of facts has a somewhat disappointing conclusion.

But even if we grant defects in the geologist as in the man, it is impossible to deny his real greatness. Those who loved Sedgwick as a friend are fast becoming few; but the number of those who reverence his memory as that of a master in science is likely to increase rather than to diminish as his work is weighed in the balance and tested by time. To myself, though I did not know him in his prime, he always appeared to be not only truly noble in spirit, but also illuminated with that divine fire which distinguishes the man of genius from the man of talent.

T. G. BONNEY.

MEASLES AND STRAW-FUNGI.

The Prevention of Measles. By C. Candler. (Melbourne, Victoria. London: Kegan Paul, Trench, and Co., 1889.)

NOTWITHSTANDING the amount of labour which Mr. Candler has expended upon this work, and the ingenuity of some of his hypotheses, we cannot but think that his method might almost be taken as an example of how an inquiry of this kind ought not to be conducted. The author starts with an account of the observations of Dr. Salisbury, an American physician, published in 1862, by which he claimed to have established that a disease called "camp measles," prevalent among American soldiers, was produced by infection with certain fungi derived from musty straw. Salisbury cautiously abstained from positively asserting that the disease was identical with common measles, but said he could see no difference between them; and that an attack of the former protected from the latter. If the diseases were identical, his explanation applied to common measles.

This hypothesis of Dr. Salisbury's was very carefully examined by Dr. J. J. Woodward, Dr. Pepper, and others, who came, by experiment and reasoning, to the conclusion that Dr. Salisbury had not proved his point; and the theory that straw-fungi are the cause of measles has been generally discredited.

Mr. Candler thinks that the refutation of Dr. Salisbury's theory was not complete; and, falling into the not uncommon fallacy that "not absolutely disproved" is equivalent to "proved," he treats it as if it were certainly established, and proceeds to build further hypotheses upon it.

This we consider to be an inversion of the right method of procedure in science. Supposing that Salisbury's results suggested matter for further inquiry, the proper way to begin would be by testing their soundness. If Mr. Candler had himself repeated, or got some scientific friend to repeat, Salisbury's experiments with mouldy straw derived from a place where measles was rife, he might have obtained results, either positive or negative, of great value; and would certainly have made a more important contribution to the subject than is contained in the present volume.

Mr. Candler further extends the straw-fungus theory by supposing that the fungi become changed into bacteria in the body; and, indeed, uses Salisbury's untested and unrepeated experiments as a proof of one of the most fundamental questions (if it be a question) of biology—namely, the alleged genetic relation of fungi and bacteria.

The author's argument is so characteristic of his book that we venture to state it formally thus. Salisbury, by injecting fungus-dust from mouldy straw into himself and others, produced a disease resembling measles. But all such diseases are produced by "pathophytes," *i.e.* bacteria. Therefore Salisbury "*caused pathophytes to develop from fungi*" (the italics are the author's) "and demonstrated that cardinal point in dispute in regard to the bacteria."

An easy solution indeed! if, at least, it were proved that the dust of mouldy straw contained no bacteria (though such are pretty certain to be present), and if it were proved also that fungi by themselves cannot produce specific diseases (though some such diseases are well known in the lower animals, and are not quite unknown in man).

But even granting these points, surely the experiment might be repeated at least once before it is made a corner-stone of cryptogamic botany!

The dangerous fungus of measles Mr. Candler believes to lurk in damp and mouldy straw palliasses; and rejecting altogether the idea of contagion, he believes that measles is entirely due to the use of straw bedding imperfectly aired. Towards the end of the book the author begins to tread on firmer ground than at the beginning, for he bases his conclusions on some induction from facts.

In the great epidemic of measles in Victoria in the years 1874-75, he affirms that he could not discover any instance of measles in a dwelling from which damp straw (in the form of bedding) had been excluded, but in every house where measles occurred, the presence of damp straw in the bed-rooms was easily made out. Some curious instances of exemption, especially in the case of public institutions, such as asylums and the like, are quoted, and we seem to be on the verge of a systematic collation of evidence. But the result is disappointing, as the enumeration of instances is altogether inadequate to establish a general law. It is strange that Mr. Candler makes so much of the exemption of lunatic asylums from measles, to account for which he has recourse to elaborate explanations of the use of straw bedding. Surely the exemption of persons shut up in asylums, prisons, &c., from contagious epidemic diseases, is a very familiar fact, and easily explained. Such persons receive few visitors, and what is to the point here, lunatics especially are seldom or never visited by children, who are the chief carriers of the measles-contagium. Nor can we say that the author is more successful in explaining on his theory the great epidemics of measles in Fiji and in Japan.

Mr. Candler's book is written with much earnestness, not without candour, and contains many curious facts, though it fails to prove its main contention. There is nothing impossible in the supposition that damp straw favours the growth of "microbes"; and it might conceivably be proved by sufficient evidence that this is a favouring or even a necessary condition for the growth of the specific virus of measles. The objection is that the evidence is quite inadequate. Moreover, were such a law established, it would by no means prove that the cause of measles was a fungus, since it might just as well be a bacterium or other living thing.

In the meantime it cannot do harm and may do much good to draw attention to the insanitary consequences which may follow the use of straw bedding. A straw palliasse unchanged and undisturbed for years is not a desirable article of furniture, and housekeepers will do well to turn such things out of their bed-rooms. Fortunately, in this country they are being rapidly superseded by steel mattresses; and on inquiry at the large furnishing houses we find that few palliasses are now sold. We shall see whether measles becomes thereby extinct.

J. F. P.

SPIDERS' WEBS.

American Spiders and their Spinning Work: a Natural History of the Orb-weaving Spiders of the United States, with Special Regard to their Industry and Habits. By Harvey C. McCook, D.D. Vol. I., pp. 1–372, and 353 Woodcut Figures. (Philadelphia: Allen Lane and Scott, 1889.)

ALTHOUGH much has been written in a more or less fragmentary way by various authors, on the spinning organs and geometric snares of spiders, as well as on the method of entrapping their prey, the present volume is the first in which all that has been before touched upon is brought together in any systematic manner. Two other volumes are intended to follow, but the one under notice completes the subject of geometric web-spinning. In Vol. II. it is purposed to deal with the habits and industry of spiders, associated with mating, maternal instincts, the life of the young, distribution of species, and other general habits; while in the third (and concluding) volume the whole of the geometric spiders—"orb-weavers"—of the United States will be treated of systematically, and illustrated by numerous coloured plates. It might have been thought that Vol. III. would have more naturally preceded the other two; but perhaps it is scarcely fair to criticize too closely the form in which an author chooses to present his subject. Dr. McCook's evident aim is to popularize the subject of spiders' web-spinning, and all that relates to it. This is shown not only by the way in which the subject is presented, but by the bestowal of English trivial names at every turn; though it may well be doubted how far science is really advanced by thus cumbering its nomenclature. Among the most interesting portions of the present volume are those in which some snares are described, combining the geometric or *Epeirid* type with that of the *Theridiidæ*, and of which no examples have yet been found in Great Britain. Space, however, forbids our going into details of these, nor, in fact, of any part of the work. The whole volume is a mass of details, evidently the result of careful and long-continued observations; and made patent not only to the mind by lucid description, but to the eye by the very graphic illustrations thickly scattered over its pages. On one point, of very great interest in the making of geometric snares—the formation of the portion studded with viscid globules—Dr. McCook approaches very nearly to a solution of the method by which these globules are placed on the lines, but the method[1] itself appears to have as yet escaped observation.

Dr. McCook tells us that his first intention was "to write a natural history of all American spiders," but no one who has gone even a little into the spider fauna of that large region will wonder that, when this intention came really to be grappled with, the plan changed; and probably those interested in the study of spiders have gained by the exchange. The work done in this volume is divided into seven parts. Part I. treats of the general classification and structure of spiders and their spinning organs; Part II. of the general characteristics, construction, and armature of webs; Part III. is on characteristic forms and variations of snares; Part IV. on certain geometric webs devoid of viscid globules, and on "spring snares"—those singular arrangements in which the spider holds the snare taut by a single line with the slack gathered up in its claws, and, on an insect striking the web, suddenly lets the slack go with a spring, to the more certain entanglement of the prey. In Part V. we have a detail of many curious facts bearing upon the skill and intelligence of spiders, and also as to the mechanical strength of their webs and their physical powers; but some of the most curious of these details, in regard to the "engineering skill" of spiders are, no doubt rightly, set aside by the author, so far, at least, as their bearing on such skill is concerned. Part VI., under the head of "Provision for Nurture and Defence," treats of the methods of using their snares in procuring food, and on the effects and uses of the poison secreted in the falces of spiders; and the volume concludes with Part VII., in which the "nesting habits" of geometric spiders are gone into, as also the origin, use, and development of nest-making in various tribes of spiders; and the "genesis of snares," under which last head the author gives us his views as to the steps by which a simple line may have become the complicated snares now formed by these spiders.

The volume thus completed is well got up, and, abounding in interest from beginning to end, may well stir up in everyone to whom spiders are not (and it is to be regretted they sometimes are) objects of abhorrence, a wish for the speedy appearance of the remainder of the work, Vols. II. and III., the proposed contents of which have been noticed above.

O. P. C.

[1] Cf. a paper on this subject by — Apstein, "Bau und Function der Spinnendrusen der Araneida," *Archiv für Naturgeschichte*, 1889, p. 29.

NATIONAL HEALTH.

National Health. By B. W. Richardson, M.D., F.R.S. Abridged from the "Health of Nations," of Sir Edwin Chadwick, K.C.B. (London: Longmans, Green, and Co., 1890.)

THE aim of this work is sufficiently explained in the preface, in which the editor states that his object has been to condense, without comment, into a single handy and cheap volume, the most practical and most popular parts of Sir Edwin Chadwick's "Health of Nations."

The volume opens with a biographical sketch of the author of the larger work, giving an interesting and detailed account of his important life-work in public health and sanitation; the remainder of the work being divided up into four sections, dealing respectively with health in the dwelling-house, in the school, the health of the community, and health in the future.

The first section, relating mainly to the dwellings of the working classes, is devoted to an inquiry into the serious consequences to health of unsanitary surroundings, such as overcrowding, want of ventilation, deficient water-supply, and imperfect drainage, especially when, as is often the case in houses of the poorer classes, the walls are pervious and absorbent through faulty construction and the use of bad materials. The author points out that as good house-drainage and complete sanitary work has proceeded in old houses, low health has immediately improved; a similar improvement becoming visible at the same time in the moral as well as the physical con-

dition of the people. A number of pages are taken up with a description of an ideal water-supply and methods of drainage, great stress being laid on the necessity for laying down drains and sewers of the smallest possible size consistent with the immediate removal of the maximum flow at any one time. The wisdom of such a plan is now admitted on all hands, the powerful flow preventing all deposit, and by maintaining a down draught from the houses, avoiding the ingress of sewer-gas.

In the section on "Health in the School," we find an account of the "Half-time System" initiated by Sir E. Chadwick with the object of ensuring to children employed in manufactories a certain time for school-work and recreation, in addition to that devoted to physical labour. The time which should be occupied by lessons at various ages, and the effect of good lighting, warming, ventilation, and personal cleanliness in augmenting the receptivity of pupils, are ably discussed, and the value of military drill as a part of the education is rightly insisted on. The methods for the prevention of the occurrence and spread of epidemics are so briefly touched upon, that we cannot but think that the importance of the subject might have demanded somewhat fuller treatment.

The most important portion of the following section deals with the results of occupation and surroundings on the length of life in various classes of society, the effects of intemperance and of bad feeding being specially considered; the author, however, being careful to point out the sources of fallacy to which all such statistics are liable. The last portion of the book is mainly devoted to an attack on the Malthusian theory.

The work is not, and does not in any way pretend to be, a student's text-book, so that the candidate for a diploma in public health will hardly find it of much value, except, perhaps, from an historical point of view. Still, there is much in its pages which may be studied with advantage by those interested in matters pertaining to general hygiene, especially as it presents in moderate compass a most readable account of the labours of a distinguished pioneer in the field of sanitary science.

OUR BOOK SHELF.

Induction and Deduction, and other Essays. By Constance C. W. Naden. Edited by R. Lewins, M.D. (London: Bickers and Son, 1890.)

THIS little work acquires a melancholy interest from the fact that the talented young authoress has not lived to see its publication. The title essay, on "Induction and Deduction," gained in 1887 the Heslop Memorial Medal, provided out of the proceeds of a bequest to the Mason Science College of Birmingham by the late Dr. Heslop, and awarded annually by the Council of the College. It is clear, concise, well-arranged, and carefully thought out; and leads one to believe that, had the hand of Death been withheld, Miss Naden would have made valuable contributions to philosophic thought. For Miss Naden the fundamental principle in philosophy is the famous Protagorean formula of relativity, that "Man is the measure of all things, of things that are that they are, of things that are not that they are not." She insists on the close inter-connection of induction and deduction in all reasoning, the two processes not being antagonistic but complementary. Both involve cognition and recognition; but whereas induction is a process of cognition involving recognitions, deduction is a process of recognition involving cognitions. The historical development is traced from the Greek cosmologists, through Plato, Aristotle, Bacon, Descartes and Locke, Mill, Jevons, and J. H. Green; and there are many signs that Miss Naden had not merely grasped but assimilated the teachings of those whose influence on the theory of reasoning she traced.

That Miss Naden was not wanting in humour is seen from the "Legend of the Inductive Method" in her introduction. This is so good as to be worth quoting.

"In the beginning was a set of philosophers, who, instead of looking about them, simply investigated their own thoughts, and tumbled into many ditches, not so much through star-gazing, as through mind-gazing. Out of their inner consciousness they extracted a great many principles which were inapplicable to Nature, and were therefore of none effect; and on account of this wilful perversion they failed to invent the steam-engine or to discover the circulation of the blood. This state of things went on for a long time; and in the Middle Ages matters grew worse rather than better; for now there appeared a set of men called schoolmen, who submitted everything to the authority of the Church and of Aristotle, and wasted their time in frivolous debates about phantoms named quiddities and hocceities and haeceities. Their method also was deductive, and was false. But in the glorious sixteenth century, and in our own glorious island, there arose a Lord Chancellor who wrote a book which changed the face of the intellectual world. This great man found out that the proper office of the mind is to make discoveries, and that the proper way to make discoveries is to interrogate Nature. He laid down rules for the correct framing of our interrogations. He is the father of all such as make far places near by steam-engines and electric telegraphs, or numb our pain by anæsthetics, or light the world by gas or electricity. His method is called inductive, and is true."

The other essays are on ethical and sociological questions, and on "Hylo-Idealism: the Creed of the Coming Day." They are somewhat unequal in value. The work is prefaced by a short memoir. C. LL. M.

The Lepidopterous Fauna of Lancashire and Cheshire. By John W. Ellis, M.B. (Vic.), F.E.S. (Leeds: Printed by McCorquodale and Co., 1890.)

THIS volume, the contents of which are reprinted from the *Naturalist*, will be of great service to all students of the subject to which it relates. Dr. Ellis does not offer his list as conclusive; but he has "endeavoured to present, as completely as possible, the facts known with reference to the occurence in Lancashire and Cheshire of the British species of Lepidoptera." The list is preceded by a short statement as to the geological and meteorological conditions which, by affecting the flora of the district, affect indirectly its lepidopterous fauna.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Intelligence of Chimpanzees.

ONE is glad to see that your review of Mr. Stanley's book calls attention to the following statement, which is made on the authority of Emin Pasha, and rendered in his own words:—

"The forest of Msongma is infested with a large tribe of chimpanzees. In summer-time, at night, they frequently visit the plantations of Mswa Station to steal the fruit. But what is remarkable about this is the fact that they use torches to light the way! Had I not witnessed this extraordinary spectacle personally, I should never have credited that any of the Simians understood the art of making fire."

On this passage your reviewer remarks:—"We cannot doubt

the accuracy of Mr. Stanley's report, nor the trustworthiness of Emin's observation; but we should like to have more details." But as Emin himself allows that he would never have credited the fact alleged had he not witnessed it personally, we may, perhaps, without discourtesy, refuse to accept so bald a statement of "this extraordinary spectacle." Of what were the torches constructed? How do these "Simians" practise "the art of making fire"? Having once acquired the art, do they use it only for the purpose of making "torches to light the way"?

Speaking for myself, and not without some knowledge of the intelligence of a chimpanzee, I confess that, until at least these "details" are supplied, I *do* "doubt the trustworthiness of Emin's observation," and I shall be satisfied to suppose that, owing to a shortsightedness of which we have heard so much, the Pasha has mistaken a band of native children for his "large tribe of chimpanzees." George J. Romanes.

July 6.

Dr. Kœnig's Theory of Beats.

I made an experiment, some years ago, which would seem to support and illustrate Dr. Kœnig's theory of beats, as set forth by Prof. Silvanus Thompson in his lecture before the Physical Society, and reported in your issue of June 19. Taking two tuning-forks, each of which gave the middle C (256 vibrations), I weighted one of them so as to make it give one beat a second when sounded with the other. Then, sounding this fork, so weighted, with another giving the fifth above, G (384 vibrations), I heard distinctly three beats a second. I could only account for these beats by assuming that the weighted fork C produced a feeble twelfth, and that the fork G produced a feeble octave. These two overtones would, if present, give three beats a second, $255 \times 3 = 765$, and $384 \times 2 = 768$. But I could not show by any independent evidence that these overtones are really present when the tuning-forks are sounded; and, in fact, the general opinion is against such an assumption.

If, however, Dr. Kœnig's theory be accepted, the beats are easily accounted for. According to his view, as stated by Prof. Thompson, these forks when sounded together would yield two sets of beats, called, respectively, *superior* and *inferior*; and each set of beats would blend into a musical tone. Thus we should get—

Inferior beat ... ... $384 - 255 = 129$
Superior beat ... ... $510 - 384 = 126$

These primary beats, or beat-tones as they may be called, of 129 and 126 vibrations would act as independent tones, and produce secondary beats of three in the second.

I hope Prof. Thompson's paper will be published in full, that we may all have an opportunity of considering the details of Dr. Kœnig's reasoning; but, in the meantime, I thought the experiment I have described would be interesting to your readers, as it is very easily made. Perhaps I should add that the experiment succeeds equally whether the forks are mounted on resonance-boxes or not; and therefore the effect cannot be ascribed to the boxes. Gerald Molloy.

Catholic University, Dublin, June 22.

The "Night-shining Clouds."

I have not yet seen, in any English publication, mention of the important results of the more recent researches of Herr O. Jesse and his coadjutors on these clouds. By taking simultaneous photographs from two or more widely separated places, the height of the clouds has been determined with great exactness. On July 2, 1889, this was found to be somewhat over 80 kilometres. The operations have evidently been conducted with great care, and the results may therefore be fully trusted. The question is therefore set at rest as to whether the clouds are self-luminous, for it is evident that at such a height their brightness is fully accounted for by the sun shining upon them. In 1886, Herr Jesse had, upon this supposition, ascertained their brightness to be from 49 to 54 kilometres, and that the lower the sun descended the smaller was the illumination needed to show them as the atmosphere darkened, so that the calculated height increased with the sun's depression below the horizon. Some people were incredulous about the great height at that time attributed; but the photographs give them a yet greater elevation, which places them quite out of the category of any ordinary clouds. Those who have not seen the photographs may query as to the possibility of identifying the same points in the two photographs compared, and may think that even synchronous photographs might show very different details by being taken from two distant stations; but, on the contrary, in those examples I have seen, the two photographs are so exactly alike that it is very difficult to discover any difference whatever between them, though taken at Nauen and Steglitz, 35 kilometres apart, which consideration of itself shows the enormous height of the clouds. In some of the photographs the stars α and β Aurigæ are distinctly visible.

The letter by "M. E." (p. 198) evidently describes an apparition of these clouds on the night of the 17th ult., when, as I am informed, they were also seen from Sunderland; but I have not myself seen them either this year or last, though they have been seen both years in Germany—more especially after midnight. They generally are seen in June and July, the earliest recorded date being May 26, and the latest August 11.

Sunderland, July 8. T. W. Backhouse.

In a letter which you published some time ago on "night-shining clouds," there was a request for notes of their occurrence. It may, therefore, interest some of your readers to know that they were well seen here on the evening of the 4th inst. They appeared rather suddenly shortly before 10 p.m., covering the sky from N. to N.E., and from the horizon for about 15° up. They were not in regular strata, but scattered in all directions, like cirrus after a storm. About midnight they were still visible, but more to the left, some being west of north. The first time I saw these clouds was on June 18, 1886, soon after midnight, when they were about N.W., and 20° from the horizon, and since then they have often been seen; but never, so far as I know, with the storm-tossed appearance they presented last week. Cecil Shaw.

Belfast, July 7.

A very fine display of luminous night-clouds was visible here during the night of the 4th inst., the luminosity extending to an altitude of 30° above the northern horizon, ending above in definite cirrous streamers, or cloud wisps. It will be seen by the Ben Nevis June Summary that these clouds were noted on the 29th ult. In Nature of the 3rd inst. (p. 222), the writer's communication on this subject was misprinted Kensington instead of Kingstown (Co. Dublin). The present dates from Scotland.

Aberdeen, July 7. D. J. Rowan.

An Electrical Effect.

It may be of value to remind teachers of an effect not generally known, which is produced by varying the ordinary mode of performing the experiment of putting pieces of zinc and silver in the mouth and touching them, to obtain the acid taste which accompanies the completion of the electric circuit.

If the piece of zinc be placed under the tongue, and a florin vertically between the upper lip and the top row of teeth, and the two metals be brought in contact, a faint flash is seen in both eyes when the eyes are open.

If the eyes are shut the sensation of light is not felt, so that the effect is probably due to a muscular twitching.

It is necessary to use a large silver coin, and not a shilling, and to push it well home behind the upper lip.

The experiment so made seems to be a handy and simple illustration of the meaning of subjective phenomena.

Clifton College, July 7. Edward B. Cook.

THE PHOTOGRAPHIC IMAGE.[1]

The history of a discovery which has been developed to such a remarkable degree of perfection as photography has naturally been a fruitful source of discussion among those who interest themselves in tracing the progress of science. It is only my presence in this lecture theatre, in which the first public discourse on photography was given by Thomas Wedgwood at the beginning of the century, that justifies my treading once again a path which has already been so thoroughly well beaten. If any further justification for trespassing upon the ground of the historian is needed, it will be found in the circumstance that in the autumn of last year there was held a celebration

[1] Friday Evening Lecture delivered at the Royal Institution by Prof. Raphael Meldola, F.R.S., on May 16, 1890.

of what was generally regarded as the jubilee of the discovery. This celebration was considered by many to have reference to the public disclosure of the Daguerreotype process, made through the mouth of Arago to the French Academy of Sciences on August 10, 1839. There is no doubt that the introduction of this process marked a distinct epoch in the history of the art, and gave a great impetus to its subsequent development. But, while giving full recognition to the value of the discovery of Daguerre, we must not allow the work of his predecessors and contemporaries in the same field to sink into oblivion. After the lapse of half a century we are in a better position to consider fairly the influence of the work of different investigators upon modern photographic processes.

I have not the least desire on the present occasion to raise the ghosts of dead controversies. In fact, the history of the discovery of photography is one of those subjects which can be dealt with in various ways, according to the meaning assigned to the term. There is ample scope for the display of what Mr. Herbert Spencer calls the "bias of patriotism." If the word "photography" be interpreted literally as writing or inscribing by light without any reference to the subsequent permanence of the inscription, then the person who first intentionally caused a design to be imprinted by light upon a photo-sensitive compound must be regarded as the first photographer. According to Dr. Eder, of Vienna, we must place this experiment to the credit of Johann Heinrich Schulze, the son of a German tailor, who was born in the Duchy of Madgeburg, in Prussia, in 1687, and who died in 1744, after a life of extraordinary activity as a linguist, theologian, physician, and philosopher. In the year 1727, when experimenting on the subject of phosphorescence, Schulze observed that by pouring nitric acid, in which some silver had previously been dissolved, on to chalk, the undissolved earthy residue had acquired the property of darkening on exposure to light. This effect was shown to be due to light, and not to heat. By pasting words cut out in paper on the side of the bottle containing his precipitate, Schulze obtained copies of the letters on the silvered chalk. The German philosopher certainly produced what might be called a temporary photogram. Whatever value is attached to this observation in the development of modern photography, it must be conceded that a considerable advance was made by spreading the sensitive compound over a surface instead of using it in mass. It is hardly necessary to remind you here that such an advance was made by Wedgwood and Davy in 1802.[1] The impressions produced by these last experimenters were, unfortunately, of no more permanence than those obtained by Schulze three-quarters of a century before them.

It will perhaps be safer for the historian of this art to restrict the term photograph to such impressions as are possessed of permanence: I do not, of course, mean absolute permanence, but ordinary durability in the common-sense acceptation of the term. From this point of view the first real photographs, *i.e.* permanent impressions of the camera picture, were obtained on bitumen films by Joseph Nicéphore Niepce, of Châlons-sur-Saône, who, after about twenty years' work at the subject, had perfected his discovery by 1826. Then came the days of silver salts again, when Daguerre, who commenced work in 1824, entered into a partnership with Niepce in 1829, which was brought to a termination by the death of the latter in 1833. The partnership was renewed between Daguerre and Niepce de St. Victor, nephew of the elder Niepce. The method of fixing the camera picture on a film of silver iodide on a silvered copper plate—the process justly associated with the name of Daguerre—was ripe for disclosure by 1838, and was actually made known in 1839.

The impartial historian of photography who examines critically into the evidence will find that quite indepently of the French pioneers, experiments on the use of silver salts had been going on in this country, and photographs, in the true sense, had been produced almost simultaneously with the announcement of the Daguerreotype process by two Englishmen whose names are as household words in the ranks of science. I refer to William Henry Fox Talbot and Sir John Herschel. Fox Talbot commenced experimenting with silver salts on paper in 1834, and the following year he succeeded in imprinting the camera picture on paper coated with the chloride. In January 1839 some of his "photogenic drawings"—the first "silver prints" ever obtained—were exhibited in this Institution by Michael Faraday. In the same month he communicated his first paper on a photographic process to the Royal Society, and in the following month he read a second paper before the same Society, giving the method of preparing the sensitive paper and of fixing the prints. The outcome of this work was the "Calotype" or Talbotype process, which was sufficiently perfected for portraiture by 1840, and which was fully described in a paper communicated to the Royal Society in 1841. The following year Fox Talbot received the Rumford Medal for his "discoveries and improvements in photography."[1]

Herschel's process consisted in coating a glass plate with silver chloride by subsidence. The details of the method, from Herschel's own notes, have been published by his son, Prof. Alexander Herschel.[2] By this means the old 40-foot reflecting telescope at Slough was photographed in 1839. By the kindness of Prof. Herschel, and with the sanction of the Science and Art Department, Herschel's original photographs have been sent here for your inspection. The process of coating a plate by allowing a precipitate to settle on it in a uniform film is, however, impracticable, and was not further developed by its illustrious discoverer. We must credit him, however, as being the first to use glass as a substratum. Herschel further discovered the important fact that while the chloride was very insensitive alone, its sensitiveness was greatly increased by washing it with a solution of silver nitrate. It is to Herschel, also, that we are indebted for the use of sodium thiosulphate as a fixing agent, as well as for many other discoveries in connection with photography, which are common matters of history.

Admitting the impracticability of the method of subsidence for producing a sensitive film, it is interesting to trace the subsequent development of the processes inaugurated about the year 1839. The first of photographic methods—the bitumen process of Niepce—survives at the present time, and is the basis of some of the most important of modern photo-mechanical printing processes. [Specimens illustrating photo-etching from Messrs. Waterlow and Sons exhibited.] The Daguerreotype process is now obsolete. As it left the hands of its inventor it was unsuited for portraiture, on account of the long exposure required. It is evident, moreover, that a picture on an opaque metallic plate is incapable of reproduction by printing through, so that in this respect the Talbotype possessed distinct advantages. This is one of the most important points in Fox Talbot's contributions to photography. He was the first to produce a transparent paper negative from which any number of positives could be obtained by printing through. The silver print of modern times is the lineal descendant of the Talbotype print. After forty years' use of glass as a substratum, we are going back to Fox Talbot's plan, and using thin flexible

[1] "An Account of a Method of Copying Paintings upon Glass, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. Wedgwood, Esq. With Observations by H. Davy." Journ. R.I., 1802, p. 170.

[1] For these and other details relating to Fox Talbot's work, necessarily excluded for want of time, I am indebted to his son, Mr. C. H. Talbot, of Lacock Abbey.

[2] *Photog. Journ. and Trans. Photog. Soc.*, June 15, 1872.

films—not exactly of paper, but of an allied substance, celluloid. [Specimens of Talbotypes, lent by Mr. Crookes, exhibited, with celluloid negatives by the Eastman Company.]

If I interpret this fragment of history correctly, the founders of modern photography are the three men whose labours have been briefly sketched. The jubilee of last autumn marked a culminating point in the work of Niepce and Daguerre, and of Fox Talbot. The names of these three pioneers must go down to posterity as co-equal in the annals of scientific discovery. [Portraits by Mr. H. M. Elder shown.] The lecture theatre of the Royal Institution offers such tempting opportunities to the chronicler of the history of this wonderful art that I must close this treatment of the subject by reminding myself that in selecting the present topic I had in view a statement of the case of modern photography from its scientific side only. There is hardly any invention associated with the present century which has rendered more splendid services in every department of science. The physicist and chemist, the astronomer and geographer, the physiologist, pathologist, and anthropologist will all bear witness to the value of photography. The very first scientific application of Wedgwood's process was made here by the illustrious Thomas Young, when he impressed Newton's rings on paper moistened with silver nitrate, as described in his Bakerian Lecture to the Royal Society on November 24, 1803. Prof. Dewar has just placed in my hands the identical slide with the Newton rings still visible, which he believes Young to have used in this classic experiment. [Shown.]

Our modern photographic processes depend upon chemical changes wrought by light on films of certain sensitive compounds. Bitumen under this influence becomes insoluble in hydrocarbon oils, as in the heliographic process of the elder Niepce. Gelatine mixed with potassium dichromate becomes insoluble in water on exposure to light, a property utilized in the photo-etching process introduced in 1852 by Fox Talbot, some of whose original etchings have been placed at my disposal by Mr. Crookes. [Shown.] Chromatized gelatine now plays a most important part in the autotype and many photo-mechanical processes. The salts of iron in the ferric condition undergo reduction to the ferrous state under the influence of light in contact with oxidizable organic compounds. The use of these iron salts is another of Sir John Herschel's contributions to photography (1842), the modern "blue print" and the beautiful platinotype being dependent on the photo-reducibility of these compounds. [Cyanotype print developed with ferricyanide.]

Of all the substances known to chemistry at the present time, the salts of silver are by far the most important in photography on account of the extraordinary degree of sensitiveness to which they can be raised. The photographic image with which it is my privilege to deal on this occasion is that invisible impression produced by the action of light on a film of a silver haloid. Many methods of producing such films have been in practical use since the foundation of the art in 1839. All these depend on the double decomposition between a soluble chloride, bromide, or iodide, and silver nitrate, resulting in the formation of the silver haloid in a vehicle of some kind, such as albumen (Niepce de St. Victor, 1848) or collodion on glass, as made practicable by Scott Archer in 1851. For twenty years this collodion process was in universal use; its history and details of manipulation, its development into a dry plate process by Colonel Russell in 1861, and into an emulsion process by Bolton and Sayce in 1864, are facts familiar to everyone.

The photographic film of the present time is a gelatino-haloid (generally bromide) emulsion. If a solution of silver nitrate is added to a solution of potassium bromide and the mixture well shaken, the silver bromide coagulates and rapidly subsides to the bottom of the liquid as a dense curdy precipitate. [Shown.] If instead of water we use a viscid medium, such as gelatine solution, the bromide does not settle down, but forms an emulsion, which becomes quite homogeneous on agitation. [Shown.] This operation, omitting all details of ripening, washing, &c., as well known to practical photographers, is the basis of all the recent photographic methods of obtaining negatives in the camera. The use of this invaluable vehicle, gelatine, was practically introduced by R. L. Maddox in 1871, previous experiments in the same direction having been made by Gaudin (1853-61). Such a gelatino-bromide emulsion can be spread uniformly over any substratum—glass, paper, gelatine, or celluloid—and when dry gives a highly sensitive film.

The fundamental problem which fifty years' experience with silver haloid films has left in the hands of chemists is that of the nature of the chemical change which occurs when a ray of light falls on such a silver salt. Long before the days of photography—far back in the sixteenth century—Fabricius, the alchemist, noticed that native horn silver became coloured when brought from the mine and exposed. The fact presented itself to Robert Boyle in the seventeenth century, and to Beccarius, of Turin, in the eighteenth century. The change of colour undergone by the chloride was first shown to be associated with chemical decomposition in 1777 by Scheele, who proved that chlorine was given off when this salt darkened under water. I can show you this in a form which admits of its being seen by all. [Potassium iodide and starch paper were placed in a glass cell with silver chloride, and the arrangement exposed to the electric light till the paper had become blue.] The gas which is given off under these circumstances is either the free halogen or an oxide or acid of the halogen, according to the quantity of moisture present and the intensity of the light. I have found that the bromide affects the iodide and starch paper in the same way, but silver iodide does not give off any gas which colours the test paper. All the silver haloids become coloured on exposure to light, the change being most marked in the chloride, less in the bromide, and least of all in the iodide. The latter must be associated with some halogen absorbent to render the change visible. [Strips of paper coated with the pure haloids, the lower halves brushed over with silver nitrate solution, were exposed.] The different degrees of coloration in the three cases must not be considered as a measure of the relative sensitiveness: it simply means that the products of photo-chemical change in the three haloids are inherently possessed of different depths of colour.

From the fact that halogen in some form is given off, it follows that we are concerned with photo-chemical decomposition, and not with a physical change only. All the evidence is in favour of this view. Halogen absorbents, such as silver nitrate on the lower halves of the papers in the last experiment, organic matter, such as the gelatine in an emulsion, and reducing agents generally, all accelerate the change of colour. Oxidizing and halogenizing agents, such as mecuric chloride, potassium dichromate, &c., all retard the colour change. [Silver chloride paper, painted with stripes of solutions of sodium sulphite, mercuric chloride, and potassium dichromate, was exposed.] It is impossible to account for the action of these chemical agents except on the view of chemical decomposition. The ray of light falling upon a silver haloid must be regarded as doing chemical work; the vibratory energy is partly spent in doing the work of chemical separation, and the light passes through a film of such haloid partly robbed of its power of doing similar work upon a second film. It is difficult to demonstrate this satisfactorily in the lecture-room on account of the opacity of the silver haloids, but the work of Sir John Herschel, J. W. Draper, and others has put it beyond doubt that there is a relationship of this kind between

absorption and decomposition. It is well known also that the more refrangible rays are the most active in promoting the decomposition in the case of the silver haloids. This was first proved for the chloride by Scheele, and is now known to be true for the other haloids. It would be presumption on my part in the presence of Captain Abney to enlarge upon the effects of the different spectral colours on these haloids, as this is a subject upon which he can speak with the authority of an investigator. It only remains to add that the old idea of a special "actinic" force at the more refrangible end of the spectrum has long been abandoned. It is only because the silver haloids absorb these particular rays that the blue end of the spectrum is most active in promoting their decomposition. Many other instances of photo-chemical decomposition are known in which the less refrangible rays are the most active, and it is possible to modify the silver haloids themselves so as to make them sensitive for the red end of the spectrum.

The chemical nature of the coloured products of photo-chemical decomposition is still enshrouded in mystery. Beyond the fact that they contain less halogen than the normal salt, we are not much in advance of the knowledge bequeathed to us by Scheele in the last century. The problem has been attacked by chemists again and again, but its solution presents extraordinary difficulties. These products are never formed—even under the most favourable conditions of division and with prolonged periods of exposure—in quantities beyond what the chemist would call "a mere trace." Their existence appears to be determined by the great excess of unaltered haloid with which they are combined. Were I to give free rein to the imagination, I might set up the hypothesis that the element silver is really a compound body invariably containing a minute percentage of some other element, which resembles the compound which we now call silver in all its chemical reactions, but alone is sensitive to light. I offer this suggestion for the consideration of the speculative chemist.[1] For the coloured product as a whole, *i.e.* the product of photo-decomposition with its combined unchanged haloid, Carey Lea has proposed the convenient term "photosalt." It will avoid circumlocution if we adopt this name. The photosalts have been thought at various times to contain metallic silver, allotropic silver, a sub-haloid, such as argentous chloride, &c., or an oxyhaloid. The free metal theory is disposed of by the fact that silver chloride darkens under nitric acid of sufficient strength to dissolve the metal freely. The acid certainly retards the formation of the photosalt, but does not prevent it altogether. When once formed the photo-chloride is but slowly attacked by boiling dilute nitric acid, and from the dry photosalt mercury extracts no silver. The assumption of the existence of an allotropic form of silver insoluble in nitric acid cannot be seriously maintained. The sub-haloid theory of the product may be true, but it has not yet been established with that precision which the chemist has a right to demand. We must have analyses giving not only the percentage of halogen, but also the percentage of silver, in order that it may be ascertained whether the photosalt contains anything besides metal and halogen. The same may be said of the oxyhaloid theory: it may be true, but it has not been demonstrated.

The oxyhaloid theory was first suggested by Robert Hunt[2] for the chloride; it was taken up by Sahler, and has recently been revived by Dr. W. R. Hodgkinson. It has been thought that this theory is disposed of by the fact that the chloride darkens under liquids, such as hydrocarbons, which are free from oxygen. I have been repeating some of these experiments with various liquids, using every possible precaution to exclude oxygen and moisture; dry silver chloride heated to incipient fusion has been sealed up in tubes in dry benzene, petroleum, and carbon tetrachloride and exposed since March. [Tubes shown.] In all cases the chloride has darkened. The salt darkens, morever, in a Crookesian vacuum.[1] By these experiments the oxychloride theory may be scotched, but it is not yet killed; the question now presents itself, whether the composition of the photosalt may not vary according to the medium in which it is generated. Analogy sanctions the supposition that when the haloid darkens under water or other oxygen-containing liquid, or even in contact with moist or dry air, that an oxychloride may be formed, and enter into the composition of the photosalt. The analogy is supplied by the corresponding salt of copper, viz. cuprous chloride, which darkens rapidly on exposure. [Design printed on flat cell filled with cuprous chloride by exposure to electric light.] Wöhler conjectured that the darkened product was an oxychloride, and this view receives a certain amount of indirect support from these tubes [shown], in which dry cuprous chloride has been sealed up in benzene and carbon tetrachloride since March; and although exposed in a southern window during the whole of that time, the salt is as white as when first prepared. Some cuprous chloride sealed up in water, and exposed for the same time, is now almost black. [Shown.]

When silver is precipitated by reduction in a finely divided state in the presence of the haloid, and the product treated with acids, the excess of silver is removed and coloured products are left which are somewhat analogous to the photosalts proper. These coloured haloids are also termed by Carey Lea photosalts because they present many analogies with the coloured products of photo-chemical change. Whether they are identical in composition it is not yet possible to decide, as we have no complete analyses. The first observations in this direction were published more than thirty years ago in a report by a British Association Committee,[2] in which the red and chocolate-coloured chlorides are distinctly described. Carey Lea has since contributed largely to our knowledge of these coloured haloids, and has at least made it appear highly probable that they are related to the products formed by the action of light. [Red photo-chloride and purple photobromide and iodide shown.]

The photographic image is impressed on a modern film in an inappreciable fraction of a second, whereas the photosalt requires an appreciable time for its production. The image is invisible simply because of the extremely minute quantity of haloid decomposed. In the present state of knowledge it cannot be asserted that the material composing this image is identical in composition with the photosalt, for we know the composition of neither the one nor the other. But they are analogous in so far as they

[1] I have gone so far as to test this idea experimentally in a preliminary way, the result being, as might have been anticipated, negative. Silver chloride, well darkened by long exposure, was extracted with a hot saturated solution of potassium chloride, and the dissolved portion, after precipitation by water, compared with the ordinary chloride by exposure to light. Not the slightest difference was observable either in the rate of coloration or in the colours of the products. Perhaps it may be thought worth while to repeat the experiment, using a method analogous to the "method of fractionation" of Crookes.

[2] "Researches on Light," 2nd ed., 1854, p. 80.

[1] Some dry silver chloride which Mr. Crookes has been good enough to seal up for me in a high vacuum darkens on exposure quite as rapidly as the dry salt in air. It soon regains its original colour when kept in the dark. It behaves, in fact, just as the chloride is known to behave when sealed up in chlorine, although its colour is of course much more intense after exposure than is the case with the chloride in chlorine.

[2] These results were arrived at in three ways. In one case hydrogen was passed through silver citrate suspended in hot water, and the product extracted with citric acid. "The result of treating the residue with chlorhydric acid, and then dissolving the silver by dilute nitric acid, was a rose-tinted chloride of silver." In another experiment the dry citrate was heated in a stream of hydrogen at 212° F., and the product, which was partly soluble in water, gave a brown residue, which furnished "a very pale red body on being transformed by chlorhydric and nitric acids." In another experiment silver arsenite was formed, this being treated with caustic soda, and the black precipitate then treated successively with chlorhydric and nitric acids: "Silver is dissolved, and there is left a substance . . . . [of] a rich chocolate or maroon, &c." This on analysis was found to contain 24 per cent. of chlorine, the normal chloride requiring 24·74 and the sub-chloride 14·08 per cent. The Committee which conducted these experiments consisted of Messrs. Maskelyne, Hadow Hardwick, and Llewelyn. B.A. Rep., 1859, p. 103.

are both the result of photo-chemical decomposition, and there is great probability that they are closely related, if not identical, chemically. It may turn out that there are various kinds of invisible images, according to the vehicle or halogen absorbent—in other words, according to the sensitizer with which the silver haloid is associated. The invisible image is revealed by the action of the developer, into the function of which I do not propose to enter. It will suffice to say that the final result of the developing solution is to magnify the deposit of photosalt by accumulating metallic silver thereon by accretion or reduction. Owing to the circumstance that the image is impressed with such remarkable rapidity, and that it is invisible when formed, it has been maintained, and is still held by many, that the first action of light on the film is molecular or physical, and not chemical. The arguments in favour of the chemical theory appear to me to be tolerably conclusive, and I will venture to submit a few of them.

The action of reagents upon the photographic film is quite similar to the action of the same reagents upon the silver haloids when exposed to the point of visible coloration. Reducing agents and halogen absorbents increase the sensitiveness of the film: oxidizing and halogenizing agents destroy its sensitiveness. It is difficult to see on the physical theory why it should not be possible to impress an image on a film, say of pure silver bromide, as readily as on a film of the same haloid embedded in gelatine. Everyone knows that this cannot be done. I have myself been surprised at the extreme insensitiveness of films of pure bromide prepared by exposing films of silver deposited on glass to the action of bromine vapour. On the chemical theory we know that gelatine is a splendid sensitizer—*i.e.* bromine absorbent. There is another proof which has been in our hands for nearly thirty years, but I do not think it has been viewed in this light before. It has been shown by Carey Lea, Eder, and especially by Abney—who has investigated the matter most thoroughly—that a shearing stress applied mechanically to a sensitive film leaves an impression which can be developed in just the same way as though it had been produced by the action of light. [Pressure marks on Eastman bromide paper developed by ferrous oxalate.] Now that result cannot be produced on a surface of the pure haloid: some halogen absorbent, such as gelatine, must be associated with the haloid. We are concerned here with a chemical change of that class so ably investigated by Prof. Spring, of Liége, who has shown that by mere mechanical pressure it is possible to bring about chemical reaction between mixtures of finely divided solids.[1] Then again, mild reducing agents, too feeble to reduce the silver haloids directly to the metallic state, such as alkaline hypophosphites, glucose or lactose and alkali, &c., form invisible images which can be developed in precisely the same way as the photographic image. All this looks like chemical change, and not physical modification pure and simple.

I have in this discourse stoically resisted the tempting opportunities for pictorial display which the subject affords. My aim has been to summarize the position in which we find ourselves with respect to the invisible image after fifty years' practice of the art. This image is, I venture to think, the property of the chemist, and by him must the scientific foundation of photography be laid. We may not be able to give the formula of the photosalt, but if the solution of the problem has hitherto eluded our grasp it is because of the intrinsic difficulties of the investigation. The photographic image brings us face to face—not with an ordinary, but with an extraordinary class of chemical changes due entirely to the peculiar character of the silver salts. The material composing the image is not of that definite nature with which modern chemical methods are in the habit of dealing. The stability of the photosalt is determined by some kind of combination between the sub-haloid or oxyhaloid, or whatever it may be, and the excess of unaltered haloid which enters into its composition. The formation of the coloured product presents certain analogies with the formation of a saturated solution; the product of photo-chemical decomposition is formed under the influence of light up to a certain percentage of the whole photosalt, beyond which it cannot be increased—in other words, the silver haloid is saturated by a very minute percentage of its own product of photo-decomposition. The photosalt belongs to a domain of chemistry—a no-man's land—peopled by so-called "molecular compounds," into which the pure chemist ventures but timidly. But these compounds are more and more urging their claims for consideration, and sooner or later they will have to be reckoned with, even if they lack that definiteness which the modern chemist regards as the essential criterion of chemical individuality. The investigation may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree. The chemist who discourses here upon this subject at the end of the half-century of photography into which we have now entered will no doubt know more about this aspect of chemical affinity; and if I may invoke the spirit of prophecy in concluding, I should say that a study of the photographic film with its invisible image will have contributed materially to its advancement.

[1] The connection between the two phenomena was suggested during a course of lectures delivered by me two years ago ("Chemistry of Photography," p. 191). I have since learnt that the same conclusion had been arrived at independently by Mr. C. H. Bottomley, of the Yorkshire College, Leeds.

THE VELOCITIES OF PROJECTILES.[1]

THE experimenters, whose work is recorded in the papers noted below, have succeeded admirably in their attempts to photograph projectiles while moving with their ordinary velocities. At the same time, they have obtained indications of the forms of the waves excited in the air by projectiles when moving with velocities higher than the normal velocity of sound in the air.

The first experiments were conducted by Mach and Wentzel with velocities of the projectiles about 240 m.s. (787 f.s.), which were below the normal velocity of sound, when they obtained only negative results. After this, Mach and Salcher carried on experiments of the same nature with three small arms, which respectively gave muzzle velocities of 438 m.s. (1437 f.s.), 338 m.s. (1100 f.s.), and 522 m.s. (1713 f.s.). The arrangements were such that, when the projectile was in the focus of the camera lens, it caused the discharge of a spark from a Leyden jar at a point in the axis of the lens which was more distant from the lens than the projectile. As the illumination was necessarily of very short duration, the instantaneous photographs were taken on a small scale. These photographs showed a well-defined wave of condensation of the air in front of the projectile when the velocity of the shot exceeded that of sound, or about 340 m.s. (1116 f.s.). All the experiments of value were

[1] Aus den *Sitzungsberichten d. kais. Akademie d. Wissenschaften in Wien*:—

(1) "Photographische Fixirung der durch Projectile in der Luft eingeleiteten Vorgänge," von E. Mach und P. Salcher. 1887.

(2) "Ueber die Fortpflanzungsgeschwindigkeit des durch scharfe Schüsse erregten Schalles," von E. Mach. 1888.

(3) "Ueber die in Pola und Meppen angestellten ballistischphotographischen Versuche," von E. Mach und P. Salcher. 1889.

(4) "Ueber die Schallgeschwindigkeit beim scharfen Schuss nach von dem Krupp'schen Etablissement angestellten Versuchen," von E. Mach. 1889.

(5) "Optische Untersuchung der Luftstrahlen," von E. Mach und P. Salcher. 1889.

(6) "Weitere ballistisch-photographische Versuche," von E. Mach und L. Mach. 1889.

(7) "Ueber longitudinale fortschreitende Wellen im Glase," von E. Mach und L. Mach. 1889.

(8) "Ueber die Interferenz der Schallwellen von grosser Excursion," von E. Mach und L. Mach. 1889.

made by the two small arms, which gave muzzle velocities of 1437 f.s. and 1713 f.s. When proper arrangements were made, the photographs of the projectiles fired by these two guns were always very fine and sharp. With a sufficient velocity of the shot, the limit of the condensed air-wave in front of the projectile appeared to be of a hyperboloidal form, whose vertex was in advance of the projectile, and axis in the line of flight. Similar traces in the photograph, indicating conical waves whose axes were also in the line of flight, took their rise from the base of the shot. Other but weaker traces of waves of air took their rise from points on the surface of the shot. All these straight lines in the photograph were inclined to the line of flight at a rather less angle than the traces of the head wave. When the velocity of the projectile was increased, the angles which the traces of the waves made with the line of flight were diminished.

When the highest velocities were obtained, the channel vacated by the projectile was immediately filled with peculiar little clouds, which appeared almost as regular and symmetrical as beads strung on a line stretched in the direction of the line of flight. And there was no indication of a vacuum in the rear of the shot, even when the velocity was so high as 900 m.s. (2953 f.s.). As the air was transparent, the form of the waves of air in the photographs must have been caused by the varying density of the air, which refracted the rays of light.

Long ago Robins noticed a change in the law of resistance of the air to projectiles at about the velocity of sound. Although Hutton disputed this change in the law of resistance and others ignored it, recent experiments have completely confirmed Robins's discovery. It now appears that the disturbances caused by the projectile in the air travel faster than the shot for low velocities, so that the compression of the air in front of the projectile is not sufficient to cause traces of waves in the photographs.

The two guns which gave satisfactory results with muzzle velocities of 1437 f.s. and 1713 f.s., showed widely different curvatures at the vertex of the wave of condensation in advance of the projectile. It was therefore very desirable that the velocity of the shot should have been exactly determined at the moment each photograph was taken. This condition has unfortunately not been sufficiently attended to, for although an improvised ballistic pendulum was used in some cases, it was soon discarded.

Afterwards the experimenters made use of guns of larger calibre. Salcher carried out experiments at Pola with a gun of 9 cm. (3·5 inches) calibre, which gave a muzzle velocity of 448 m.s. (1470 f.s.). Other experiments were made at Meppen, by Mach, assisted by his son, with a gun of 4 cm. (1·6 inch) calibre, which gave a muzzle velocity of 670 m.s. (2198 f.s.). The head wave appeared as a stronger and broader hyperbolic curve in the photograph, which was rather more in advance of the head of the shot than in the case where small arms were used. But when the velocities of the shot were nearly the same in the two cases, the traces of the waves in the photographs made nearly the same angle with the line of flight. This perhaps might have been expected, as it has been found experimentally that the resistance of the air to projectiles varies as the square of their diameter.

Further experiments were afterwards carried out in the laboratory. In this case projectiles composed of various metals were used, as brass, aluminium, and lead, which were of various forms. Attempts were made to determine the velocities of the projectiles in two different ways, neither of which can be regarded as quite satisfactory. In one case it was assumed that the work done on the projectile by a given charge of powder would be constant. But this assumption would not be true for considerable variations in the weight of the projectile. In the other case, the velocity was calculated by using the inclination, a, of the trace of the rear wave in the photograph to the line of flight, on the supposition that the velocity of sound = velocity of the projectile $\times \sin a$.

Much labour and ingenuity have been expended in bringing these experiments to their present satisfactory state. The ground has been well prepared for sets of systematic experiments made with useful forms of projectiles fired with various muzzle velocities. The results given by spherical projectiles might prove useful to the theorist. Other experiments might be carried out with ogival, hemispherical, and flat-headed elongated projectiles. In all cases the readings of the barometer and thermometer should be recorded, and the velocity of the projectile should be measured. The ballistic pendulum would probably give the best results if the block was shielded from the action of the wave of condensed air which accompanies the projectile.

Further, E. Mach has attempted to compare the velocity of the report of a gun with that of the projectile. In one series of his experiments, when the terminal velocity of the projectile was higher than the normal velocity of sound, the time of flight of the projectile, and the time in which the report of the gun travelled over the same distance, agreed very closely. But in another series, where the terminal velocity of the projectile was below that of sound, it was found that the time of flight of the projectile was greater than that of the report of the gun over the same distance. It was therefore considered that the report of the gun travels at the same velocity as the projectile so long as the velocity of the projectile is greater than that of sound. But when the velocity of the projectile is reduced by the resistance of the air below the velocity of sound, then the report of the gun travels in advance of the projectile, moving with the normal velocity of sound. As experiments are frequently made with velocities of the projectile more than double that of sound, there seems to be no difficulty in the way of deciding whether the report of a gun travels at the same velocity as the projectile for high velocities. If so, as appears probable, there arises the question as to the velocity with which the report of the gun travels in various directions from the muzzle of the gun. If a stretched membrane could be made to interrupt a galvanic current for a moment on the passage of a sound-wave, it would not be difficult to determine the law of propagation of the report of a gun in all horizontal directions. For the projectile might be made to cut equidistant screens, and if lines of properly prepared membranes, at the same distance apart as the screens, were run in various directions, each line being provided with its own galvanic current and marker, the progress of the projectile and of the report of the gun in the chosen directions might be registered on the surface of a cylinder rotating with a known velocity. B.

NOTES.

On Friday last, Mr. Isaac Roberts, F.R.S., of Maghull, Liverpool, was presented with an address on the occasion of his removal from Liverpool to his new observatory near Tunbridge Wells. The presentation took place in the Council Chamber at the Town Hall before a large and representative assembly. The Mayor (Mr. Thomas Hughes), who presided, referred in eulogistic terms to the services rendered to astronomy by Mr. Roberts in his chosen field of celestial photography. Principal Rendall proposed the adoption of the address, in which reference was made to Mr. Roberts's long and honourable business career in Liverpool, and to the important discoveries made by him in stellar photography. The address was signed by the Mayor, Principal Rendall and the Professorial staff of University College, many members of the City Council and of learned and scientific Societies in Liverpool, and other prominent citizens. Mr. John Hartnup, of the Bidston Observatory, seconded the motion, and

it was supported by Mr. A. G. White, the President of the Master Builders' Association. The Mayor then made the presentation. Mr. Roberts, in responding, drew attention to the fact that the city contained no monument or record of the labours of the two great Liverpool astronomers, Lassell and Jeremiah Horrocks, and expressed his willingness to join in any movement having that object in view. He also explained that his reasons for leaving Liverpool were because of the unsuitable nature of the atmosphere for taking observations.

IT is expected that the Electrical Standards Committee will arrange for a discussion, at the Leeds meeting of the British Association, on the best values to adopt for the units of electrical measurement.

MR. W. C. MACDONALD, a merchant of Montreal, has just made a munificent contribution to McGill College. He has given 150,000 dollars to the Law Faculty for the endowment of the Dean's and another chair, and also 50,000 dollars for the endowment of a Chair of Experimental Physics, and has offered to erect buildings for the Faculty of Applied Science, to include class-rooms and laboratories. Altogether, the value of Mr. Macdonald's gift is about 400,000 dollars.

AT a meeting of the Council of the South Wales University College on the 2nd inst., Mr. Archibald C. Elliott was elected to the Engineering Professorship just founded at Cardiff.

THE death took place, on the 29th ult., of Mr. Alexander Parkes, of West Dulwich, and formerly of Birmingham, at the age of seventy-six years. Mr. Parkes was well known as the inventor of the substance parkesine or celluloid, and also of many important manufacturing and metallurgical processes.

THE death, on the 2nd inst., is announced of Mr. John Page, chief engineer of the canals of the Canadian Dominion, and the projector and constructor of the enlarged St. Lawrence Canal system.

THE German Emin Pasha Relief Committee has received a telegram announcing the arrival of Dr. Peters with his Expedition in Usugara.

AT the invitation of Sir William MacGregor, Mr. C. Hedley, of the scientific staff of the Queensland Museum, has gone to New Guinea for the purpose of making a thorough scientific investigation of the invertebrate fauna of the east coast of that country.

MR. JAMES BENNETT has (according to the *Colonies and India*) been commissioned by Lord Knutsford to proceed to Lagos, to make full inquiry into and report upon the mineral and vegetable resources of the colony with a view to their further development. Mr. Bennett is the inventor of a special process for extracting, by means of chemicals, pure rubber from the milk of the wild fig-tree, of which several species are to be found in Lagos and the neighbourhood, and it seems likely that considerable advantage will accrue to the colony from his visit. Mr. Bennett will devote particular attention to such products as rubber, gums, fibres, and minerals, in which it is thought that the present trade of the colony may be largely increased, or which are considered likely to become subjects of local manufacture.

MR. BROWN, the South Australian Government Geologist, has left Adelaide for the north, having been specially commissioned to carry out the geological survey of the Macdonnell Ranges, and to report on the Hale River gold-field. He will be joined on the journey by two members of the Board already selected, and some valuable work will, it is thought, be accomplished by the party before they return.

ACCORDING to the Report of the Oxford University Extension scheme which has been issued, and which comes up to the commencement of July, "Since June 1889, 148 courses have been delivered in 109 centres by 25 lecturers. Examinations were held at the conclusion of 119 courses, and the examiners have awarded certificates of merit or distinction to 927 candidates. The courses were attended by 17,854 students, and the average period of study covered by each course was 10 weeks." In 1885–86 the number of courses delivered was 27 only, and the number of lecture centres 22. Amongst the chief signs of progress recorded are (1) a great extension of University teaching in small towns; (2) a marked increase in the number of working men attending the lectures; (3) the arrangement of a number of successful and well-attended courses during the early summer months; (4) the establishment of 36 Students' Associations at various centres; and (5) the federation in two new districts of the various lecture centres. The Students' Associations are very valuable, inasmuch as "they encourage the students to undertake regular reading throughout the year in preparation for, or in continuation of, the courses of lectures." The federation movement is also extremely helpful. It enables the difficulty sometimes experienced in procuring lecturers to be more easily surmounted, and it fosters and stimulates local interest in the study undertaken. The Committee regrets that a greater proportion of students do not present themselves for examination, but those who do go through the ordeal appear, on the whole, to come out very creditably. Scholarships are given to the writers of the best essays on a number of subjects connected with those studied during the course; and "amongst the successful essayists," we are told, "were two carpenters, two clerks, a fustian weaver, an artisan employed in a Government dockyard, and three elementary teachers." In an examination recently held, those who were awarded certificates included "a national schoolmistress, a young lawyer, a plumber, and a railway signalman." Again, we are informed that "a course of lectures on zoology recently given by an Oxford lecturer in Devonshire was attended by a student whose essays convinced the lecturer of her singular powers of accurate and original observation. She was encouraged by the lecturer to undertake a course of systematic study, and at his suggestion became a candidate in the examination for scholarships at Somerville Hall, where she was elected to the second scholarship."

AT the third summer meeting of University Extension and other students, which is to be held at Oxford in August, Mr. E. B. Poulton, F.R.S., will lecture on the influence of courtship on colour, and Mr. Francis Gotch on the physiology of the nervous system; Prof. Patrick Geddes will deal with problems of evolution, organic and social; Prof. Green, F.R.S., will give a course on geology; and Mr. C. Carus-Wilson lectures on geological phenomena. The teaching of geography, by Mr. H. J. Mackinder; protective adaptations in plants, by Mr. J. B. Farmer; and some aspects of light, by Mr. V. Perronet Sells, are also subjects announced in the programme.

DURING the cruise of the *Garland* on the west coast of Scotland in June, for the purpose of examining oyster and mussel grounds, Mr. Anderson Smith records the following captures of more especial interest to naturalists. The large *Pennatula quadrangularis* was found to be commonly distributed in great abundance in several lochs. The rare *Isocardia cor* was taken in the trawl in Loch Sunart, alive. *Balanoglossus* was obtained from deep water off Dunvegan, Skye, and may be considered the first specimen recorded from Scotland. The rare fish *Cepola rubescens*, L., or Red Band-fish, was taken off Jura, and is an addition to the fauna of the outer waters, although one or more specimens have been recorded from the Clyde area. Among *Crustacea* many interesting species were found, and the individual supply was such as to lead to the presence at some time or other of a more plentiful fish supply than was met with during the cruise.

A TELEGRAM from Quilimane announces the departure of an Expedition to Zumbo, under the command of Captain Soares d'Andrea, overseer of the River Zambesi. Satisfactory news is said to have been received of Senhor Joaquin Almeida's Expedition to Gungunghama, which landed at Chaichai, 30 miles above the mouth of the River Limpopo, on its way to Gazaland. Good news has also been received from Captain Cerales, in Bilene.

THE latest information of the Russian Expedition to Tibet, under the command of Colonel Pevtsoff, is contained in the following letter from the mining engineer Bogdanovitch, published by the Russian newspaper the *Messenger of the Volga*:—"Having happily passed through the winter at Nia, the Expedition set out on April 24 to traverse the defile of Idjelik-Khanoum, and thus reach Tibet. Colonel Pevtsoff had sent half his camels, carrying 23 bales with his collections, to the banks of the Cherchen River, where they could recover their strength with the abundant pasture. These animals are intended to facilitate our return to Russia. Our baggage will be carried into Tibet on oxen hired for the purpose. We ourselves are riding thither on horseback, carrying with us the light portion of our effects. We left Nia with 30 horses. During the winter M. Roborovsky made an excursion to Cherchen, and I made one to the mountains of Karangon-Fag, south of Khoten. During my tour I met Grombchevsky, who came with me to Khoten in February, and thence returned for a short time to Nia. The health of all the members of the Expedition is perfect, and during the winter we have received all our letters and papers from St. Petersburg, thanks to the good offices of M. Petrovsky, our Consul at Kashgar. We shall send our collections to Russia through his agency." M. Grombchevsky has informed the military Governor of the Syr-Darya district that the time of his journey has been extended until January 15, 1891. His Expedition has already traversed 5000 versts. M. Grombchevsky will pass the summer in exploring Tibet between Polon-Lhasa and Rudok.

THE July number of the *Kew Bulletin* contains further information on the cultivation and preparation of the colouring substance known as annatto. The present instalment deals with the West African seed, which does not appear to possess the qualities of that from Jamaica. A new method of preserving grain from weevils is suggested, while there is a long correspondence on Colombian india-rubber. The letters contain an account of a tree which yields rubber, and which is known in commerce as "Colombia Virgen." It has the peculiarity of growing at high elevations, and therefore in a comparatively cool climate. Another section deals with the fibre industry of the Bahamas, and particulars are given of the establishment of the botanical station at Lagos, the first of its kind on the West Coast of Africa. A letter from the Curator, Mr. McNair, gives interesting information respecting some of the plants under experimental cultivation there.

AN appendix to the *Bulletin* contains a list of new garden plants, including not only those brought into cultivation for the first time during 1889, but the most noteworthy of those which had been re-introduced after being lost from cultivation. Other plants included in the lists have been in gardens for several years, but either were not described or their names had not been authenticated until recently. All hybrids, whether introduced or of garden origin, described for the first time in 1889, are included. The list contains a reference to the place where the plant is first described or figured, or where additional information is given; besides the natural order and country, a brief notice of the habit and most striking points of each plant is given.

THE Lucayan Indians, who inhabited the islands now called the Bahamas, were the first Indians seen by Columbus. In less than twenty years this interesting people, numbering, according to the estimate of the conquerors, 40,000 persons, was wholly exterminated. The hammock was found among the Lucayans, and both the word and the thing were adopted by the Spaniards, through whom they were passed on to other nations. Various skulls have been recovered from caves in the Bahamas, and have been made the subject of a valuable paper by Mr. W. K. Brooks. This paper was read some time ago before the National Academy of Sciences, America, and has now been reprinted as a separate memoir, with carefully executed illustrations. Columbus testifies that the Lucayans were "of good size, with large eyes and broader foreheads than he had ever seen in any other race of men"; and Mr. Brooks says this agrees perfectly with the results he has reached, the most conspicuous characteristics of the skulls he has examined being the great breadth noted by Columbus, and the massiveness and solidity of the head. "We may, therefore, unhesitatingly decide," says Mr. Brooks, "that they are the remains of the people who inhabited the islands at the time of their discovery, and that these people were a well-marked type of that North American Indian race which was at that time distributed over the Bahama Islands, Hayti, and the greater part of Cuba. As these islands are only a few miles from the peninsula of Florida, this race must at some time have inhabited at least the south-eastern extremity of the continent, and it is therefore extremely interesting to note that the North American crania which exhibit the closest resemblance to those from the Bahama Islands have been obtained from Florida."

THE *Times* gives some details of the new expedition to the North Pole, for which the Norwegian National Assembly voted 200,000 kroner on the 30th ult., and which will be under the charge of M. Nansen. Hitherto, with one possible exception, all attempts to reach the North Pole have been made in defiance of the obstacles of Nature. It has been an open campaign between the endurance of man and the icy barrier of the Arctis Seas, in which Nature has always been triumphant. On this occasion a systematic and well-organized attempt will be made to ascertain if Nature herself has not supplied a means of solving the difficulty, and if there is not, after all, a possibility of reaching the North Pole by utilizing certain natural facilities in these frozen seas of which all earlier explorers were ignorant. The circumstances on which these new hopes are founded may be thus summarized. The *Jeannette* Expedition of 1879-81 and the loss of that vessel seemed to sound the knell of all expeditions to reach the Pole by Behring Straits; but in the end the results of that effort are shown to have been more satisfactory and auspicious than any of the officers of the *Jeannette* could have hoped for when, with extreme difficulty, they succeeded in reaching Siberia across the ice from their wrecked vessel. In June 1884, exactly three years after the *Jeannette* sank, there were found near Julianshaab, in Greenland, several articles which had belonged to the *Jeannette* and been abandoned at the time of its wreck by the crew, and which had been carried to the coast of Greenland, from the opposite side of the Polar Sea, on a piece of ice. This fact at once aroused curiosity as to how it accomplished the journey across the Arctic Ocean, and as to what unknown current had borne the message from Behring Straits to Greenland. However these objects reached Julianshaab, they could not have come in an eastern direction, through Smith's Sound, for the only current which reaches Julianshaab is that from the eastern coast of Greenland *viâ* Cape Farewell and the north. Nor is there much probability that they were borne in a western direction from the place where the *Jeannette* sank, for all the currents round Nova Zembla, Franz-Josef Land, and Spitzbergen are known, and it seems impossible for

the ice bearing the relics of the unfortunate *Jeannette* to have traversed the intervening distance in the space of three years, even if it were possible at all. There remains only the alternative that there is a comparatively short and direct route across the Arctic Ocean by way of the North Pole, and that Nature herself has supplied a means of communication, however uncertain, across it. Increased significance to the discovery of the *Jeannette* relics in 1884 was given by the identification in 1886 of bows found on the coast of Greenland with those by the Eskimo in the vicinity of Behring Straits, at Port Clarence, Norton Sound, and the mouth of the Yukon River. M. Nansen's Expedition will endeavour to realize these hopes of a direct route across the apex of the Arctic Ocean. A specially constructed boat of 170 tons will be built, and provisions and fuel taken for five years, although it is hoped that two will suffice. The Expedition will consist of 10 or 12 men, and M. Nansen proposes to leave Norway in February 1892.

The *Meteorologische Zeitschrift* for June contains summaries, by Dr. T. Hann, of the results of the meteorological observations at the following international Polar stations :—(1) Sodankylä, in Lapland, where observations were made for two years ending August 1884. (2) Möller Bay, in Novaia Zemlia—September 1882 to August 1883. (3) Sagastyr, at the mouth of the Lena —September 1882 to June 1884. The observer at this station remarks that they were all more susceptible of cold in summer than in winter; in autumn this susceptibility ceased. In winter they could expose themselves experimentally for a few minutes to a temperature of about $-58°$ F., with scarcely any clothing, without any unpleasant feeling. The explanation is probably to be found in the complete stillness of the air at the time.

The Harvard College has published, as vol. xxii. of its Annals, the very complete and valuable series of meteorological observations made at the summit of Pike's Peak, Colorado, between January 1874 and June 1888. This station is the highest in the world, being 14,134 feet above the sea-level. The observations, which have been prepared for publication under the superintendence of the Chief Signal Officer, contain the actual readings taken several times daily, and for a portion of the time even hourly readings, in addition to monthly means for various hours. General Greely draws attention to several nteresting facts resulting from a cursory examination of the data. The maxima of both pressure and temperature occur in July, and the minima in January; the annual march of both elements is the same, and the two curves are almost coincident. The mean temperature for the above period was 19°·3; the maximum observed was 64°, and the minimum $-39°$. The maximum daily range occurs in July and September (about 14°·3), and the minimum in December (11°·6) which is only about half of the range on the low plateau country to the eastward. The precipitation exhibits peculiarities in its distribution throughout the year; 35 per cent. of the whole amount falls in the summer, and 33 per cent. in spring, the maximum occurring in July and the minimum in February. The mean wind velocity decreases gradually from 26·6 miles per hour in January to 12·5 in July, and 12·3 in August, and it decreases from 2h.–4h. a.m. to 11h. a.m. and noon. The mean hourly velocity during any day rarely exceeds 50 miles; the highest velocity was 112 miles per hour on May 11, 1881, which General Greely states has been frequently exceeded at exposed points on the Atlantic and Pacific coasts. The prevalent direction is from south-west to north-west. Pike's Peak is frequently visited by electrical storms, but they only occur when the air is moist; many interesting details of these are given in the extracts from the observers' journals, at the end of the volume.

The Chief Signal Officer of the U.S. Army has published a valuable "Supplement to the Monthly Weather Review" for the year 1889, which contains a general discussion of the weather of that year over the United States and Canada, by Captain Dunwoody, illustrated by seven charts prepared from data from about 1000 stations. The annual mean temperature was highest in the southern parts of California, Arizona, and Florida, where it rose above 75°, and it was lowest in Manitoba, where it fell below 35°. The highest maximum temperature was 117° at Yuma, Arizona, on July 3. The highest temperature ever recorded by the Signal Service observers was 119°, at Fort McDowell, Ariz., in 1887. The lowest temperature reported by a regular station of the Signal Service was $-43°$, at St. Vincent, Minn., on February 23. The lowest minimum ever reported by a regular station of the Signal Service was $-63°$, at Poplar River, Mont., in 1885. With regard to atmospheric pressure, Captain Dunwoody remarks that the effect of marked departures from the usual distribution of monthly mean pressure was noticeable on the paths of the storms. In August and December, for instance, when the pressure over the Southern States was more than 0·1 inch above the normal, no cyclone traversed the country east of the Mississippi and south of the Ohio Rivers. With regard to rainfall, at several of the stations in the Middle Atlantic States, the annual amount was the heaviest ever reported, and the greatest deficiencies occurred in Louisiana and Washington. Fogs occurred in the vicinity of the Banks of Newfoundland most frequently from April to October; in August, fog occurred on 22 days, and in January and December on only 5 and 4 days respectively. The charts show, in addition to the mean values for 1889, the departures of that year from the normal values.

The occurrence of St. Elmo's fire at sea has been lately studied by Captain Haltermann, of Hamburg, who made examination of a number of ships' log-books for 1884 and 1885, reporting 156 cases, in 800 months of observation (*Met. Zeits.*). He finds a greater number of cases in north than in south latitudes. And of 63 cases observed in the North Atlantic (the stormiest sea in winter) 49 occurred in the months November to April, and only 14 in the other half of the year. Of the total (156) only 27 were unaccompanied by thunder and lightning, and only 6 by precipitates of some kind. Snow and hail showers, with strong wind, seemed specially favourable. Of 133 cases accompanied by rain, there were only 15 without also thunder and lightning; while of 32 with hail, 18 were without thunder and lightning; and of 14 with snow, 12 without thunder and lightning. As to wind, there were instances with all degrees of intensity. The wind was in most cases (beyond 35° lat.) from equatorial direction, and this, with the commonly observed decrease of pressure, indicates that the cases mostly occurred in the front part of depressions. In 46 cases the barometer rose, and in 8 it was unaffected. In most cases the thermometer fell. Between the equator and 10° N. lat. 12 cases were observed, and not one in the corresponding region to the south, where the trade wind generally prevails. In the region of the constantly blowing trade wind St. Elmo's fire is never met with. The western half of seas extending polewards from 30° lat. seems to afford the best conditions. On the whole, the occurrence of St. Elmo's fire may probably be ascribed to the same causes as give rise to thunder and lightning.

Mr. J. Lloyd Bozward, of Worcester, writes to us that "during a rainstorm on Tuesday (July 1), black rain fell in a district lying between the parishes of Crewle and Broughton Hackett in this county. In road-ruts where rain-water had collected, a considerable film of black sediment remained the day after the storm. The day had been remarkable for a dense canopy of shifting masses of dark-coloured clouds of the nimbus formation. Great rainstorms had been prevalent in this and the adjoining counties. The temperature had been low, and the weather rather like that of November than of July."

MR. L. W. WIGLESWORTH, writing from Brunswick, says:—"I am indebted to a friend for the following observation. A squirrel, in leaping from a height of 33 feet to the ground, caused itself, by means of curving its tail strongly to one side just before alighting, to swerve in its course and so avoid some hard substance upon which it would otherwise have fallen. It landed safely upon a more suitable spot. If no one has done so already, I should like to call attention to the use of the squirrel's tail as a steering and balancing organ during the animal's passages through the air. For other uses see NATURE, vol. xx. p. 603."

THE fresh instalment of the Panama Canal Report deals with the various plans and specifications submitted to the Committee, which are divided into four categories: (1) a canal completely isolated, and making no use of the existing rivers and streams; (2) a canal making use of the existing waterways; (3) a canal with a ship railway over part of the course; (4) a canal with a tunnel through the high land of Culebra. The Report points out the various defects or omissions in the different schemes.

MESSRS. GEORGE BELL AND SONS will publish in a few days an octavo volume entitled "The Diseases of Crops and their Remedies," by Dr. A. B. Griffiths. The work is illustrated with 51 figures, and the chemical treatment of plant diseases is fully discussed.

MR. G. CLARIDGE DRUCE, 118 High Street, Oxford, is compiling a Flora of Berkshire, which will give all available information upon the plants of that county and their distribution through it and the adjoining counties. In order to make the work as complete as possible, the compiler would greatly value any notes on plant occurrences which may be sent to him.

THE schoolmaster, it would seem, is not abroad in Spain, at least as far as geography is concerned. A leading journal of Barcelona announces that England has ceded to Germany Heligoland which is situated on the African coast. This fact suggests to it a number of ingenious political considerations. At the end of the article it is mentioned that Heligoland does not belong to anybody, and is situated between the African territories of Nyanza, Victoria, and the Congo.

SOME very remarkable observations on the production of the ripe figs of *Ficus Roxburghii*, Wall., have recently been published by Dr. D. D. Cunningham, F.R.S., of the Indian Medical Service. The species is diœcious, the male receptacles or figs containing perfect male flowers with pollen, together with imperfect or atrophied female or "gall-flowers," which never produce seed; the female figs contain perfect female flowers only. Both kinds of fig are visited by the "fig-insect," usually a species of *Eupristis*, for the purpose of laying its eggs in the ovary. This is effected in the "gall-flowers" of the male figs; but in the female figs the efforts of the insect to deposit its eggs within the ovary are frustrated by the great thickness of the wall of the ovary. It is very rare to find more than a very few grains of pollen in the female figs; and, according to Dr. Cunningham, the embryo-sac in the female flowers retains, up to the period of the visits of the insect, the character of a uninucleate cell without oosphere, synergidæ, or antipodal vesicles. The full development of the embryo in the female flowers is brought about simply by hypertrophy of the tissues, the result of the stimulation caused by the unsuccessful attempts of the insect to pierce the wall of the ovary. If these observations are confirmed, we have here one of the most remarkable instances of parthenogenesis yet recorded in the vegetable kingdom.

A NEW crystalline carbohydrate, of the composition $C_{18}H_{32}O_{16}$, named by its discoverers *stachyose*, has been extracted by Drs. von Planta and Schulze from the bulbs of *Stachys tuberifera* (*Berichte*, 1890, No. 10, p. 1692). It crystallizes from 90 per cent. alcohol in well-defined hard brilliant crystals belonging to the triclinic system, and containing three molecules of water of crystallization, $C_{18}H_{32}O_{16} + 3H_2O$. When these crystals are powdered, and heated to 103°–104°, they lose their water, leaving a colourless powder consisting of the free carbohydrate $C_{18}H_{32}O_{16}$. The crystals and their aqueous solution possess a faint sweet sugar-like taste, and the solution in water, which is of neutral reaction, rotates the plane of polarization strongly to the right. The solution does not reduce Fehling's solution until after warming with a mineral acid, when reduction rapidly ensues. On heating with nitric acid, the carbohydrate furnishes 37·3 per cent. of mucic acid. When heated with resorcinol and concentrated hydrochloric acid, a deep red coloration is produced. One of the principal products of the inversion of stachyose is galactose, as shown by the following experiment. About 30 grams of stachyose were boiled with a litre of 2½ per cent. sulphuric acid for an hour in a flask furnished with a reflux condenser. After cooling, the sulphuric acid was precipitated by barium hydrate, the barium sulphate filtered off, and the filtrate evaporated to a syrup. On extracting the syrup with 95 per cent. alcohol, and allowing the extract to evaporate over oil of vitriol, crystals slowly separated, possessing, after recrystallization, the right-handed rotation of galactose ($\alpha_D = 80°·5$). From these properties stachyose is considered to belong to the group of carbohydrates termed by Prof. Tollens crystallizable polysaccharides. In this group are included raffinose or mellitose, gentianose, and lactosine Stachyose resembles the latter substance very closely, especially as regards the formation of galactose on inversion; but it is distinguished from lactosine by its much lower dextro-rotatory power. As regards the preparation of stachyose from the *Stachys tuberifera*, the bulbs were first crushed and the juice extracted as completely as possible by water. The extract was then successively treated with lead acetate and nitrate of mercury, the lead and mercury removed by a current of sulphuretted hydrogen gas, the filtered liquid neutralized with ammonia and evaporated to a thin syrup upon a water-bath. This syrup was then poured into alcohol, when a thick precipitate was formed, which gradually collected as a dark-coloured syrup in the lower portion of the flask. After removal of the alcohol the syrup was dissolved in water, treated with phospho-tungstic acid, and filtered, excess of phospho-tungstic acid being subsequently removed by baryta-water. A stream of carbon dioxide was then led through the liquid, which was again filtered, evaporated, and poured into absolute alcohol, when a perfectly white precipitate was obtained, consisting of almost pure stachyose. The crystals are best obtained by pouring a concentrated aqueous solution of the precipitated carbohydrate into such a quantity of absolute alcohol that a 91 per cent. solution of alcohol is obtained. Crystals of stachyose immediately commence to separate.

FOR the first time since the establishment of the Gardens of the Zoological Society there is now to be seen there one of the ancient breed of the English wild cattle, Earl Ferrers having presented to the Society a fine young bull, which he captured in Chartley Park, Staffordshire. From Garner's "Natural History of Staffordshire" it appears that the wild ox formerly roamed over Needwood Forest. In the thirteenth century William de Farrarus caused the park of Chartley to be separated from the forest, and the turf of this extensive enclosure still remains almost in its primitive state. Here a herd of wild cattle has been preserved down to the present day, and they retain their wild characteristics, like those at Chillingham.

THE additions to the Zoological Society's Gardens during the past week include a Water-buck (*Cobus ellipsiprymnus* ♂), a Serval (*Felis serval*), six Vulturine Guinea Fowls (*Numida*

vulturina), three Mitred Guinea Fowls (*Numida mitrata*) from East Africa, presented by Mr. George S. Mackenzie; a Tawny Owl (*Syrnium aluco*), British, presented by Mr. G. Gurney; a Long-eared Owl (*Asio otus*), British, presented by Miss Muriel Hele; a Feathery-footed Owl (*Athene plumipes*), a Black and White Jackdaw (*Corvus daiirica*) from Newchang, South Mantchuria, presented by M. J. De La Touche; two Indian White-Eyes (*Zosterops palpebrosus*) from India, a Yellow-winged Sugar-Bird (*Cœreba cyanea* ♂) from Brazil, a Dufresne's Waxbill (*Estrelda dufresnii*) from South Africa, six Vulturine Guinea Fowls (*Numida vulturina*) from East Africa, deposited; a Plumbeous Fish-Eagle (*Polioaëtus plumbeus*) from North-west India, two Golden-headed Parrakeets (*Cyanorhamphus auriceps*) from New Zealand, a Green-winged Dove (*Chalcophaps indica* ♀) from India, purchased; two Emus (*Dromæus novæ-hollandiæ*), received in exchange; a Yak (*Poëphagus grunniens* ♂), a Viscacha (*Lagostomus trichodactylus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on July 10 = 17h. 15m. 5s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4355	—	—	17 55 41	−23 2
(2) α Herculis	3·1-3·9	Orange.	17 0 38	+14 31
(3) β Draconis	3	Yellow.	17 28 0	+52 23
(4) ζ Draconis	3	Bluish-white.	17 8 30	+65 51
(5) 205 Schj.	8	Very red.	17 38 29	−18 37
(6) R Scuti	Var.	Red.	18 41 36	− 5 50

Remarks.

(1) Unfortunately this interesting object only attains a low altitude in this country, but it is quite possible that there may be some nights on which spectroscopic observations may be made. It is the object known as the "Trifid Nebula," which is thus described in the General Catalogue:—"A very remarkable object; very bright; very large; trifid; double-star involved." For a further description observers may refer to Herschel's "Outlines." The spectrum was recorded as "continuous" by Captain Herschel in 1868, but in the same year it was observed by Prof. Winlock at Harvard College, and found to contain bright lines. This observer records: "Spectrum of the multiple star continuous, with many bright lines and some bands; one end of spectrum at λ 4280± . . . one bright line seen by C. S. Peirce at λ 4980±." I am not aware that any further observations of the spectrum have been made, but these observations should certainly be repeated with as large an aperture as possible. There can be little doubt that the line near λ 4980 is really the chief nebula line at λ 500. The appearance of bands is especially interesting, as indicating that only a relatively low temperature can be in question.

(2) The spectrum of α Herculis is probably well known to everyone who possesses a telescope and spectroscope. It is a very beautiful one of Group II., all the bands being very wide and dark, giving an appearance of alternating bright and dark bands. From the observations of Prof. Lockyer, Mr. Maunder, and myself, there can now be little doubt that we have here to deal with a mixed spectrum of bright carbon flutings and dark metallic ones. One bright band in the green is coincident with the chief carbon band, and has, moreover, the same appearance. The measures of the dark bands in the green and yellow by Vogel and Dunér show close coincidences with the flutings of manganese (λ 558 and 586) and lead (λ 546), and I have confirmed these by direct comparisons. The principal object in inserting the star in this column is to remind observers that this is a good opportunity for them to demonstrate for themselves that in stars of this type we are dealing with cometary conditions, as indicated by the carbon radiation.

(3 and 4) These stars have spectra of the solar type and of Group IV. respectively (Gothard).

(5) Dunér describes the spectrum of this star as one of Group VI., in three zones, of which the green is the brightest. He states that the spectrum is rather feebly developed, but it is not clear whether this is due to the faintness of the star, or that the bands are narrow as compared with other stars of the group. If the latter, the star may be one of the long-required connecting links between stars of this group and stars of the solar type.

(6) The spectrum of this variable does not appear to have been recorded, although its magnitude at maximum is about 5. The minimum is irregular, 6·0-8·5, and the period, according to Schmidt, is about 168 days. There will be a maximum about July 14.

A. Fowler.

Secular Inequalities in the Moon's Motion.—In the *Astronomical Journal* for June 20, Prof. J. N. Stockwell contributes the abstract of a discussion of the problem of the secular variation of the motion of the moon's perigee and node. The value found for the secular variation of the mean longitude of the moon's node does not differ very materially from that found by Laplace and subsequent investigators. But it is otherwise with the secular equation of the motion of the moon's perigee; and if the value Prof. Stockwell has obtained for the secular motion of the moon's perigee is nearly correct, the value found by Laplace and his immediate successors cannot be regarded even as a first approximation to the value of that motion.

If the mean longitude of the moon's perigee be denoted by ω, and the number of centuries from a given epoch by i, the variation $\Delta\omega$ of the mean longitude of the perigee at any number of centuries from the epoch are quoted by Airy as follows:—

	$\Delta\omega$
	″
Laplace	$-30{\cdot}55i^2$
Börg and Burckhardt	$-29{\cdot}98i^2$
Damoiseau	$-39{\cdot}70i^2$
Plana	$-40{\cdot}23i^2$
Hansen	$-39{\cdot}18i^2$
Hansen	$-36{\cdot}31i^2$.

Notwithstanding this agreement of the results of other investigators, Prof. Stockwell has found, by direct calculation, that $\Delta\omega$ is very nearly expressed by the formula

$$\Delta\omega = +\,15''{\cdot}61i^2;$$

and since the motion of the perigee is direct, it follows that this motion is *accelerated* instead of being *retarded* from age to age, as has been hitherto supposed. The application of the result to the discussion of some ancient eclipses is reserved for a future communication.

Annular Eclipse of June 17.—The current number of the *Comptes rendus* contains a letter from M. A. de la Baume Pluvinel to M. Janssen, respecting his observations at Canea Photographs of the annular and partial phases were obtained, and will be of service in determining the diameters of the sun and moon. M. Pluvinel also finds that there is no difference between photographs of the spectrum of the edge of the sun during the annular phase and the ordinary solar spectrum. It is interesting to note that during the eclipse the temperature fell from 33°·4 to 27°·4 C.

THE ETHNOLOGY OF THE GAMBIA REGION.

The Governor of the Gambia, in his last Report, devotes a long section to an account of the African tribes connected with that settlement, of which the following is a brief summary:—

Mandingoes.—The head-quarters of this extensive and powerful race lie in the mountainous district near the sources of the Niger and the Gambia, extending as far as Kong. From this region they overran the surrounding country westward to Bambouk, and still pushed on, until the banks of the Gambia, as far as the sea, more or less, fell under their sway. At the present moment the principal countries on the north bank of the river are occupied mostly by Mandingoes, and the dominant tribes in Combo, on the south bank, are also of the same race, though the heathen Jolas in the bordering Fogni country are able to hold their own against them. They practically control the trade of the lower river. Three-fourths of the ground-nuts hitherto cultivated have been grown by them; the export of bees'-wax seems to be also dependent upon the Mandingoes, who bring it down from the interior of the Jola country. They also bring cattle and hides into the market, and cultivate cotton largely, which their women spin and weave into the country cloths which play

so conspicuous a part in the trade of the river. The Mandingo language is rich and musical, and susceptible of more variety of expression than the Jolof tongue, which, next after the Mandingo, is, perhaps, the most prevalent language. The latter adopt the decuple system of numeration, whereas the former only possess a quinquennial period. The Mandingoes, as a rule, are Mohammedans, though many are "Soninkees"; and in all their faith is permeated more or less with Fetishism. The term Soninkee is applied by Mohammedans to all people, irrespective of race, who drink spirits. Physically, they are in general a spare, athletic race, of medium height, often with aquiline features, but in contour always distinct from the typical Negro. In colour, they are not so dark as the Jolofs, but their hair is woolly. The laws in Mandingo towns are administered by "Alcalis," or Sumas, both terms having the same signification. The only difference is that the former is a kind of Prime Minister in a Mohammedan town, while the latter holds a similar office in a Soninkee town. Murder and adultery are punished by death. The sentence in the former case is carried out by killing in the same manner as the murder was committed, and in the latter the adulterer is usually killed with cutlasses. The adulteress suffers only whipping, and is cast out by her husband. Theft is punished by whipping, an instrument something similar to the "cat" being used for the purpose. Slander and disrespect to parents or the aged are punished by fine, which goes to the Alcali and headman of the town. Immorality as distinguished from adultery is almost unknown; but if practised and discovered would meet with the death penalty as in adultery. The Mandingoes still keep up a connection with their original country, and recognize a supreme authority in the ancient Mandingo kingdom, though the recognition is more sentimental than real, the distance being too great for any effective authority to be exercised.

Sereres.—This race occupies the neighbourhood of Joal, Seine, and Baol, to the north of the Gambia, and outside British jurisdiction. They are a distinct people, with a language having no affinity either to the Mandingo or Jolof. They are an independent and comparatively industrious race, cultivating largely both corn and rice; they also rear numerous cattle. They seldom buy cotton goods, and have no craving for luxuries of any description. Their wardrobe never consists of more than two *pagns* or country cloths. During the dry season many Serere youths come to Bathurst to work as labourers for about three months, their ambition being satisfied when they have earned sufficient to buy a trade musket, a knife, a wooden box, and a few minor articles, such as iron bars, iron pots, raw cotton, &c. Others at times come in with small canoes, and cut firewood for the Bathurst market, and also do a little fishing. In religion the Sereres are infidels, and, except in a few instances, have hitherto resisted all attempts to convert them to Islamism. They recognize a Supreme Being, but he is only invoked in case of hostile invasion, a fashion which has doubtless been borrowed from the Mohammedans. The king of Seine, who is the ruler of the Serere nation, keeps one Marabout attached to his person for the express purpose, but his services are never put into requisition on any other occasion. Physically they are a fine, well-grown race, with not unpleasant features, their complexion as a rule being of a deep black. As with the Mandingoes, murder and adultery are punished with death; shooting or decapitation, according to the decree of the king, being the means adopted. Immorality is treated in a more lenient fashion, and resolves itself into a question of money. It is stated by persons who know the customs of both tribes well that the Mandingoes and Sereres frequently condone the offence of adultery if the male culprit is rich enough to satisfy the outraged honour of the husband, and moreover from the necessity of extreme caution that the wives resort to various cunning devices in order to deceive their husbands. The virtue of these communities is therefore more apparent than real. Each Serere man is permitted by custom to have ten wives, but indulgence in a greater number is regarded as a pardonable folly. Theft is punished in a very drastic manner. The thief has the whole of his goods confiscated and handed over to the victim of the robbery. The primitive quinquennial period in reckoning is adopted by the Sereres, as is the case with the Jolofs.

Nominkas.—This race occupies the region known as the kingdom of Nuomi or Barra. Formerly Barra was the most important of all the kingdoms of the Gambia, owing to the number and strength of the war canoes controlled by the king. The present Nominkas appear to be divided into two sections, named respectively the Nomibartokas (meaning those living at the entrance of the river) and the Nomibantokas (meaning those living more within the river). The former occupy the region between Jonwar and Jinneck, and the latter live between the towns of Essow and Jooroonko. The Nominkas are all Mandingoes, but the Nomibartokas live so near to the Sereres that they speak this language in addition to their own. The Nominkas communicate with Bathurst by means of large canoes, which some of them are very clever at making. These canoes will sometimes carry as much as three tons of ground nuts, of which they cultivate large quantities. In religion most of the Nominkas are now Mohammedans, though originally they were Soninkees. Their laws are similar to the Mandingoes, from whom they sprang.

Jolas.—The history of this primitive and extraordinary race is involved in much obscurity. No idea appears to exist among themselves in regard to their origin, and even tradition is silent except as to recent events in the chronicles of their country. Even under favourable circumstances, Jola intelligence is of such a low standard that it is not easy to acquire much reliable ethnological information from them. So far as it is possible to learn from the people themselves, the Jolas, or Fellups, have always occupied a region having for its eastern boundary Vintang Creek, following the course of that tributary, and extending as far south as the head waters of Cazamance, continuing along the north bank of that river to its mouth, and from thence extending to the limits of foreign Combo. The Banyans, Papels, Balantes, and Biafares, sometimes called Jolas, appear to be allied races. Durand, a former Governor of the Isle of St. Louis, in his voyage to Senegal, published in 1806, gives some interesting details of these people, and the extensive Portuguese establishments which then existed at various stations in Vintang Creek and the Cazamance. He remarks that both banks of the latter river "are inhabited by savage and cruel Fellups, who will not hold any communication with the whites, and are always at war with their neighbours." Those, however, who resided in the neighbourhood of the Gambia, appear to have shown different characteristics, for in writing of the town of Bintan (Vintang), the same author says:—"The negro inhabitants of this part are Felups, they speak a language peculiar to themselves, and are idolaters. . . . Those of Bintan, or its environs, who are occupied in commerce, are gentle, frank, and civilized; they like strangers, are always ready to render them service, and are candid and honest in their commercial dealings." Vintang Creek, once an important trading district, producing large quantities of wax, hides, and ivory, is now all but abandoned, and the people content themselves with the cultivation of sufficient rice and corn to supply the bare necessaries of life. They are decidedly an industrious race, and numbers of them come to Bathurst to obtain work as labourers, especially during the trade season. Vessels are laden almost entirely by Jola women, and the merchants would find it difficult to get on without them. Physically they are not an attractive-looking race, and both sexes wear little or no clothing. In their own country there is practically no government and no law; every man does as he chooses, and the most successful thief is considered the greatest man. There is no recognized punishment for murder or any other crime. Individual settlement is the only remedy, and the fittest is the survivor. Unlike the rule amongst most African races, there is absolutely no formality in regard to marriage, or what passes for marriage, amongst them. Natural selection is observed on both sides, and the pair, after having ascertained a reciprocity of sentiment, at once cohabit. No presents are made by the bridegroom, and the consent of parents is entirely ignored. They do not intermarry with any other race. There appear to be three distinct languages spoken by the Jolas, having no affinity to those of the contiguous tribes, and but little resemblance to each other. The vocabulary appears to be poor, as might be expected in the case of a people with so few wants. The Jolas do not count beyond ten, and distinct terms are used only up to five, as in all the tribes noticed, except the Mandingoes. Beyond ten the counting becomes pantomimic, the people using both hands and feet to represent higher numbers. Pieces of stick are also employed for the same purpose. The Jolas, whether from persecution, or for some other reason, have always been an isolated race, and have shunned contact with their neighbours. In spite of the proselytizing nature of the powerful Mandingoes, they have utterly failed to introduce Mohammedanism, and the Portuguese appear to have been equally unsuccessful in establishing the Roman Catholic religion.

Jolofs.—Although "Jolof" is a word very frequently used in Bathurst, and most of the inhabitants speak that language, yet, as a matter of fact, very few of the genuine race are to be found in it. The habitat of the Jolofs is in the adjoining French colony of Senegal. The Jolofs proper are stated to be a handsome race; they are proud, and exceedingly vain, claiming for themselves a very ancient descent. The women are inordinately fond of gay apparel and personal adornments of every description. They frequently pierce the ear along the entire edge with a series of holes, so that this feature may be, as far as possible, loaded with ornamentation. The wool is pulled out to its extreme length and plaited into thin strips, which hang from the head, giving a peculiar character to these natives. Of their moral character report speaks very unfavourably, mendacity, deceit, and licentiousness being prominent characteristics of this people. In religion they are fervent Mohammedans; they rarely intermarry with any other race, but are extremely sensitive to any mishap in this direction. The Jolof language is expressive, and has received considerable attention from philologists, more than one grammar having been published. Golberry, who gives a vocabulary of the Jolof language, pertinently comments upon the curious fact that in spite of the contiguity of the Jolofs to the Moors, who adopt the Arabic system of numeration, the former should have persistently adhered to the method of reckoning on one hand only, instead of on both. It is a curious and perplexing circumstance that the Mandingoes, who are an inland people, and probably came into contact with more enlightened races at a later period than the tribes nearer the coast, should be in advance of all the other races in this portion of West Africa in their system of counting. The question whether this method originated with the language, or has been acquired at a later period of their history, must be left for philologists to settle. The Mandingoes, however, have always been great traders, and it is possible that their instincts taught them at an early stage the advantages of a system based on ten fingers instead of five.

Salum Salums.—These are neighbours of the Sereres, and through intermarriage their language is a mixture of Jolof and Serere. In religion they are partly Marabouts and Soninkees. The former frequently take wives from the latter, but no Marabout would give his daughter to a Soninkee unless to a king or a prince, and that reluctantly.

Lowbeys.—This race may be described as the gypsies of North-West Africa. It is almost impossible to obtain any certain information in regard to their history. They wander about from place to place, but have no settled country. There can be no doubt that they are practically the same race as the Foulahs, though for some reason they have become detached from them. Those seen by the Governor were decidely better looking than the average Negro, resembling the Foulahs, though of a darker complexion. They confine themselves almost exclusively to the making of the various wooden utensils in use by natives generally, and the manufacture of canoes. They settle temporarily with any tribe but never intermarry with another race, thus preserving the type of feature which obviously separates them from their human surroundings. In religion most of them are pagans, though a few profess Islamism. They have no laws of their own, but are guided by those of the people with whom they are for the time being located. In case of war happening, they very sensibly remove at once to a district where there is peace. The Foulahs and Toocalores, to whom allusion is made below, are practically the same race. Little need be said of them, as the former are a well-known race, and many travellers have noted their unusual lightness of complexion. Dr. Goulsbury, in his report on the Upper Gambia Expedition, gives a concise history of this people. Their capital is Timbo in the Futa Jallon country. The Toocalores reside principally in the Futa Toro country in Senegal, but from having intermixed with other races they are darker in colour. They are a warlike people, and at times are troublesome to our neighbours the French. An appendix to the report contains a vocabulary of common words and expressions used in the Mandingo, Jolof, Serere, Jola, and Foulah languages, all of which are spoken within a comparatively small radius of the Gambia. "No one can fail to be struck with the marked differences in the word forms of the various languages, though Mr. Robert Cust, in his valuable work, 'The Modern Languages of Africa,' classes all except the Foulah in one group, which he styles the northern section of the Atlantic sub-group, and which extends from the River Senegal to Cape Mount. It is difficult, however, for any but a trained philologist to detect wherein the relationship lies, or how such radical distinctions could exist and be preserved in the languages of races living in close proximity to each other. The Jolas especially offer a very curious problem to the ethnologist; it is not probable that they were ever an interior race which has been pushed gradually by stronger neighbours to the sea, and it is somewhat extraordinary that they should have been able hitherto to withstand the power of the conquering Mandingo, and to maintain their individuality. It is true they have always been a savage and intractable people, but in point of numbers their weakness would seem to mark them out as an easy prey to the invaders. This, however, is far from being the case, and there is but little of the Jola country in the hands of strangers."

SEEDLING SUGAR-CANES.

THE Government of Barbadoes has issued a valuable Report bearing on seedling sugar-canes. It records the results obtained by Prof. J. B. Harrison and Mr. J. R. Bovell on the experimental fields at Dodds Reformatory in 1889. As the subject is one of great importance to the cane industry, the following extracts may be read with interest. We may note that a paper describing the fruit of the sugar-cane was lately read before the Linnean Society by Mr. D. Morris, and that seedling canes are growing at Kew.

"In our Report for 1888 we briefly alluded, for the purpose of insuring priority, to the fact that we had succeeded in obtaining seedlings of the sugar-cane.

"That the sugar-cane could not produce fertile seeds has been for many years regarded by botanical authorities as a proved and accepted fact, whilst very many of the older planters here believed that the canes could produce fertile seed.

"Attention here was first strongly directed to this point in 1859 by the Hon. J. W. Parris, who succeeded, at his estate, Highlands, in St. Thomas's parish, in rearing successfully self-sown seedlings. . . .

"Mr. Parris has recently stated to us that he finally succeeded in planting four and a half acres with canes raised from these original seedlings, and that he estimated their yield of sugar at over four hogsheads to the acre. He, however, from certain objectionable characteristics which arose in the canes, finally abandoned their cultivation, and did not again turn his attention to the subject. In order to test the truth of Mr. Parris's discovery of cane seedlings, several persons here attempted to raise them from the cane arrows. This was done successfully by Mr. Carter, of Bridge Cot, and by Mr. J. Wiltshire Clarke, neither of whom, however, appeared to have attached much importance to their results. At another time Mr. T. Clarke, of Cane Field, discovered cane seedlings growing from a fallen cane arrow, but did not succeed in raising them, and Mr. E. S. Sisnett found some cane seeds growing in Christ Church about the year 1861; these were allowed to grow amongst canes that were planted in the usual way, but as they were very small and thin when they reached maturity they were destroyed. In this last case the seeds appear to have come from the Bourbon canes. Next we find that the late Mr. W. Drumm paid much attention to this subject and wrote several letters to the *Sugar Cane* upon it. He, however, stated to us in March 1884 that, whilst he had repeatedly obtained cane seed, he had never succeeded in raising canes from it, and that he believed the various instances we have mentioned to be errors of observation.

"At Dodds the cultivation of the different varieties of canes in large numbers and side by side has placed us in a specially favourable condition for examining into this question. In January 1888, Mr. J. B. Pilgrim, one of the overseers at Dodds, reported to us that in the neighbourhood of one of the experimental fields he noticed that certain fine grasses were springing up, and we found at intervals from then to the middle of March similar seedlings. These were found not only on the surface of the field, but also growing in the bottom of a somewhat deep drain which had been recently dug. Much difficulty was experienced in preserving these seedlings, as they were exceedingly sensitive to the effects of exposure to the sun or wind. In June 1888 the seedlings which had survived were transplanted, giving us about 60 plants. Certain of them were dug up with great care, and placed in water until the soil crumbled away from their roots, and were carefully examined for any traces of cane

that might be on the roots. Nothing could be detected, and we were strengthened in an opinion that they were true seedling canes by the very great difference in their mode of growth from that of canes growing from the eyes of canes. A few months later we found that there were several distinct varieties amongst them. In December 1888 we examined them with great care, and grouped them into ten groups according to their most strongly marked characteristics, and found that in many of our groups thus formed the canes graduated from one group into another. Many of these canes exhibited some of the characteristics of certain of our varieties, together with the characteristics of other varieties, but in some cases we could not even form any opinion as to their parentage, as they differed completely from any canes we had ever seen. During the latter stages of their growth these canes were examined by many planters and sugar chemists, all of whom were particularly struck with the amount of variation they exhibited and with the fact that certain of them were entirely different from any canes they had previously seen. The canes, as grouped, were replanted in the usual manner, and are now in course of experimental cultivation. The remaining canes were reaped on March 8, 1889, and fifty plants yielded 307 pounds of cane tops and 1626 pounds of canes, which gave 61 per cent. of juice of a density of 10°·6 Beaumé, containing 1·629 pounds of sucrose and 0·090 pounds of glucose in the imperial gallon. The following are the compositions of the canes, cane-juice, and megass:—

	Canes.	Cane-juice.	Megass.
Water	68·11	81·18	48·20
Sucrose	12·62	15·13	8·70
Glucose	0·69	0·83	0·48
Ash	0·47	0·30	0·75
Albuminoids	0·33	0·17	0·59
Fibre	15·44	—	39·60
Organic matters	2·34	2·39	1·68
	100·00	100·00	100·00

"In order to definitely settle the question of whether the sugar-cane produced fertile seed, from the middle of December 1888 to that of February 1889 most careful search was made through the fields for growing seedlings and for arrows containing fertile seed. The search for both of these proved successful, but only on the fields in which the varieties were growing and on which, as pointed out by us in our 1888 Report, the conditions for fertilization are most favourable. The seedlings, as found, were transplanted into boxes, but, on account of the unfavourable climatic conditions, great difficulty was experienced in preserving them: on one occasion an accidental exposure to the sun for about three hours destroyed five out of seven contained in the exposed box. One seedling was found attached to a portion of cane arrow which had fallen in a damp and sheltered position. The portions of cane arrows found which apparently contained fertile seed were collected, the apparent seeds carefully separated from the spikelets of the panicles and sown at intervals, commencing on January 12. Ten days after, some of the seeds were seen to be germinating, and certain of them were removed and preserved as microscopic objects. Of the apparent seeds, less than 5 per cent. germinated, and not more than one-fourth of the germinated ones finally survived.

"As the self-sown seedlings and those raised from the seeds by ourselves reached a sufficiently advanced stage of growth (the exceedingly slow growth of the seedlings at an early time is most marked, a point which in certain previous researches may have prevented the attainment of complete proof of the fact that the sugar-cane produces fertile seed, and in which mode of growth the seedlings strikingly differ from the rapid growth of canes from the buds) were, similarly to the seedlings of 1888, transplanted into the field, and are now in course of experimental cultivation.

"As far as our experience at present shows, the conditions most favourable for the production of fertile seed by the sugar-cane are found in the cultivation of varieties side by side and in comparatively large numbers, although from observations recently made, apparently fertilized ovules are to be found from time to time upon arrows of Bourbon canes growing by themselves. To secure the germination of the seeds, it is necessary to sow them soon after the arrow ripens, under similar conditions to those necessary with the seeds of other of the Gramineæ of low germinating power.

"The fertile seeds inclosed in the glumes are long and narrow, being from 3 to 4 millimetres in length and 0·65 to 0·70 millimetres in breadth, and terminate in a beard from 6 to 8 millimetres long."

MUSICAL SCIENCE.[1]

THE object of this little pamphlet is one with which musical students are tolerably familiar. The author complains that the science of acoustics, although now well advanced, is unable to explain the actual structure of musical compositions, or to account for their effect on the mind. Many writers have made the same complaint, and have endeavoured, each according to his own fancy, to "account for" everything by some particular system of his own.

Now, it happens that some quarter of a century ago a person named Helmholtz wrote a great book with the express object of explaining this difficulty. He showed, about as conclusively as anything can be shown, that, although physical science has furnished an intelligible basis on which the musical art is founded, it goes but a very little way in explaining practical musical composition, this being guided chiefly by the æsthetic instincts and the artistic feelings of the best composers, with which physical science can have very little to do.

One would have thought that such a doctrine would be hailed with satisfaction by musicians, as exalting and ennobling the share of art in the generation of high-class music. But, strange to say, it is the musicians who chiefly dispute it, and who would wish to substitute for the heaven-born gift the dry process of scientific deduction.

Our author is of this opinion. He tells us that if by science we are to understand a thorough rational understanding of any subject, musical science has not yet been discovered; it waits still its Columbus, or its Galileo, or its Cuvier.

This may be in a certain sense true, but the science wanted for the purpose is not *physical* science. We know already pretty well all that physical science can tell us about music; but there is a science much deeper—namely, that which would investigate the general effect produced by music on the mind, as depending on its composition and style. This is the *psychology* of music, an abstruse branch of æsthetics, and it is this that must tell us, if it can, how music has attained its present power over the feelings and the emotions of mankind. It is only lately that attention has been called to this by competent writers; what is popularly said or sung about it has seldom any serious meaning.

The idea promulgated in this pamphlet is that all the mysteries of the art may be explained on the principles of *rhythm*—not, as usually understood, having to do with time and measure and accent and so on, but in a more hidden application to the generation of sounds. The system is not completely elaborated, and it is not possible to do more here than give a very general notion of it. The first six chapters treat of rhythms in general; and the author gives a drawing of a machine for illustrating them. This consists of a series of "Savart" ratchet wheels, which, having different numbers of teeth, can give rise to various rhythmic combinations of their beats. He then deduces "laws" from the consideration of these, of which the following are some specimens:—

"When we listen to a series of isochronous blows, we perceive them at once in binary rhythm, and we therefore call this perception *spontaneous*, *natural*, and *instinctive*.

"We cannot perceive a series of isochronous blows in ternary rhythm, except with the concurrence of the will; hence we call this perception *voluntary*."

These are simple fundamental laws; the following are more complicated ones:—

"Whatever is the number of teeth of a wheel, and the velocity at which it revolves, there is always the spontaneous perception of the isochronous series in binary rhythm.

"If, to a series perceived in binary rhythm, we cause another to follow, which has with it any ratio whatever represented by r, this will be at once perceived in the same ratio r, which proves that the brain is endowed with the faculty of comparison.

"In any association whatever of two series of different rhythms, there is the production of a forced perception which compels the immediate perception of the two rhythms."

[1] "Musiconomia: Leggi Fundamentali della Scienza Musicale." By Dr. Primo Crotti, Professor of the History of Music in the Royal Conservatory of Parma. (Parma, 1890.)

In chapter iv. he explains a "physiological hypothesis," that the natural impression given by binary rhythm arises probably from the naturally symmetrical structure of the human body, and the binary action of its functions, such as breathing and the beating of the heart, whereas a ternary rhythmic motion seems something heterogeneous and unnatural. In chapter v. he discusses the effects of rhythms on our organism, simple or natural rhythms giving an agreeable impression, and unnatural or complicated rhythms giving one of a contrary description. Then follows a long chapter of formulæ and complicated arithmetical statements of rhythmical combinations of various kinds.

These remarks, on rhythms generally, occupy two-thirds of the pamphlet; the remaining third is intended to show how they may be applied to the nature and effects of musical sounds. Chapter vii. contains a description of the major musical scale as harmonically deduced by the aid of the monochord; and after that we begin to get a glimpse, though obscurely, of the nature of the general argument. The following extracts may give an idea of it:—

"The only sounds of the scale which are in binary rhythm are the first, 1 : 1, and the last, 1 : 2; and these are in fact the only ones which imply rest. The fifth, 2 : 3, is constituted by a ternary rhythm, and is, in fact, the sound of greatest motion which is contained in the scale. This most powerful motive action gives to this sound the greatest tendency towards the sounds of rest, authorizing it to fall directly on them, however distant from it.

"The ratio 4 : 5, which represents the major third, is constituted by a quinary rhythm—a rhythm of semi-motion which has such an action that while it makes us feel faintly the need to pass to the fundamental, it may almost supply it coming after the fifth."

Thus we arrive at the kernel of the theory, which appears to be that the effects of different combinations of rhythmical blows or noises are assumed to be applicable to the vibrations causing musical sounds, and to account for the effects of such sounds in an emotional point of view. It is something akin to the old Euler doctrine of the "simplicity of ratios," but it professes to be more comprehensive.

It is not carried out very far in this book, but the author promises that if he lives long enough, and has sufficient means, he will complete it in a larger treatise. Then, perhaps, we shall see how it will explain the construction of "Israel in Egypt," Haydn's Quartettes, and Beethoven's Ninth Symphony.

THE MUSEUMS ASSOCIATION.

The first annual meeting of the Museums Association was held in Liverpool on June 17, 18, and 19, under the presidency of the Rev. H. H. Higgins, M.A. Liverpool was represented by the President, Mr. J. T. Moore, Mr. R. Paden, Mr. J. Chard, Mr. P. Cowell, Mr. H. A. Tobias, and a number of other gentlemen. In addition to the home contingent, the following were present:—Mr. F. W. Rudler, Mr. R. J. Howard, Mr. R. Ashton (Blackburn); Mr. J. Vicars, Mr. J. J. Ogle (Bootle); Mr. W. W. Midgley (Bolton); Mr. Butler Wood (Bradford); Mr. John Storrie (Cardiff); Mr. Montagu Browne (Leicester); Mr. C. G. Virgo (Manchester); Mr. T. J. George (Northampton); Mr. J. W. Carr (Nottingham); Mr. R. Howse (Newcastle); Prof. Boyd Dawkins, Mr. W. E. Hoyle (Owens College); Major Plant (Salford); Alderman Brittain, Mr. E. Howarth (Sheffield); Lieutenant-Colonel Turner, Mr. John Tym (Stockport); Mr. Robert Cameron, Mr. J. M. Bowley (Sunderland); Mr. L. Greening, Mr. H. Roberts, Mr. F. W. Moncks, Mr. C. Madeley (Warrington); Mr. H. M. Platnauer (York).

The proceedings were opened by Mr. J. T. Moore, as Mr. S. W. North, chairman at the last meeting (held in York), was unavoidably absent. The Rev. H. H. Higgins gave his presidential address, and the following papers were read and discussed:—"On Museum organization and arrangement," by Prof. W. Boyd Dawkins, F.R.S.; "Suggestions for aid in the determination of natural history specimens in Museums," by Mr. F. W. Rudler; "A new method of mounting Invertebrates for Museum and lecture purposes," by Dr. H. C. Sorby, F.R.S.; "Notes on the Liverpool Free Public Museum," by Mr. T. J. Moore; "Circulating school cabinets for elementary schools," by Mr. John Chard (Assistant in the Liverpool Museum); "The best means of making Museums attractive to the public," by Mr. R. Cameron; "A plea for local geological models," by Mr. T. J. Moore; "Museum cases and Museum visitors," by Mr. E. Howarth; "Notes on the Moscow Museum," by Mr. Willoughby Gardner; "Winter evening lectures in Museums," by Mr. R. Paden (Assistant in the Liverpool Museum).

Some very pleasant expeditions were made, thanks to the untiring energy of the local Secretary, Mr. H. A. Tobias, who was ably seconded by Mr. Cowell and Mr. McMillan. The members of the Association were most hospitably received; they were entertained at lunch by his worship the Mayor, and received invitations to a *soirée* of the Library, Museum, and Arts Committee, and to a magnificent *conversazione* given by the Japanese Consul, Mr. James L. Bowes.

SCIENTIFIC SERIALS.

American Journal of Science, June.—Prof. Elias Loomis: a memorial address prepared by H. A. Newton at the request of the President and Fellows of Yale College.—The magnetic field in the Jefferson Physical Laboratory, Part II., by R. W. Willson. In the February number of the *Journal* the author gave some observations of the variations of the horizontal intensity in different parts of the Jefferson Physical Laboratory in 1886–87, and upon the disturbance in the magnetic field produced by the presence of iron steam pipes and other iron masses. He now finds from extended observations that brickwork produces a great disturbance of the magnetic field, and thinks, therefore, that in general it would be safer to make exclusive use of wood for buildings and piers intended for refined magnetic measurements.—The electrical resistance of the alloys of ferro-manganese and copper (from determinations made by Mr. B. H. Blood), by Edward L. Nichols. The observations show that ferro-manganese-copper alloys decrease in electrical resistance each time they are subjected to a change of temperature. In one case an alloy containing 80·82 per cent. of copper and 19·12 per cent. of ferro-manganese, was hard drawn in the process of obtaining a strip suitable for measurement. Its specific resistance at 20°, referred to pure copper as unity, was 30·38; this resistance gradually diminished as the strip was repeatedly heated to 100° and cooled to 20°, until after seven such heatings it had fallen to 30·072. The effect of successive annealings upon the resistance of a number of alloys is also described.—Fluid volume and its relation to pressure and temperature, by C. Barus. The paper contains the introductory part of a series of experiments on the compressibility of liquids, in progress at the Physical Laboratory of the U.S. Geological Survey. Taking the results from 0° to 185° as a whole, it follows that if with the observed thermal expansion compressibility be supposed to increase inversely as the first power of the *pressure binomial* ($A + p$, where A is constant), then temperature and pressure must vary linearly to maintain constancy of volume.—On hamlinite, a new rhombohedral mineral from the herderite locality at Stoneham, Mi., by W. E. Hidden and S. L. Penfield.—On a large spring-balance electrometer for measuring (before an audience) specific inductive capacities and potentials, by Alfred M. Mayer. The chief characteristic of the excellent piece of apparatus described is that it shows *directly*, and not inferentially, that different dielectrics transmit the force of electricity in different degrees.—Notice of new Tertiary mammals, by O. C. Marsh.

The *American Meteorological Journal* for June contains:—An article on the distribution of cloud over the globe, specially prepared by M. L. Teisserenc de Bort from a former paper on this subject (Nature, vol. xxxvi. p. 15), with diagrams of mean isonephs for March, which is the clearest month over the globe, aud for July, which, on the whole, is a cloudy month, and also with figures showing the appearance of the cloud bands on the earth, compared with other planets having atmospheres.—Is the diurnal variation of the magnetic needle a meteorological phenomenon?, by Prof. R. Owen. The object of the paper is to show that our atmosphere is the medium influenced magnetically by the sun, in affecting the diurnal movement of the needle. The author thinks that the facts adduced may aid us in understanding why storms in the northern hemisphere rotate from right to left, and advance from lower to higher latitudes.—A translation of Dr. R. Assmann's paper on the climatological influence of influenza.—Report of the meeting of the New Eng-

land Meteorological Society on April 15. The chief subject of discussion was climatic changes, which were considered in two divisions: (*a*) Secular changes, introduced by Prof. W. M. Davis. He stated that secular variations have undoubtedly taken place, but we cannot give specific explanations of them. (*b*) Supposed recent changes, introduced by Prof. W. Upton. Several long series of observations were examined, and, while slight indications of periodicity were found, there was no trace of progressive change.—Trombes and tornadoes, by M. H. Faye (concluded from the May number).—Method of determining the direction of the wind by observation of the undulations at the margins of the disks of the heavenly bodies, especially the sun and moon, viewed through a telescope, by Don V. Ventosa, of the Madrid Observatory. The author states that there are always two points on the limb diametrically opposite, where the undulations travel tangentially to it and in the same direction, while in intermediate regions the waves appear more or less inclined to the limb. These motions indicate by their directions those of the wind which produces them.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 19.—"On the Changes produced in the Circulation and Respiration by Increase of the Intracranial Pressure or Tension." By Walter Spencer, M.S., Assistant Surgeon to Westminster Hospital, and Victor Horsley, B.S., F.R.S.

The authors have made for some time the effect of an increase in intracranial pressure or tension the subject of an experimental inquiry, so far as the increase affects the circulation and respiration.

They conclude that the increase in intracranial pressure influences the circulation and respiration through the diminution in the physiological activity of the medulla which it causes.

The authors first give an historical *résumé* of the work of previous observers.

The following is a summary of the chief results obtained:—

I. *The Heart.*—A considerable increase of the intracranial tension was required to influence the heart; it became slowed and finally arrested. This happened more readily after respiration had ceased, and required a higher pressure to produce it when artificial respiration was employed, whilst division of both vagi nerves abolished any slowing or arrest. The arrest, when produced, continued permanently, unless the pressure was quickly removed, or artificial respiration employed, or the vagi divided. But if the pressure was maintained whilst artificial respiration enabled the heart to start again, then the cardio-inhibitory influence was gradually lost, so that the heart returned from being very slow to its normal rate, or increased beyond the latter until the rate became equal to that seen after division of the vagi. When the vagi were divided at this stage the rate of the heart did not alter.

The Blood Pressure.—A primary rise, small in the dog, larger in the monkey, was followed by a fall distinct from that produced by the slowing of the heart, and not necessarily accompanying it. When the heart started again the blood pressure rose, finally reaching the level seen after division of the vagi, so that no further rise took place when this was done. The power of producing a fall of blood pressure was easily lost. After division of the vagi the blood pressure was raised by increasing the intracranial tension and by artificial respiration, so that it could be maintained at a level between 300 and 400 mm. Hg for considerable periods.

Respiration.—This was likewise impaired and arrested. Its arrest reacted upon the heart and the blood pressure upon it, so that after the rise of blood pressure respiration occurred, even although a much higher intracranial tension was maintained than had been sufficient to arrest it when the blood pressure was lower.

II. By the direct application of pressure in the upper part of the 4th ventricle a slowing of the heart with a rise of blood pressure was caused, whilst respiration continued, so rapid as even to be nearly three times the rate of the heart in some cases. Pressure below the calamus scriptorius arrested the respiration without directly influencing the heart, whilst in the lower part of the 4th ventricle respiration was impeded or arrested along with a fall in blood pressure, and some slowing of the heart, followed by arrest, after the respiration had ceased.

"On the Alleged Slipping at the Boundary of a Liquid in Motion." By W. C. Dampier Whetham, B.A., Coutts Trotter Student of Trinity College, Cambridge. Communicated by J. J. Thomson, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge.

The experiments of Helmholtz and Piotrowski on the oscillations of a metal sphere suspended bifilarly, and filled with various liquids, gave finite values to the slipping coefficients. The theory of the flow of liquids through capillary tubes, applied to these results, show that such an effect would produce a marked change in the time of flow of a given volume of liquid. Poiseuille showed that for a glass tube there was no slip, and it follows that the flow through a gilt tube of about a millimetre in diameter should be twenty times as fast as through a glass one.

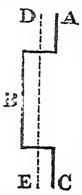
The time of flow of a given quantity of water through a glass tube was observed, and the interior of the tube was then silvered. The time was always the same for the glass and for the silver surface. The velocity of flow was varied within wide limits, and pushed near the point at which the flow ceases to be linear.

Other experiments were made on drawn copper tubes, which also agreed with Poiseuille's laws. Even when the interior surface was modified by cleaning with acids and alkalies, polishing with emery powder, coating with oil, or amalgamating with mercury, there was no change in the rate of flow. There is certainly no slip with substances which are wetted by the liquid.

Some preliminary experiments of Piotrowski on an oscillating glass flask, the interior of which was afterwards silvered, were then repeated, and it was shown that, when more precautions than Piotrowski took were used, the friction on the flask was the same, whether the surface was glass or silver.

Physical Society, June 20.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Prof. A. W. Worthington made a communication on the stretching of liquids. The three known methods by which this may be effected—viz. the barometer tube method, the centrifugal method, and the method of cooling—were described, and the precautions necessary in filling the tubes and in freeing the liquids from air discussed. With non-volatile liquids, such as sulphuric acid, the tubes are put in communication with a good pump, and before sealing, the liquid in the tube is kept at a higher temperature than that in the communicating vessel, in order that a stream of vapour may be passing outwards and carry with it any air liberated from the glass during the process of sealing. Before using tubes by the centrifugal method the author finds it advantageous to subject them to considerable "jarring" at intervals. This usually breaks the liquid column, and liberates a small bubble of air which may then be floated out. By repeating this many times, the adhesion of the liquid is greatly increased. With these precautions he had subjected water to a tension of 7·9 and sulphuric acid to one of 12 atmospheres. The cooling method of Berthelot (*Ann. de Chemie*, xxx., 1852) was then tried. In this method the liquid nearly fills a strong closed glass tube at a particular temperature. On slightly heating, it expands and fills the whole tube, any residual air being dissolved. On cooling again, the liquid remains extended, and still fills the tube until at last it lets go with a violent "click," and the bubble of residual air and vapour reappears. The tension of the liquids tested under these circumstances have usually been calculated from the relative change of volume on the assumption that the coefficient of extensibility is the same as that of compressibility. The author exhibited and described an apparatus by which the tension and the extension can be measured simultaneously. The tension is ascertained from the enlargement of the ellipsoidal bulb of a thermometer sealed into the containing vessel, and the extension calculated from the volume of the bubble after the click. The tension thermometer had been calibrated by internal pressure, and in determining the extension, correction is made for the change of volume of the apparatus. By this method he had subjected alcohol to a tension of 17 atmospheres, and found that the coefficient of extensibility is much less than that of compressibility. It is not clear what causes the liquid to let go of the glass, but it is found that the bubble can be caused to reappear by passing an electric current through a wire sealed in the capillary tube. Sir Wm. Thomson remarked that Prof. Worthington's paper was a curious commentary on the usual mathematical definition of "a liquid" as a substance which offered no resistance to being separated into parts. Speaking of freeing liquids from air, he said the beneficial effect of jarring could

easily be shown by tapping an ordinary "philosophical hammer"; separation of the column always leaves a bubble which can then be floated off. He had also found that, in freeing liquids from air by boiling, it was advantageous to have a long escape tube so that part of the liquid condenses and runs back.—Mr. C. V. Boys read a paper on the measurement of electromagnetic radiation by himself, Messrs. A. E. Briscoe and W. Watson. When Mr. Gregory described his new electric radiation meter on November 1, 1889, one of the authors said that the observed effect might be due to some cause other than expansion by heating, and that if it was a true heating effect it might be measured thermally. The present communication describes experiments undertaken to investigate the question. The first method employed was developed from the idea that if two fine wires be placed near together, and both act as resonators to a primary oscillator, the electrodynamic attraction caused by the electric currents up and down the wires, and the electrostatic repulsion between the charges on them, might result in the relative motion of the two wires. From theoretical considerations based on the assumption that the currents are harmonic in time and space, the authors inferred that the electrodynamic effect would preponderate at the middle of the wires, whilst the electrostatic repulsion would be greatest at the ends. To cause the attractions and



repulsions to conspire in producing rotation, cranked resonators, A, B, C (see figure), were made; one was fixed, and the other suspended by a quartz fibre, to turn about a middle line, DE. These were inclosed in a glass vessel, and on starting the oscillator a turning movement was observed in a direction opposite to that expected. This motion was eventually traced to the electrostatic influence of the oscillator, for although the imperfectly conducting surface of the glass acted as a perfect screen from such action when the potentials of the oscillator were varied slowly, it did not do so for changes occurring about 500 million times per second. After adopting means to avoid this disturbance, and constructing lighter resonators, the experiments were repeated, with negative results. From the dimensions of the quartz-fibre used it was estimated that a force of 158 millionths of a grain could have been detected with certainty; this would have corresponded to about $\frac{1}{300}$ of an ampere in each resonator. It is hoped that by further increasing the sensitiveness of the apparatus, and using parabolic reflectors, the effect sought for may be detected. In the second method of attacking the subject, a Joule's dynamic air-thermometer was employed. This consisted of a glass tube with a partition along the middle extending nearly to the ends. If one side of the tube be warmed, convection currents circulate, and deflect an index placed in the steam. A small mirror suspended about one edge, and counterpoised, was used for an index, and was so sensitive that it was impossible to get the air still enough by any ordinary method of screening. However, by the ingenious device of putting the thermometer within a larger tube kept rotating by clockwork, the difficulties were surmounted. A doubled wire placed in one side of the thermometer served as resonator, and on starting the oscillator a large deflection resulted. A similar deflection was caused by applying about $\frac{1}{5}$ of a volt to the ends of the wire. This proved that the effect observed by Mr. Gregory is due to heating. The least rate of heating observable with the air thermometer was found to correspond to one calorie (gramme-water-Centigrade) per 24 hours in the whole tube, or 1 calorie per centimetre of wire in 103 days. Dr. Lodge asked Sir William Thomson whether, when electric pulses travel along parallel wires with the velocity of light, any action could exist between them, for two charged spheres travelling together at that velocity exert no mutual attraction or repulsion. In reply, Sir William said he was inclined to think Mr. Boys's treatment of the subjection was in the main correct, but it was quite possible that at such velocities the ordinary laws might be modified by the fact that the time taken for the force to be propagated from wire to wire is comparable with that required for the pulse to travel the whole length of the wire. As an example of the peculiar effects of rapid discharges, he said he had seen two copper wires which had been flattened against each other by lightning. Mr. Boys thought that in his resonators a condition analogous to stationary waves would exist, for the pulses are reflected from the ends. Dr. Lodge said he had that afternoon observed the action of parallel strips when Leyden-jar discharges were passed through them. The strips gave a kick at each discharge. Mr. Gregory mentioned that, in trying to increase the sensitiveness of his meter so as to measure the variation with distance, he had found that two resonators in proximity interfered with each other. He had, however, succeeded in increasing the sensibility about five-fold. Prof. Worthington asked if it was possible to measure the energy of the oscillator, and also whether the quantity caught by the resonator could be estimated from the solid angle it subtended at the source of energy, wherever that might be. Prof. Perry considered it easier to infer the energy of the source from that received by the resonator. Dr. Lodge said the energy of the source could be easily measured. The power radiated was enormous whilst it lasted, vastly exceeding that of tropical sunshine; and, if it could be made continuous, the apparatus would soon be red-hot. The energy radiated, he said, converges on the resonators, and hence the solid angle method of estimating the amount received would be erroneous. Moreover, the source was not at the oscillator, but at a quarter wave-length from it, and most of the energy returns to the oscillator; only a small fraction is splashed off and sent into space. Small oscillators radiate powerfully because the quarter wave-lengths are small; whereas the slow oscillators or alternators used commercially radiate very little of their energy. The exact law of variation of intensity of radiation with distance was rather complicated, but the theory had been completely worked out by Stokes in 1848. Mr. Blakesley thought the energy that returns to the oscillator would be available for subsequent radiations. Dr. Lodge pointed out that wires or other resonators placed within the quarter wave-length would intercept part of the returning energy.—Two communications—notes on secondary batteries, by Dr. Gladstone and Mr. Hibbert; and an easy rule for calculating approximately the self-induction of a coil, by Prof. J. Perry—were taken as read. In the first of these the authors show cause for believing that the beneficial effect produced by adding sodium sulphate to the ordinary electrolyte is due partly to its facilitating the reduction of lead sulphate and also to its power to diminish local action between the electrolyte and different parts of the lead plates. As regards the chemical actions which take place during the working of ordinary cells, they see no reason to doubt the view put forward by one of them in 1882, that the substance produced in the voltaic reaction is ordinary lead sulphate, $PbSO_4$. They also conclude that the high E.M.F. of a cell immediately after stopping the charging current is due to the inequality of acid strength near the two plates, and the gradual fall of E.M.F. is caused by the equalization of strength produced by diffusion.—Prof. Perry's rule relates to hollow cylindrical coils, and is expressed by the following formula:—

$$\text{L (in secohms)} = \frac{n^2a^2 \div 10^7}{1{\cdot}844a + 3{\cdot}1c + 3{\cdot}5b};$$

where n = number of windings,
a = mean radius of winding in centimetres,
b = axial length,
c = radial depth of winding,

and b and c are less than $\frac{a}{2}$.

The time-constant of such a coil is given in terms of the volume of copper (V′) in cubic centimetres by

$$\frac{\text{L}}{\text{R}} = \frac{\text{V}' \div 1000}{0{\cdot}728a \div 1{\cdot}33c + 1{\cdot}5b};$$

and the conditions for making this small are pointed out in the paper.—A paper by the Rev. T. Pelham Dale was postponed till next meeting.

Anthropological Institute, June 10.—Prof. Flower, C.B., F.R.S., Vice-President, in the chair.—The Chairman exhibited a "ula" or fetish brought by the Rev. L. O. Warner from the neighbourhood of Lake Nyassa.—Mr. Theodore Bent read a paper on the nomad tribes of Asia Minor. The paper referred in the first place to the heterogeneous mass of nationalities on and around the Cilician plain, but took only one point for discussion—namely, the religion of the Ausairee around Tarsus, identifying this cult with that of the Ali-ullah-hi of Northern Persia, and proving that most nomads, from the Mediterranean to the Caspian, belonged to this secret religion. The dogmas of the religion were set forth as obtained from three sources, namely: (1) account of the renegade Suleiman; (2) studies amongst the Ali-ullah-hi; (3) researches amongst the Ausairee of Tarsus.—The Rev. E. F. Wilson read a few notes on some North American Indians.—

In a paper entitled "A Contribution to a Scientific Phrenology," Mr. Bernard Hollander presented the result of further investigations into brain-functions—the first series of which has been published in the Journal of the Anthropological Institute of August 1889—showing again a striking similarity between modern experimental researches and the observations made by the founders of the phrenological doctrine. (*a*) The centre for visual perception and ideation [first occipital convolution]—considered by some physiologists to be the centre for the "concentration of attention"—corresponds with the localization of "concentrativeness," by Geo. Combe. (*b*) Mr. Herbert Spencer, who in the *Zoist*, vols. i. and ii., published his phrenological observations, considers the area, which Dr. Gall noted to be connected with visions and hallucinations, to be the centre for the revivification of ideas, which in its unnatural actions is accompanied by a difficulty in distinguishing revived impressions from real perceptions. The localization is the same as Dr. Ferrier's centre [12], the excitation of which causes such movements of eyeballs and head as are "essential to the revivification of ideas." (*c*) Excitation of the third and fourth external convolutions in jackals and cats is accompanied by retraction of the ear, a sudden spring or bound forward, opening of the mouth with vocalization and other signs of emotional expression, such as spitting and lashing the tail as if in rage. Dr. Gall located in the same area the "carnivorous instinct," termed "destructiveness" by his followers, and considered by Prof. Bain to be merely another name for the irascible emotion. Though the investigations are by no means finished, Mr. Hollander expressed the hope that an examination of his two communications to the Institute may induce men of science to reconsider the antiquated system of phrenology, which has hitherto failed to recommend itself to the scientific world.

Geological Society, June 18.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—The Borrowdale plumbago, its mode of occurrence and probable origin, by J. Postlethwaite.—Notes on the valley-gravels about Reading, with especial reference to the Palæolithic implements found therein, by O. A. Shrubsole. The following deposits containing implements are described:—A. *North of the Thames*. (i.) Gravel at Toot's Farm, Caversham; 235 feet above sea-level. (ii.) Clayey gravel by side of Henley Road, Caversham; 168 feet above sea level. (iii.) Subangular gravel at Shiplake; 200 feet above sea-level. B. *South of the Thames*. (i.) Gravel at Elm Lodge Estate, Reading; 197 feet above sea-level. (ii.) Gravel on disturbed beds at Redlands; 157 feet above sea-level. (iii.) Comminuted flinty gravel at Southern Hill; 223 feet above sea-level. (iv.) Gravel at Sonning Hill; 185 feet above sea-level. (v.) Gravel at Ruscombe, Twyford; 165–170 feet above sea-level. The author concludes that the highest gravels (235–280 feet above sea-level) do not, so far as is known, contain any traces of man, and that a considerable amount of valley-erosion occurred before the deposition of the earliest gravels which have furnished human relics. Further, he considers that the deposits indicate the occurrence of a severe climate at an early stage, and its recurrence at a later one, viz. during the deposition of the gravels found at a height of 197 feet and 144 feet respectively above the sea-level. He believes that many of the implements found in the lower levels at Reading have been derived from gravels of various dates and different levels, which have been swept away by denudation, and that this will account for the mixed character of the types of implements. After the reading of the paper, Mr. Monckton said he had noticed great variability of the gravels around Reading, and would like to learn whether it was possible to trace the subdivisions shown in the section of the pit at Grovelands for any distance laterally. Mr. Abbott could not understand from the section displayed that the Groveland gravel belonged to the Thames system. The author maintained that the variations could, to some extent, by traced laterally. The appearance of dip towards the Kennet in the section referred to by Mr. Abbott was misleading. He did not expect contemporaneous and identical valley-gravels to be discovered on the Oxford and Berks sides of the river in the way suggested. At the point in question the levels were very different.—The next meeting of the Society will be held on Wednesday, November 12, 1890.

Royal Microscopical Society, June 18.—Mr. Frank Crisp in the chair.—Mr. Mayall mentioned, in explanation of the delay in bringing forward the report of the new objective, that, before the Committee met officially to examine the objective, it had been agreed to support the report by the production of photo-micrographs of the various objects used as tests. They were, however, disappointed to find that the visual and actinic foci were not coincident, and at the request of Prof. Abbe the objective was returned to Jena. After a lapse of several weeks, Dr. Czapski replied that he had not found any trace of a "chemical" focus non-coincident with the visual focus, and the objective was again forwarded to London. The Committee then met, and the same fractured valve of *P. angulatum* was focussed accurately and then photographed, and it appeared quite sharp in the photograph. The transit of the objective from London to Jena had somehow got rid of the "chemical" focus. Unfortunately, the slide had become seriously deteriorated, so that the critical tests which they intended to photograph could no longer be tried. They were therefore compelled to await the arrival of another slide, which Dr. Van Heurck had most kindly sent, but which the Committee had not yet been able to examine.—In the absence of Mr. Pringle, the new photo-micrographic apparatus recently made to his instructions by Messrs. Swift and Son for the Royal Veterinary College, was described by Mr. Mayall.—Mr. E. M. Nelson exhibited upon the screen two photographs of the bordered pits of pine-wood. He thought these pictures showed clearly that the pits were of the nature of clack valves, and probably served the purpose of checking the downward pressure of fluid in the vascular system. He also showed some new photographs of diatoms × 1350, including one erratic form, which he proposed calling *Craspedodiscus punchbowlii*, from its resemblance to a punch-bowl.—Mr. Mayall gave a summary of the contents of a paper, by Dr. Charles E. West, of Brooklyn, on early binocular instruments.—Mr. Dowdeswell's paper, entitled "A Contribution to the Study of Yeast: Part I., Baker's Yeast," was read. Culture-tubes, containing specimens illustrative of the subject, were handed round for inspection.—Mr. C. D. Sherborn read some portions of a paper which had been prepared by himself, conjointly with Mr. H. W. Burrows and the Rev. G. Bailey, on the Foraminifera of the Red Chalk of Norfolk, Lincolnshire, and Yorkshire.

PARIS.

Academy of Sciences, June 30.—M. Hermite in the chair.—On the partial eclipse of the sun of June 17, by M. J. Janssen (see Our Astronomical Column).—On an attempt at oyster-culture carried on in the fish-pond of the Roscoff Laboratory, by M. de Lacaze-Duthiers.—On the photographic spectrum of Sirius, by Dr. Huggins. A new group of the ultra-violet series of lines is described, extending from λ 3199 to λ 3338.—On the application to great falls, in canals, of locks with oscillating liquid columns, and on a method of utilizing the automatic oscillating tube without its being blocked when the fall is considerably increased, by M. A. de Caligny.—On the residual charge of condensers, by M. E. Bouty. The author describes some experiments made with mica condensers. Among the results obtained are: (1) That a charge absorbed between the times θ and $\theta + t$ by a condenser which does not leak is identical with the residual charge liberated between θ and $\theta + t$ by the same condenser charged during a very long time. (2) This residual or absorbed charge is proportional to the electromotive force of the charging battery.—Researches on the application of the coefficient of optical rotation to determine the nature of the compounds which are produced by the action of malic acid on neutral tungstates of soda and potash, by M. D. Gernez. The experiments show: (1) That, both with salts of soda and potash, a regular increase of negative rotation occurs with solutions of increasing strength until a maximum of $-7° 7'$ is reached, when equal equivalents of the two bodies are used. (2) A diminution of the rotation with change of sign and a positive maximum of $+2° 42'$ for one equivalent of acid to two equivalents of the salt. (3) A diminution of the rotation with change of sign and a negative maximum of $-2°\cdot1$ when the solution contains one equivalent of acid to three equivalents of the salt.—On the action of titanium chloride on metals, by M. Lucien Lévy.—On the decomposition of rocks and the formation of arable land, by M. A. Muntz.—The author has found nitrifying micro-organisms universally distributed, even occurring on the bare rocks of mountain peaks, and attributes to them a considerable share in the work of breaking down rock-masses into soil.—On the development of the blastoderm in the isopodous Crustacea (*Porcellio scaber*, Latr.), by M. Louis Roule.—Crystallographic and optical properties of pyroxene obtained by means of superheated water, by M. A.

Lacroix. The conclusion is drawn that all its properties are sufficiently characteristic to identify artificial pyroxene with that of volcanic rocks.—The identity of composition of some sedimentary phosphates with apatite, by M. Henri Lasne. Phosphates from various sources and of different geological ages have been found to consist essentially of calcium fluophosphate of the same percentage composition as apatite, together with varying amounts of clay, calcium sulphate, &c.—On the reproduction of sillimanite and the mineralogical composition of porcelain, by M. W. Vernadsky. Kyanite and andalusite are transformed into sillimanite when raised to a white heat; the same mineral, or some body very like it, is shown by the author to be produced on heating together an intimate mixture of dry SiO_2 and dry Al_2O_3. He further proves that the products of decomposition by heat of topaz, dumortierite, and kaolin are composed in great part of the same substance, and that the crystalline portion of porcelain consists also of this mineral.—On the fauna of pyritic Ammonites of Djebel-Ouach, province of Constantine, by M. G. Sayn.—Craniectomy on a microcephalous subject, by M. Lannelongue. A remarkable operation on a female, aged four years, is described, resulting in a considerable amelioration of the condition of the patient.—On a new system of representing geographical relief, by M. Eugène Guillemin.

Berlin.

Physical Society, June 13.—Prof. Du Bois-Reymond, President, in the chair.—At the opening of the meeting, Prof. Schwalbe referred in the warmest terms to the loss the Society had sustained by the death of Director F. Gallenkamp, who had for many years acted as its Librarian.—Prof. Vogel spoke on photography in natural colours as attempted at first by Seebeck, then in succession by Becquerel, Niepce, St. Victor, Poitevin, Zenker, and most recently by a Hungarian named Verres. He exhibited a series of photographs in colours obtained by Verres, which, however, showed conclusively that he has not solved the problem, since, although the reds appear as red in the photographs, so also do the yellows and greens appear as red, and the blues as an undeterminate colour. These photographs, on the other hand, mark a distinct advance in colour-photography, since they are fixed, while those of Zenker, although more strikingly coloured, were not fixed. The speaker criticized Zenker's views on the mode of formation of a coloured photograph, and expressed his disbelief in the possibility of any *one* substance being so changed by rays of different wave-length as to emit, from various parts of itself, rays of exactly corresponding wave-length.—Prof. Kundt exhibited a spiral of bismuth, as employed by Dr. Lenand to demonstrate the influence of a magnetic field upon the electrical conductivity of this metal; he further showed by experiment how considerable this influence is, and pointed out that it provides a means of measuring the intensity of the field.—Prof. Lampe explained that some years ago he had announced to the Society that a problem on maximal attraction of a point dealt with by Gauss had been previously propounded and treated by Playfair. More recently he had found that even Playfair was not the first to deal with this problem, but that a partial solution had been obtained by De Saint Jacques in 1750.

Physiological Society, June 20.—Prof. Du Bois-Reymond, President, in the chair.—Dr. I. Munk gave a *résumé* of the present state of knowledge as to the absorption of fat. The fact that fats with a high melting-point, such as stearin, are not absorbed is usually adduced in support of the supposed importance of emulsification; on the other hand, some of the speaker's own experiments had shown that a small amount (5–7 per cent.) of this fat may be absorbed. In support of the saponification of fats he described some recent experiments made on the patient with a lymphatic fistula (Nature, vol. xli. p. 504) and on dogs. Thus, for instance, when spermaceti was administered to the patient after prolonged fasting, the lymph became cloudy and milky in the third or fourth hour of digestion. Analysis of the whole lymph secreted during thirteen hours showed that 15 per cent. of the spermaceti had passed into the lymph, not, however, in an unchanged condition, but as palmitin, showing that the spermaceti must have been decomposed in the alimentary canal, and that the palmitic acid of which it is partly composed must have become united with glycerin. He made further experiments with oleate of amyl-alcohol, hoping to verify the decomposition of this fat by observing that the animal exhibited symptoms of poisoning with amyl-alcohol: this was, in fact, observed. The above compound could not, owing to its pungent taste, be given in sufficiently large doses to the patient with the lymphatic fistula to be conclusive; but an analysis of the lymph secreted from the fourth to the twelfth hours showed that it contained, not the compound of oleic acid and amyl-alcohol, but olein—a further proof of its decomposition before absorption. So many difficulties stand in the way of the view that all fats are saponified before absorption, that the speaker considered the various points in connection with the process of fat absorption as still undetermined.—Prof. Ewald gave an account of the sudden death of a patient following upon the introduction of a flexible gastric sound; a subsequent *post-mortem* showed that the cause of death was rupture of an aortic aneurism. He then proposed as a subject for discussion the question as to whether the rise of blood-pressure which led to the rupture was due to the slight abdominal pressure or to some psychic excitation. The majority of those who joined in the discussion regarded the former as the causative factor of the rise of aortic blood-pressure.

Brussels.

Royal Academy of Sciences, May 6.—M. Stas in the chair.—The following communications were presented:—On the conditions of the act of chemical combination; modifications arising from the presence of inactive dissolvents; extract of a letter from M. Menschutkin, Professor of Chemistry at St. Petersburg, to M. Louis Henry. Prof. Menschutkin has studied the combination of $(C_2H_5)_3N$ with C_2H_5I in the presence of inactive dissolvents, for example, hydrocarbons, simple ethers, ketones, &c. The experiments show that such substances exercise a considerable influence on the velocity of combination, it being found that if 1 represents the constant of velocity of the reaction noted above in hexane, C_6H_{14}, this constant for the same combination in $CH_3—CO—C_6H_5$, all other things being equal, is 847·7.—The state of vegetation on March 21 and April 21, 1890, in Gembloux, Huccorgne, Liége, and Spa, by Prof. G. Dewalque. The observations that have been obtained of herbaceous plants are very discordant. It is estimated, however, that vegetation was from 6 to 8 days behind on March 21, and 4 or 5 days behind on April 21.—On the characteristic points of some remarkable lines in conics, by C. Servais.—On the curvature in curves of the second degree, by the same author.—Note on the development in series of sine, cosine, and exponential functions, by Prof. Alphonse Demoulin.

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THURSDAY, JULY 17, 1890.

THE INDIAN CIVIL SERVICE AND THE INDIAN FOREST SERVICE COMPETITIONS.

THOSE who devote attention to educational questions are looking with interest for the publication of the new schedule for the Indian Civil Service competitions. But past experience of the Civil Service Commissioners, who are largely responsible for these matters, and on whom the various departments must chiefly rely for the carrying out of their ideas, causes the interest of many of us to be not unmixed with a considerable degree of anxiety lest there should be in this case a repetition of the recent Woolwich and Sandhurst *fiascos*. Therefore, notwithstanding the favourable character of Sir John Gorst's recent reply to Sir Henry Roscoe, we hope that those at the Universities who are interested in the question, and the leaders in science, will not yet rest upon their oars, but that they will bring under the direct notice of the authorities at the India Office the present position of science studies at the Universities and the views that are held there on this important subject, in order that the latter, who we believe hold fair views upon the subject, may be in a position to judge of the fitness of any scheme that may be submitted to them and of its correspondence or the reverse with the present condition of higher education. We bring this subject again under the notice of our readers, partly because of its importance, and partly because in the new regulations for the India Forest Service we have recently been afforded a fresh example of the inability of those who are officially intrusted with these matters to properly estimate the requirements of the public services. These new regulations are, no doubt, better than those which they are intended to replace in several respects, notably so in that the absurd list of fourteen compulsory subjects by which this examination has hitherto been distinguished has now been abolished, and also in that the examinations will now run somewhat closely on the lines of the army competition—a change which will probably secure for them a wider field of candidates than they have hitherto had. But, considered as a method of selecting those who are most likely to do good work in a scientific profession, the scheme must be pronounced to be a failure, since it will neither insure the selection of the most promising men for the particular service required of them, nor, as many will think, encourage those who intend to compete to give themselves a really liberal education.

The subjects and their mark values are as set out below:

Class I.—Obligatory Subjects.

1.	Mathematics, Elementary	2500	Not less than one-third of full marks in each of these subjects must be obtained to qualify.
2.	English Composition	1000	
3.	German	2000	

Class II.—Optional Subjects.

4.	Mathematics, Higher	2000	Any two, but not more than two, of these subjects may be selected.
5.	French	2000	
6.	Latin	2000	
7.	Greek	2000	
8.	English History	2000	
9.	Botany	2000	
10.	Chemistry	2000	
11.	Physics	2000	
12.	Physical Geography and Geology	2000	

Class III.—Additional Subjects.

13.	Freehand Drawing	500	Either or both of these may be taken up in addition to those in Classes I. and II.
14.	Geometrical Drawing	300	

A close inspection of the scheme reveals at once certain serious objections to it. In the first place, whilst admitting that the authorities have done well to provide fairer opportunities for students whose education has been largely in literature, we must claim, both in the interests of the service and of the candidates, that those who seek admission as probationers for so essentially scientific a service ought in every case to be required to show some moderate degree of capacity for scientific work before they are admitted to their professional studies. The Professors at Cooper's Hill are men of the highest standing, and they will undoubtedly give an excellent training to all who are fitted to undergo it. But they cannot create scientific aptitude in those who are intrusted to them. Hence, if men who are deficient in the proper qualities are selected as probationers, either the service or the probationers must suffer; for it must happen, either that the scientific standard of some of those who are finally selected will be unduly low, or else that some probationers who ought never to have been selected will be finally rejected after much loss of time and much expenditure in money. To show how real this objection to the scheme is, it is only necessary to point out that under the new scheme a candidate may offer himself for examination in the following subjects with every reason to hope for success:—Elementary mathematics, English composition, German, Latin, Greek, drawing. We do not think that the staunchest upholders of the study of literature will support this selection of subjects as one by means of which a satisfactory judgment of the fitness of the candidates for a scientific profession can be made. It is plain that a young man who shows ability in these subjects may or may not have a reasonable degree of scientific aptitude also.

On the other hand, many will think that the new scheme permits even too great a neglect of literary studies on the part of those candidates whose bent is for science, since several combinations such as the following would also be possible:—Elementary mathematics, English composition, German, chemistry, physics. We are sure that many advocates of science teaching will feel that in this group of subjects literature is too much neglected; and we believe that a youth of nineteen or thereabouts might add to it another modern language, or some knowledge of Latin, with advantage to his studies in mathematics and science, as well as to his general education.

In connection with this question, too, it must be remembered that candidates are practically compelled by the severity of these competitions to stick to those subjects in which they are most likely to compete successfully, for a long period, often for several years, beforehand. So that, for example, a young man who is only moderately good at science and rather better at languages will be most likely to win a place in this scientific service by neglecting all scientific reading up to the age of nineteen or twenty years! Surely this is an example of how not to do it!

It seems to us, therefore, that the proposed scheme for the India Forests Department imperatively requires

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such amendment as shall secure evidence of a reasonable degree of capacity for science on the part of every probationer, whilst it would be well also if it could be made to encourage a rather wider range of literary study in the earlier education of those whose main interests lie in the direction of science. What is desirable could be attained in several ways. But it could, perhaps, be best effected by permitting candidates to offer themselves for examination in three subjects instead of two from Class II.; with the limiting condition that one at least of these three must be taken from numbers 5, 6, 7, 8, and one of them from 9, 10, 11, 12.

We trust that this subject may also be brought under the notice of the authorities at the India Office. It seems evident from the changes already made that they are in no way prejudiced against either scientific or literary studies, and we feel sure that if they will institute inquiries they will find that similar opinions to those we have expressed are widely held on this subject.

THE VOLCANOES OF HAWAII.

Characteristics of Volcanoes, with Contributions of Facts and Principles from the Hawaiian Islands: including a Historical Review of Hawaiian Volcanic Action for the past Sixty-seven Years, a Discussion of the Relations of Volcanic Islands to Deep-sea Topography, and a Chapter on Volcanic-Island Denudation. By James D. Dana. Illustrated by Maps of the Islands; a Bathymetric Map of the Atlantic and Pacific Oceans; and Views of Cones, Craters, a Lava-Cascade, a Lava-Fountain, &c. (London: Sampson Low, Marston, Searle, and Rivington, 1890.)

THE veteran geologist of the United States has rendered an inestimable service to science by the publication of this splendid monograph, which has just made its appearance simultaneously in this country and in the United States. To find any work on a similar subject comparable with it either in importance, or in the influence it is likely to exert upon geological thought, we must go back to the publication of Fouqué's "Santorin," of Von Waltershausen's "Etna," or Scrope's "Volcanoes of Central France."

The Hawaiian volcanoes are unquestionably the grandest on the face of the globe. Their vast dome-shaped masses, with slopes averaging from 6° to 8°, rise to heights of only 14,000 feet above the sea-level; but deep-sea soundings have shown that they stand on a floor 12,000 to 18,000 feet below that level, so that, as Prof. Dana points out, the higher volcanic mountains of the Sandwich Islands must have an elevation of not far from 31,000 feet above their bases! Beside these lofty and bulky domes, the graceful volcanic cones of the North and South American continents, of Japan and Java, sink into insignificance. The Hawaiian Archipelago contains no less than fifteen volcanoes of the first class, all but three of which appear to be now extinct. The active volcanoes of Hawaii give rise to lava-floods, which, in their bulk and in the distances they flow from their point of emission, are only surpassed by those of Iceland. In their remarkably non-explosive action, in the characters of their great pit-craters, in the wonderful liquidity of their lavas—giving rise to veritable fountains of molten rock—and in the beauty and singularity of some of their igneous products, the Hawaiian volcanoes are without a parallel anywhere else in the world.

The Hawaiian volcanoes appear to form two nearly parallel bands, which doubtless indicate great lines of fissure in the earth's crust, the extreme length of these being about 400 miles. The recent topographical surveys of the islands made by Prof. W. D. Alexander, Surveyor-General to the Hawaiian Government, and a number of recent soundings in the adjoining seas, enable us to realize, in a way that was not previously possible, the dimensions and forms of these vast volcanic piles.

Prof. J. D. Dana has enjoyed exceptional facilities for studying these unique centres of igneous activity. As naturalist of the U.S. Exploring Expedition, he visited the islands in November 1840, after the great eruption of Kilauea that had taken place in May of the same year. The work of the actual survey and description of the craters was unfortunately not committed to Prof. Dana; for the energetic, though scientifically untrained, head of the Expedition, Captain Wilkes, determined to undertake this task himself; and the naturalist was sent away to another station while the survey was in progress. Had Prof. Dana been present to advise and assist the surveying officers, it is clear that many unfortunate errors would have been avoided, and that the accounts of the volcanoes contained in the "Narrative of the United States Exploring Expedition" would have had far greater scientific value.

After his return to the States and his settlement at Yale College, Prof. Dana showed his continued interest in the Hawaiian volcanoes, by keeping up a constant correspondence with missionaries and other residents in the islands; and every great eruption was carefully chronicled in the pages of the *American Journal of Science*, which he has so long edited. The memoirs of Brigham and Captain Dutton, and the enlargement and correction of our topographical knowledge of the islands, resulting from the Government survey, seem once more to have aroused the author to a sense of the importance of the subject, and in 1887 he commenced a series of papers on the history of the changes in the Mount Loa craters. He had not proceeded far with this work, however, before he felt the need of a second personal examination of the district. With characteristic energy, he undertook, in spite of his advancing years, a ten-weeks' journey, involving over ten thousand miles of travel, in which he visited all the chief points of interest; and the book before us is the outcome and monument of his labours.

The work of criticizing and reconciling the accounts given by numerous travellers, beginning with notices written as long ago as the year 1823, has been admirably performed by Prof. Dana. Without his personal knowledge of the localities, and the aid afforded by the new and accurate maps of the islands, the task would, indeed, have been a hopeless one; for many of the descriptions were penned by unscientific and careless writers, and inaccuracies and exaggerations are encountered at every step. By sifting and correlating this confusing mass of evidence, however, the author is able to give a clear and connected narrative of the changes in the Kilauea crater, and to illustrate the position of its floor after each of the great eruptions, which took place in 1823, 1832, 1840, 1868, and 1886. The result is that we are furnished for the

first time with the means of judging of the real nature of the processes going on in the pit-craters of non-explosive volcanoes. A similar discussion of the records concerning Mokuaweoweo, the summit-crater of Mount Loa, enables the author to furnish an interesting, but necessarily less complete, narrative of the operations going on there during the same period. The want of anything like synchronous action between these two great craters in the same mountain-mass, *one of which is at an elevation of* 10,000 *feet above the other*, has often been remarked upon; and the truth of the conclusion—one which must always be taken account of in attempts to explain volcanic phenomena—is fully established in the work before us.

Prof. Dana forcibly illustrates the remarkable contrast between the effusive eruptions of the Hawaiian volcanoes with their extremely liquid and perfectly fused basaltic lavas, and the explosive outbursts of Vesuvius, Krakatão, and Tarawera. He describes the characters and limited distribution of the curious glassy lavas, and their derivatives—the curious Pele's hair and the beautiful "thread-lace scoriæ"; and he points out the inaccuracy of the early chemical analyses of the Hawaiian lavas, which have misled so many subsequent writers. His remarks on the characteristics and origin of the chief varieties of the lava, and especially of the pseudo-bombs—vast pillow-like masses of lava covered with a thin vitreous crust—are remarkably interesting and suggestive.

One of the most valuable chapters in the book is that on the petrographical characters of the Hawaiian lavas, supplied by the author's son, Prof. E. S. Dana. The singular fissile basalts of the higher cone, which resemble phonolite, and several other remarkable types are here described for the first time. Very noteworthy are the curious feathery forms of augite which occur in some lavas, and the strangely-elongated crystals of olivine which are found in others. But the part of the chapter which will unquestionably awaken the greatest amount of interest in the minds both of mineralogists and geologists is that which deals with the curious stalactites found in certain caverns in the lavas. That these stalactites are formed by aqueous action there cannot, as Prof. E. S. Dana shows, be any reasonable doubt. Yet the stalactites are built up of crystals of felspar, augite, and magnetite, all the constituents formed by igneous action in the lavas themselves, being present, with the exception of olivine! All students of mineral synthesis are acquainted with the fact that the same species can often be formed by several, and sometimes by very diverse, methods. Mr. Sorby has even shown how fragments of quartz-crystals, originally formed in a granite or other igneous rock, may after enormous intervals renew their growth and become complete crystals again under purely aqueous conditions; so that the same crystal may in different parts be the result of totally different kinds of action. In spite of these facts, however, few petrographers would be prepared to find that, from aqueous solutions, rocks made up of felspar, augite, and magnetite could be formed in the way described in this interesting essay. Prof. E. S. Dana not unnaturally announces these remarkable conclusions with some diffidence and reserve; yet it is impossible to find any flaw whatever in the line of argument by which he seeks to establish their truth.

Prof. J. D. Dana has prefaced his description of the Hawaiian Islands by a sketch to which the title of "Characteristics of Volcanoes" is more directly applicable. In this introduction, which only extends to some 27 pages, many of the great problems of vulcanology are discussed with singular clearness and freedom from bias.

The work concludes with two interesting appendices, the first on "Volcanoes and Deep-sea Topography," and the second on "Denudation of Volcanic Islands; its Amount a Mark of Age." The book is well illustrated with maps and sketches, and some plates reproducing photographs will serve to give a just idea of the peculiar lava cascades and fountains of Hawaii—phenomena which have not unnaturally excited the imagination of untrained observers, and given rise to startling drawings and florid descriptions in popular works of travel. But the sober truth is, that the wonders of Hawaii stand in no need either of exaggeration or embellishment from the writer or the artist.

We heartily welcome the volume as the crowning labour of the greatest of America's men of science—the latest, and not by any means the least important, of a long series of contributions to science on very diverse subjects, but of unvarying excellence. J. W. J.

A POLYGLOT MEDICAL VOCABULARY.

Terminologia Medica Polyglotta: a Concise International Dictionary of Medical Terms. Compiled by Theodore Maxwell, M.D. Cantab. (London: Churchill, 1890.)

THE current literature of medical subjects is extensive and polyglot, and those who endeavour to keep themselves abreast of the most recent research in any branch require to dip into works in many languages, and need to have at hand some such aid as the present vocabulary, wherein they can seek for the several vernacular synonyms of those newer technicalities which modern developments of science have produced, and which are not to be found in the ordinary dictionaries. Moreover, it is often necessary that the special senses in which some of the older and more general words are used by medical writers should be defined. One may be very well acquainted with the anatomy of the brain as described in the English standard works, and yet have much difficulty in following the descriptions in German or French books on cerebral pathology, when vernacular names are used for the several parts; and one longs for some international agreement as to a uniform system of scientific terminology like the Latin generic and specific names of the Linnæan nomenclature.

The compilation of a new dictionary is a task involving an enormous amount of labour, and when, as in the work under notice, the synonyms of each term in seven languages have to be sought and tabulated, the difficulties of the undertaking are seriously increased. The compiler has evidently expended great care, and exercised much judgment in his toilsome task, and doubtless this vocabulary will prove of much help and be highly appreciated by students of foreign medical literature. The typography is excellent and clear, and the work is singularly free from errors of the press.

It is questionable whether the selection of French as the fundamental language was a wise choice. There are

fewer original terms in the French scientific vocabulary than in either English or German; fewer modern writings of value in the medical literature of France than in either of the other great European literatures; and fewer students of medical literature in French-speaking countries than in either English or German-speaking countries. The Russian element might also without very much loss be eliminated; for as there are no Russian-French references it can be of little assistance to anyone reading Russian literature, and will only be of value to the limited class of Russian students of other literatures, or the still more limited class of foreigners writing medical works in Russian.

The special function of a work like the present is to supplement, not to supplant, the ordinary dictionary; therefore such a work should be reduced to as small a bulk as possible. To this end there should be as little overlap as possible, as there is no advantage in including such words as are to be found in the ordinary dictionaries, unless there is something specific in their use to be explained. In the work before us, this principle has not been adopted, and its size has consequently been unduly enlarged by the introduction of many common words which have no such peculiarities. Thus, taking at random the pages 184-5, there are the words Heirath, Heavy, Heat, Heating, Hebung, Hebel, Heften, Heifer, Height, Heiss, Heizung, Helfen, Hell, Helm, Hembra, Hemd, Heaviness, Heiter, Helios. In these pages alone, one-fourth of the entries are common dictionary-words. (By inadvertence, Helios, the sun, is said to be Latin.) Turning at random to another page (445) there are seventeen words in a row—Wave, Wax, Weak, Weaken, &c.—of the same description. One might, perhaps, defend the introduction of the word "Stays" in the sense of corset, but one does not see why "Star" (étoile) or "Stamp" (timbre) should have space devoted to them.

By rigid adherence to a definite order of languages in the enumeration of synonyms, the bulk of the work has also been largely increased. Much space would have been saved, and the utility of the book by no means impaired, if, when the same word was used for the same idea in two languages, instead of the repetition of the word the initials indicating the languages had been prefixed to one entry of the word. Thus, instead of wasting three lines with "E. Opodeldoc, soap-liniment; G. Opodeldoc; I. Opodeldoc," or two lines with "I. Organo; S. Organo," E. G. and I. might have been prefixed to the one, and I. and S. to the other. There is no gain of clearness in the tabulation, and a distinct loss of handiness; for it is especially true in the case of a dictionary that the greater the book the greater the evil.

While in most cases the author has confined himself to the enumeration of synonyms, there are some words to which he has appended definitions. The principle upon which words have been selected for this distinction does not seem apparent; for instance, it was surely unnecessary to define "Faux (f. fausse), adj., qui n'est pas vrai." Some of the definitions are curious; thus, "delusion" is defined as "a belief in something incredible to sane people, resulting from diseased working of the brain convolutions." It is doubtless right that "Daft" should appear in a polyglot dictionary, but it can scarcely be reckoned as English, and is as much deserving of having its nationality indicated as "Knocked up," which is given as "(en Amérique) enceinte."

In a few cases inconsistencies of spelling have eluded the corrector's eye: thus, the adjective "Lacrymalis" is accurately given, but a few lines below the neuter form appears as "Os lachrymale"; Aneurysm is spelled with an *i*; but oversights of this kind are very few.

The author has adopted in some cases the useful plan of marking with an asterisk those words under which the full synonymy is given. In a few instances this has become misplaced; thus for the Latin "Caduca" the equivalent "membrana decidua *" is given, but there is no such entry under "membrana," and opposite "Decidua" there is simply the French synonym "caduque," which is the heading to which the star should have been appended.

There are some words which one would have expected to have place in such a work that are not to be found. Ache, Aching, Acromegaly, Caul, Limbus, Limbic, Lobe, Monoplegia, Laparotomy, are a few of these. Black alder is given, but neither black wash nor black draught. Red precipitate, Citrine ointment, Daffy's elixir, are surely as deserving of place as Dover's or James's or Gregory's powder.

Fault-finding is at all times an ungrateful task, but it becomes especially unpleasant when the subject is a work of real merit, and we have indicated these weaknesses so that in subsequent editions the usefulness of the work may be increased. If its size were diminished by the exclusion of ordinary dictionary-words, by the better grouping of those that are identical, and by the judicious excision of unnecessary definitions, a portable, useful work would be produced, which we doubt not would find its place on the desk of the majority of students of foreign medical literature.

ALEX. MACALISTER.

MASKS FROM NEW GUINEA AND THE BISMARCK ARCHIPELAGO.

Masken von Neu Guinea und dem Bismarck Archipel. By A. B. Meyer. *Königliches Ethnographisches Museum zu Dresden.* Band VII. Folio, pp. 15, Plates 15. (Dresden: Stengel and Markert, 1889.)

DR. A. B. MEYER has written the seventh of the series of fine publications of the Royal Ethnographical Museum of Dresden which are brought out under his direction. He has selected for description and illustration the masks from New Guinea and the Bismarck Archipelago which are to be found in the collection under his care. The descriptions are as a rule very brief, but they are to the point, and indicate the zoological training of the author. The latter is shown not only by the precision of the descriptions, but also by the addition of the generic name to the animals represented by the masks or used in their adornment. Of the 83 specimens in the Dresden Museum, 61 have been illustrated in this memoir in a most admirable manner by a photographic process the excellence of which leaves little to be desired. On comparing these photographs with woodcuts of similar objects, the advantage of the former is at once apparent, as the texture of the various substances used in the manufacture of the masks is faithfully rendered, and

the faultiness of the original design or pattern is not glossed over by an engraver. It is a great pity that the magnificent collections in the British Museum cannot be rendered available for home study by the publication of similar photographs.

It is to be regretted that Dr. Meyer confined his account of the masks to those contained in the Dresden Museum, and has not compared these with the specimens which are to be found in other museums.

A good opportunity for a thorough treatment of the subject has thus been lost. For example, allusion is made to the occurrence of masks in the Elema district of the Papuan Gulf, but no description or figure is given of them, although numerous specimens of these have found their way into museums. Of the eight masks which are figured from Torres Straits, one of the most characteristic varieties is unrepresented—that one which represents a crocodile's head surmounted by a human face. A fret pattern occurs on a mask from Jervis Island. This is alluded to by Dr. Meyer, and is compared with somewhat similar patterns, of which woodcuts are given, on two masks from German New Guinea, and with two patterns on arrows from Dutch New Guinea. The Torres Straits pattern, unlike the others, is precisely similar to the common form of the pattern, and as it does not occur on other objects from that district we can only conclude, contrary to Dr. Meyer, that it was directly copied from some introduced object; the same mask is further ornamented with some imported red woven material. Dr. Meyer suggests that the helmet masks from New Ireland, and the feather helmets and masks from the Sandwich Islands, are reminiscences of the helmets of the Spanish voyagers of the sixteenth or seventeenth century. He also considers it probable that the use of masks in this part of the world originated in New Ireland, and extended through New Hebrides to the northern portion of the German territory of New Guinea, and thence by an overland route to the head of the Papuan Gulf and Torres Straits. Other routes were northward to the Caroline Island, Mortlock, and south-east to New Caledonia. Dr. Meyer has been able to discover very little concerning the uses of masks; all that he can say is that they are used in "masquerades, festivals, general feasts, secular, religious, and war dances." It is, however, very probable that particular kinds of masks are used for definite occasions, and that the masks which are worn say during initiation ceremonies could not be put on at a seasonal festival. There is no evidence, so far as British New Guinea is concerned, that masks are ever worn at the festive or secular dance, or at the war dance; they appear to have a definite sacred or religious significance.

This valuable memoir concludes with an interesting quotation from Weisser's paper on masks from New Ireland. Early in May the men of one village repair to another village with which they have a feud. Each man then puts on the mask which he has been secretly preparing during the previous year, and the men of the one village dance opposite to those from the other. After this they have a feast, and exchange sago cakes, which they eat with caution, fearing poison; criticism of the masks of the opposite faction affords ample opportunity for the continuance of the animosity.

A. C. H.

OUR BOOK SHELF.

Larva Collecting and Breeding. By the Rev. J. Seymour St. John, B.A. (London: William Wesley and Son, 1890.)

THE alternative title of this little volume, which is of convenient size for the pocket, is "a hand-book to the larvæ of the British Macro-Lepidoptera and their food plants; both in nature and in confinement, with authorities," and is sufficiently explanatory of its scope and objects. The arrangement of the first portion of the book is entomological, of the second and concluding portion botanical. In the former the larvæ are arranged and named according to "The Entomologist Synonymic List of British Lepidoptera," and the food plants are enumerated as subsidiary to these. In the second half the food plants are specified in the order of the "London Catalogue of British Plants" (eighth edition). The book is therefore susceptible of a twofold use; it will induce the entomologist to become a field botanist, and conversely it will greatly aid the student who has some knowledge of the native flora in his efforts to become practically acquainted with the lepidopterous larvæ. So much energy is misdirected, particularly by young people, in making collections of butterflies and moths for the mere sake of collecting, that the intelligent use of this little book is calculated to effect a salutary change. It will, at least, direct greater attention to the life-histories of the Lepidoptera, and if it should be instrumental in inducing the collector to preserve and mount the larva alongside the male and female specimens of the mature butterfly or moth, so much the better. It is too common a practice to ignore the "grub" as unlovely and despicable; though from an economic point of view it possesses a higher interest than the winged insect, and is certainly not inferior to it in importance from a scientific standpoint. Nearly all the Lepidoptera which are familiar in this country as crop-pests are actively injurious only in the larval stage.

As the author intimates, such a work as this is necessarily a compilation, and, from its very nature, it is hardly possible to make it exhaustive. All who use it in the field will find opportunities to annotate and amplify it, and possibly to suggest emendations. The common names as well as the systematic names of the plants are given, and it might be useful if in a future edition the common names of the insects were, as far as possible, also enumerated. A few misprints have escaped notice, as *Galium sexatile* (p. 103), and *Rynchospora alba* (p. 137).

Mr. St. John's book represents a good idea well carried out, and it should have the effect of stimulating the study of natural history in the field.

Practical Chemistry for Medical Students. By Samuel Rideal, D.Sc. (Lond.), F.I.C., F.C.S., F.G.S. (London: H. K. Lewis, 1890.)

THIS book is intended by the author to embody the tests for those substances which a medical student is required to identify at the first examination of the Conjoint Examining Board in England. The attempt to compress this information into 53 small pages has resulted, as might have been expected under the circumstances, in a cram-book. Indeed, the only justification, if such it can be called, for the addition of another to the many works on qualitative analysis is that the book contains in the minimum space the knowledge required for a special examination. This knowledge is, however, frequently of a questionable nature. Thus, "calcium sulphate, $CaSO_4$ (*gypsum*)," is described as a "white *amorphous powder*;" "sodium carbonate, Na_2CO_3," as a "white solid, crystalline *or* amorphous;" "ferric chloride, Fe_2Cl_6, a yellow amorphous *powder*," and so forth: statements of a kind which, although they constitute a large portion of the book, are both fragmentary and inaccurate. The endeavour to

attach valency values to the metals is carried out in all cases with the exception of iron, to which no value is affixed. The reason for this omission is not obvious, as the author does not hesitate to call lead a dyad, antimony a triad, &c. Amongst minor points the use of potassium antimony tartrate for potassium antimonyl tartrate, of arsenic acid for arsenic pentoxide, may be noticed.

The book may go some way to fulfil the author's expectation that it will give the student "some acquaintance with the art of test-tubing," but that it will materially increase his knowledge of the principles of practical chemistry, or sharpen his appreciation of the *raison d'être* of a chemical process, is another matter.

Manual of Pharmaceutical Testing. By Barnard S. Proctor, F.I.C. Pp. vii., 176. (London: *The Chemist and Druggist*, 1890.)

THIS book is a collection of tests suitable for ascertaining the purity of the chemicals of the British Pharmacopœia, &c. The tests described are the simplest possible, and can be carried out with the apparatus and chemicals in use at the dispensing counter. They apply more especially to the impurities of manufacture than to adulterations. In many cases they are simply those recommended by the British Pharmacopœia for determining if the purity of a material falls short of the required standard. As a rule they are qualitative, and sufficiently accurate for the purpose in view, although quantitative methods, more especially in determinations of solubility, or fixed residues of volatile liquids, are employed. The book contains a chapter on manipulation, which includes the method of weighing precipitates, and an index, and will be found a handy volume to the pharmacist.

The Encyclopædia of Photography. By Walter E. Woodbury. (London: Iliffe and Son, 1890.)

THIS work, which will be concluded in about twelve parts, is written on the same lines as other photographic encyclopædias, but treats especially of the sciences of optics and chemistry. The art of photography being so largely practised nowadays, it is curious what a small percentage of those who have taken it up know anything about optics or chemistry, which form the basis of the whole subject.

Throughout the book the author has borne this well in mind, and has spared no pains to place before the reader, in a simple and clear manner, the principles underlying the formation of images, the construction of lenses, chromatic and spherical aberration, the theory of atoms and molecules, and many other very important points relating to optics and chemistry.

The illustrations, which will be about 200 in number, consisting of explanatory sketches and diagrams, will be found, if up to the standard maintained in this first part, to serve their purpose well.

For amateurs this encyclopædia should be very useful, as it is written especially for beginners, and some of the most complicated terms likely to lead to confusion are avoided as much as possible.

Dynamics for Beginners. By the Rev. J. B. Lock, M.A. Third Edition, stereotyped. (London: Macmillan and Co., 1890.)

THE author has fully succeeded in supplying the want that has been long felt, of a book which should explain the elementary principles of dynamics, illustrating them by easy examples in a manner suitable for use in schools with boys of ordinary mathematical attainments.

Section I. deals with rectilinear dynamics, in which the fundamental principles are explained. The words "velo" and "celo," abbreviations for unit velocity and unit acceleration respectively, are here used, and the author says in the preface, "Of their value for the purposes of teaching and explanation I have received the very strongest testimony from those best qualified to judge."

Sections II. and III. treat of "Direction" and "Illustrations," the former dealing with the parallelograms of distances, velocities, and accelerations, chords of quickest descent, &c., the latter with projectiles, oblique impact, relative motion, hodograph, &c.

Work, energy, power, are discussed in Section IV., and there is a chapter on the indestructibility of matter.

An excellent set of examples is collected at the end, and a series of examination papers is added, taken from the various examinations held from time to time at Oxford and Cambridge.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

"The Climates of Past Ages."

I FEEL somewhat disappointed not to see a flood of correspondence in your pages arising out of Dr. Neumayr's very interesting lecture on the climates of past ages. The subject is difficult and complex, and the factors of the problem are no doubt various and of different kinds. I wish to make a few remarks on some of these.

It seems impossible to doubt that the sun is losing heat; and, consequently, that the quantity of heat annually received by the earth from the sun is less than it once was. Now, one of the most remarkable of the facts before us is the evidence, from fossil vegetation, of comparatively warm climates in the polar regions. There is no similar evidence respecting the equatorial regions; but it is probably impossible that such evidence should be preserved, so that its absence proves nothing as to the equatorial climate of the same period; but it is worth noticing that, if we suppose the force of solar radiation increased, the increase of terrestrial temperatures will be greater in high than in low latitudes, because, with the increased quantity of heat received into the atmosphere, an increased quantity will become latent by evaporation in the lower latitudes, and will be carried to the higher latitudes by vapour-bearing winds.

If our planet had neither atmosphere nor ocean, the temperature of the higher latitudes could be raised only as a direct result of increased solar radiation. If it had an atmosphere but no ocean, the increase of the temperature of the higher latitudes would be assisted by heat-bearing winds; and this would be at the expense of the temperature of the lower latitudes, which would be lowered by the heat so carried away. In the actual case of our earth, with both atmosphere and ocean, this action will be greatly increased by the power of vapour-bearing winds to carry heat in the latent form, which again becomes sensible heat on the condensation of the vapour. This appears to show that a considerable increase of temperature might be produced in the higher latitudes by a comparatively small increase in the force of solar radiation.

Dr. Neumayr says that the cause of the glacial climate is quite unknown; and at the same time he asserts that both hemispheres—the northern and the southern—were glaciated at the same time. I dispute both of these opinions. I think Mr. Croll has shown the direction in which the explanation of the glacial climate is to be sought; and if this is so, the two hemispheres were not glaciated at the same time, but alternately.

If, during a glacial period, the northern and the southern hemispheres were each alternately glaciated for geologically short periods, this would account for the fact mentioned by Dr. Neumayr, that the glacial period appears not to have been continuous, but interrupted by periods of milder climate. Croll's theory accounts for this. His theory is, that glacial periods occur at those astronomical epochs when the eccentricity of the earth's orbit is at its greatest; and a glacial climate is produced in the two hemispheres alternately, according as the

summer of each hemisphere is in perihelion or in aphelion. This, in consequence of the precession of the equinoxes, will occur at intervals of about 25,000 years. That is to say, if in either hemisphere the summer is now in perihelion, at the end of 12,500 years its summer will be in aphelion, and in 12,500 years more it will be in perihelion again. Mr. Croll maintains that glaciation occurs in the hemisphere where there is perihelion summer and aphelion winter, because of the intense cold of such a winter. I think, on the contrary, that the facts of climate which come under our observation show that winter cold has little or no effect in producing glaciation; and that a cold summer, which leaves the winter snow unmelted, is the most favourable condition for glaciation. Such is the climate of the Antarctic continent now. It is obvious that a summer in aphelion, when the eccentricity of the earth's orbit was many times greater than now, must have been a very cold summer.

This theory of the glacial climate appears perfectly satisfactory. The astronomical cause is known to exist, the geological effects are known to exist, and the effect is that which the cause must necessarily produce.

Even if it were true that a glacial climate prevailed in both hemispheres at the same time, no geological evidence could prove such a fact. No geological evidence could tell whether glacial mounds in Norway and in Patagonia, for instance, were strictly contemporary, or separated in date by an interval of 12,000 years.

Dr. Neumayr appears to retain the old notion that changes of climate may be to some extent due to changes in the position of the earth's poles. I am no mathematician, and cannot speak on such a subject with any authority, but Sir William Thomson believes he has proved that the earth is for all dynamical purposes perfectly solid and rigid; and I should think that the axis of rotation of a perfectly rigid oblate spheroid is unchangeable.

Belfast, July 10. JOSEPH JOHN MURPHY.

The American Meteor.

I RECEIVED the following observations from my son, G. S. Henslow, who witnessed the fall of the meteor referred to lately in NATURE. I forward it, as it may perhaps interest some of the readers of this journal.

"The meteor fell about 5 p.m., and divided in mid-air, part of it falling in Minnesota near a town called Kasota; this portion was not found. The other and larger piece fell near Butt City, Iowa. The two places are about a hundred miles distant. It exploded on reaching the ground into myriads of fragments, a number of which have been picked up and sold at fabulous prices. The State University of Minnesota bought the largest piece. It fell on the open prairie, but broke into such small fragments that the surrounding soil was scarcely disturbed at all. We all saw it fall here at Windom. It illuminated the southern sky, and left a cloud resembling the smoke from the funnel of an engine. On bursting, there was a sound like a sharp peal of thunder." G. HENSLOW.

SPONTANEOUS IGNITION AND EXPLOSIONS IN COAL BUNKERS.

AT the Royal United Service Institution, on Friday, July 4, a paper on this subject was read by Prof. Vivian B. Lewes, Royal Naval College. Rear-Admiral N. Bowden-Smith was in the chair.

The lecturer, after premising that in the fast ocean steamers it is now becoming an event of frequent occurrence for the contents of the bunkers to spontaneously ignite, whilst in the Service such a thing as fire in the bunkers is practically unknown, and an occasional, although fortunately very rare, explosion of gas is the worst trouble which the coal stores of our naval monsters have given rise to, directed attention to the causes which give rise to the so-called "spontaneous ignition of coals," and traced the particular circumstances which tend to increase the tendency to it.

The pyrites or coal brasses present in the coal when exposed to dry air undergo little or no change, but when moisture as well as air is present they absorb oxygen and combine with it, forming sulphates of iron, and the ordinary explanation of the spontaneous ignition of coal is that this process of oxidation causes a rise of temperature in the coal which determines its ignition; this, however, has of late years been much doubted, and it can now be proved that the pyrites when present in ordinary quantities are perfectly incapable of doing more than adding slightly to the general rise of temperature, although when present in very large masses they may increase the tendency of the coal to spontaneous combustion by swelling during oxidation, and causing the coal to crumble, and also by setting free sulphur, which, having a lower melting-point of ignition than coal (482° F., or 250° C.) would lower the temperature at which the mass would catch fire.

The real causes which give rise to heating and ignition in any large accumulation of coal are twofold. First, the absorption of oxygen from the air by the carbon; and secondly, the chemical action set up by the absorbed oxygen with the hydrocarbons of the coal.

The most important point to be noticed is the extraordinary effect which initial temperature has on the rapidity of chemical actions of this kind. At a low temperature, and indeed up to about 100° F. = 38° C., the absorption of oxygen, and consequent chemical action, will go on slowly with practically little or no chance of undue heating taking place, but directly the temperature exceeds 100° F., then, with some classes of coal, ignition is only a question of time and mass.

Although the ignition point of various coals lies above 700° F., yet if many of these coals are powdered, and are placed in perforated zinc cases in masses of 2 lbs. or upwards, and these are kept at a steady temperature of about 250° F. in an oven, ignition will generally follow in a few hours; whilst between this and 150° F. it will take days instead of hours for the same result to follow, and at ordinary English temperatures several thousand tons of coal would have to be stored in a very broken condition before any risk of heating or ignition would ensue. In considering this question with regard to coal bunkers, it must be remembered that, although the considerations which had to be taken note of in the case of coal-laden ships still exist, yet they are considerably modified by the smallness of the amount of coal carried, and by the methods of loading and storage employed.

Liability to spontaneous ignition increases with:—

1. *The increase in the bulk of the cargoes.*—Evidence given before the Royal Commission of 1875 showed that in cargoes for shipments to places beyond Europe the cases reported amount to ½ per cent. in cargoes under 500 tons; in cargoes from 500 to 1000, 1 per cent.; 1000 to 1500, to 3·5 per cent.; 1500 to 2000, to 4·5 per cent.; and over 2000 tons, to no less than 9 per cent. Mass influences this action in two ways:—

(*a*) The larger the cargo, the more non-conducting material will there be between the spot at which heating is taking place and the cooling influence of the outer air.

(*b*) The larger the cargo the greater will be the breaking-down action of the impact of coal coming down the shoot upon the portions first loaded into the ship, and the larger thereby the fresh surface exposed to the action of the air.

2. *The ports to which shipments are made* (26,631 shipments to European ports in 1873, resulting in only ten casualties, whilst 4485 shipments to Asia, Africa, and America gave no less than sixty).—This startling result is due to the length of time the cargo is in the vessel, the absorption and oxidation being a comparatively long action, but a far more active cause is the increase of temperature in the tropics, which converts slow action into a rapid one.

3. *The kind of coal of which the cargo consists* (some coals being especially liable to spontaneous heating and ignition).—There is great diversity of opinion on this

point, but it is pretty generally admitted that cases of heating and ignition are more frequent in coals shipped from east coast ports than in South Wales shipments. So much, however, depends upon the quantity of small coal present, that a well-loaded cargo of any coal would be safer than a cargo of Welsh steam coal in which a quantity of dust had been produced during loading.

4. *The size of the coal* (small coal being much more liable to spontaneous ignition than large.)—This is due to the increase of active absorbent surface exposed to the air, a fact which is verified by the experience of large consumers of coal on land; gas managers recognizing the fact that coal which has been stamped down or shaken down during storage is more liable to heat than if it has been more tenderly handled, the extra breakage causing the extra risk.

5. *Shipping coal rich in pyrites* (*or brasses*) *whilst wet.*—The effect of external wetting on coal is to retard at first the absorption of oxygen, and so to check the action; but it also increases the rate of oxidation of the pyrites, and they, when oxidized, swell and split the coal into pieces, and this increases heating due to the exposure of fresh dry surfaces.

6. *Ventilation of the cargo.*—For ventilation to do any good, cool air would have to sweep continuously and freely through every part of the cargo—a condition impossible to attain in coal cargoes—whilst anything short of that only increases the danger—the ordinary methods of ventilation supplying just about the right amount of air to create the maximum amount of heating. The reason of this is clear. A steam coal absorbs about twice its own volume of oxygen, and takes about ten days to do it under favourable conditions, and it is this oxygen which, in the next phase of the action, enters into chemical combination, and causes the serious heating. Ventilation, such as used to be sometimes arranged for by a box shaft along the keelson with Venetian lattice up-shafts, supplies about as much air as is necessary to produce the results which end in spontaneous ignition.

7. *Rise in temperature in steam colliers, due to the introduction of triple-expansion engines and high-pressure boilers.*—The increase in stokehold temperature, due to this, is from 5° to 10° F., and this affects the temperature of the adjacent parts of the vessel.

In the coal bunker, the question of mass, which plays so important a part in a hold laden with coal, is almost entirely eliminated, as 50 to 400 tons would be about the capacity of any ordinary bunker, and the cases of spontaneous ignition in masses of coal less than 500 tons do not amount to more than ¼ per cent. The question of initial temperature, therefore, becomes the one important factor. Bunker fires are almost entirely confined to vessels in which the bunker bulkheads are only separated from the funnel by a narrow air-space, or are in close proximity to the boilers themselves; but where the bunkers are stepped back from the funnel casing and boilers, spontaneous ignition is a great rarity. If coal is kept at a high temperature, even though it be far below its igniting point, ignition is only a question of time, and if the bunker coal next the bulkhead is kept at 120° F., any coal with a tendency to absorb oxygen will run a great chance of igniting within a few days. In order to prevent spontaneous combustion of the coal under these circumstances, all that is necessary is to reduce the temperature of the bulkhead in contact with the coal, as if this is kept at a temperature not exceeding 80° to 90° F., there is little or no fear of the oxidation of the hydrocarbons of the coal proceeding with such rapidity as to cause ignition in such a quantity of coal as can be carried in the bunkers, the iron decks, by subdividing the mass, also helping to reduce any risk. In order to reduce the temperature to the required extent, it would be necessary to make the bulkheads close to any heating surface, such as the funnel casing, double, and the side spaces six inches apart, the inner wall being provided at intervals with water-tight openings, through which the interior space can be coated with protective compositions from time to time. Through this double casing sea-water would be allowed to circulate very slowly, and would effectually prevent any undue rise of temperature, whilst to make the arrangements complete a thermostat should be fixed on the inner plate of each bulkhead, which, if the temperature rose to 100° F., would ring a bell in the captain's room, when the rate of flow of water could be increased until the required fall in temperature took place. Should this arrangement prove impossible from any structural cause, then a rapid current of air forced through the bunkers by means of a fan, or even an up-current formed by a good air-pump ventilator in the crown of the bunker, would go far to keep the temperature within safe limits. If such an arrangement were adopted in the fast liners, bunker fires would become a thing of the past, whilst such an arrangement of double bulkhead and water circulation would also solve the still more important problem of how to keep the magazines on board Her Majesty's ships at a sufficiently low temperature to fit them for the storage of E.X.E. and S.B.C. prism powders, and the still more delicately constituted smokeless powders, none of which could otherwise be kept in the auxiliary magazines of the new programme ships; as for safety they are placed between the boilers, and must, of necessity, reach a temperature far above that which any powder could stand without losing moisture, and in consequence developing far higher strains than the guns should properly be subjected to.

The question of explosions in coal bunkers and in the holds of coal-laden ships is a subject totally distinct from that of spontaneous ignition. During the conversion of woody fibre derived from various forms of vegetation into coal, considerable quantities of a gaseous compound of carbon and hydrogen, called methane, marsh-gas, or light carburetted hydrogen, is evolved, and as the action has been spread over long ages most of this gas has found its way to the surface of the coal seam and has diffused itself through the superincumbent soil and has escaped; but a portion has been occluded (absorbed) in the pores of the coal itself, and some also imprisoned in small cavities and fissures in the coal. Marsh-gas, when pure, is perfectly non-explosive, and burns quietly with a faint luminous flame, producing, as the products of its combustion, carbon dioxide and water vapour, but when mixed with ten times its own volume of air, and a light applied, it explodes with a force equal to about 210 lbs. on the square inch. Another cause which tends to increase the danger of explosion is that if the air is charged with fine coal-dust, less than one per cent. of marsh-gas mixed with it gives an explosive mixture, and also extends the area of explosion. In both colliers and coal bunkers the risk of explosion is greatest during the first ten days after shipment.

Marsh-gas is a non-supporter of combustion, so that the presence of the gas, or a mixture of it with air, if present, is a safeguard against spontaneous ignition; and if the precautions pointed out to prevent ignition were carried out in conjunction with simple precautions against explosion, explosions and fires in coal cargoes and bunkers would soon be a thing of the past.

The lecturer strongly advocated the adoption in the bunkers of all new vessels of the double bulkhead, and water circulation to such portion of the bunkers as impinge upon any unduly heated portion of the hold, and that all bulkheads should be made gas-tight; whilst in bunkers containing not more than 300 to 400 tons of coal, as thorough ventilation as possible should be obtained by fitting water-tight air-pump ventilators in the deck above the surface of the coal, while inlets for as cool air as possible should be provided at the bottom of the bunkers, and, where necessary, air driven in from the

fan. Under no conditions should any but safety-lamps be used in coal holds or bunkers.

A discussion followed, and the proceedings closed with a vote of thanks to the lecturer.

A WINTER EXPEDITION TO THE SONNBLICK.[1]

IT is not often that an Alpinist finds leisure to spend a month in winter at an altitude of 10,154 feet above the level of the sea. It may, therefore, interest the members of the Alpine Club, to have the experiences of one who, though not a member of their Society, yet was fortunate enough to make the unusual ascent, which was chiefly undertaken in the interests of science.

It is well known that since 1886, thanks to the united efforts of the Alpine Club, and of the Imperial Austrian Meteorological Society, and in a special manner to the energy and public spirit of Herr Ignaz Rojacher, there is now a thoroughly equipped Observatory on the highest peak of the Sonnblick. This Observatory has been established with the view of affording to students of natural science, physics, astronomy, and meteorology, the means of making such observations as are only practicable at great heights; and of providing them with accommodation in a part of the building which has been named by the owner "The Study."

In carrying on certain inquiries which are only to be solved on high mountains, I had for this purpose spent a month in the summer of 1881 on the Hoch Obir (6716 feet) in Carinthia, and I determined the first winter after the erection of the Observatory on the Sonnblick still further to resume the investigations in a situation which afforded a clear, cold, winter atmosphere, which was absolutely necessary. I was unfortunately unable to realize my intention the first winter (1887), which was the more to be regretted inasmuch as the winter of 1887, and especially the month of February, was unusually fine, whereas that of 1888 was the severest ever known. The "oldest inhabitant" of those parts had no remembrance of such heavy falls of snow and such dark and stormy weather as we experienced in the February of 1888—the month for which I had made all my arrangements for an expedition to the Sonnblick.

My expedition was undertaken with the following objects:—(1) To investigate the radiation of the earth into space, and the irradiation of the atmosphere upon the earth's surface, in order to ascertain, more accurately than had hitherto been done, the temperature of the aërial envelope of the earth. (2) To investigate the question of the blueness of the sky. (3) To discover whether the sparkle of the stars was altogether due to the lower strata of air. Having had a grant from the Imperial Academy of Sciences in Vienna for the purpose, I succeeded in enlisting the services of Dr. Trabert, a young indefatigable man of science, as assistant, to make simultaneous observations on the Rauris, whilst I observed on the Sonnblick.

We reached Lend on the morning of February 3, where we handed over our seven cases of scientific instruments, and my provisions for a month's sojourn on the Sonnblick, to Herr Rojacher's men, who conveyed the whole on a couple of sledges through Embach to Rauris; we driving to Kitzloch Rauris, where we found Herr Rojacher awaiting us, and, after a tough climb of an hour and a quarter up the mountain pass of Kitzloch, we proceeded by sledge to Rauris.

This first day was perhaps the finest during our stay in the Rauris Mountains; on the next, it began to snow; and it was in a heavy snow-storm that I had to set out for Kolm; and so heavy was it, that it was with the greatest difficulty that Rojacher and I, in our sledge, followed by the *Rossknecht* with my baggage, were enabled to reach the Bodenhaus. From thence, through the woods, to Kreuzbichl, the snow fell thicker and thicker, and it seemed as if we should never get to our destination. Beyond Kreuzbichl there was no path of any sort, and we had simply to wade through the deep snow for fully an hour, before we reached Kolm, Herr Rojacher's residence (5249 feet). On my arrival, I was just in time to telephone to Rauris that I had reached so far in safety, the telephone communication being immediately thereafter interrupted. That journey from Rauris to Kolm had given me some idea of what a snowstorm in those regions meant. The avalanches caused by the weight of snow, had broken down the telephone wires, completely burying them, and, in one place, carrying them away for a distance of over two kilometres.

The *Rossknecht* had just reached Bodenhaus, but was utterly unable to push on further. It was four days before all my cases could be brought on to Kolm; and then the men had to carry them on their backs. Here was I, cut off from the world, snowed up at Kolm, and with little apparent prospect of getting to the Sonnblick; the snow falling faster and faster for four whole days, without intermission. But I was thankful enough to have reached there, for the valley beneath was laid waste with avalanches, making the roads impassable. However, the five days in which I was blockaded at Kolm were anything but wearisome. I could well have undergone a longer imprisonment with a companion so ingenious and intelligent as Rojacher. He had always some interesting subject to discuss, or new problem to set concerning the Tauern range. What perhaps interested me the most were his descriptions of winter life in this inhospitable altitude—its pleasures and difficulties, and particularly his explanation of the *Lahnen*, the local word for avalanches.

There are two kinds of *Lahnen*, he explained, *Windlahnen* or *Windsbretter* (wind avalanches), and *Jauk* or *Grundlahnen* (ground avalanches). The first belong exclusively to winter; the second to spring. These last are the avalanches of which people who live far out of the reach of avalanches have formed the one and sole idea of their nature and composition, thus confounding the two. They are, however, totally different.

The action of the ground, or *Jauk*, *lahn*, as its name denotes, is to break away from its base on the ground; and, as its second name denotes, mostly in consequence of warmer temperature, *i.e. Jauk*, south wind. It is composed of a huge mass of melting snow saturated with thaw water, that, restrained by the enormous friction of the earth, carries slowly along with it everything that impedes its course. It is set in motion when the moisture of the thawing ground has sufficiently diminished the earth's friction which has hitherto held it back. It needs no propelling medium; its own weight causes it to slide. The prevailing idea that any small particles of snow set primarily rolling by a bird, or any such unimportant agency, can gradually increase to the dimensions of an avalanche, is a pure fallacy. The rolling is a secondary matter; the primary agent in an avalanche is its sliding. They travel slowly, Rojacher said—that is, there is mostly time for escape on first hearing the roar of the heavy falling mass; with the *Windlahn* is no such hope, as both Rojacher, and all others whom I questioned, assured me.

The *Windlahn* he explained in the following manner. The first falls of winter snow fill up all inequalities of the surface. If it lies for a time, it consolidates and forms an even, slippery surface. More snow falling upon this smooth surface has a tendency, by its own weight, to slide off. This is certain to occur if after a heavy fall of snow the new layer has acquired such weight that its pressure overcomes the slight resistance of the underlying stratum, and any chance obstacles that hold it back. As soon as the top pressure is great enough to start a fissure, the

[1] By Dr. J. M. Pernter, of the Imperial Academy of Sciences in Vienna.

whole mass of the fresh-fallen snow sweeps with the velocity of the wind from off the slippery surface beneath. That is a *Windsbrett*, or *Windlahn*; so called, not that it is caused by the wind, but that in its headlong passage its velocity creates a storm wind which in its turn commits ravages and devastation far beyond the range of the falling avalanche.

I had many opportunities, while at Kolm and on the Sonnblick, of witnessing those terrible avalanches. During the night of February 4–5, a *Windsbrett* fell from Bucheben, filling the whole valley beneath for a distance of two kilometres with 13 feet of snow. The avalanche itself could not force its way up the side of the opposite mountain, but the wind caused by it unroofed a farmhouse, 650 feet above the valley, and blew in the windows.

The day I started for the Sonnblick, a *Windsbrett* parted from the Hoch Narr Glacier, causing such a terrific gale of wind in Kolm that the people were in terror of their lives. The next day we looked down from the Sonnblick on the snow-field whence the avalanche had parted, and Rojacher and his assistant, Peter Lechner, estimated its length and breadth at 650 feet, and depth 13 feet, representing a fallen mass of at least 160,000 cubic metres.

One peculiarity of wind avalanches, that makes them such a special danger to tourists, is that it is so easy to start one unawares. On an inclined, slippery surface of hardened snow, there lies a thick superstratum of fresh-fallen snow, ready, so to speak, to slip away at any moment. It often requires but the weight of one man, and there are generally at least two, to produce the slight pressure that sets loose the avalanche. In such a case there is heard a dull thundering crack, immediately after which, either the mass of snow starts, in which case the men are borne down on it with the swiftness of the wind, seldom to be seen again; or, after the first crack, the mass remains stationary, the *Windsbrett* has "settled," and the travellers proceed scatheless on their way.

I underwent such an experience during my ascent of the Sonnblick, not without considerable alarm, I must confess. Not far from the miner's lodge, at about 7550 feet of altitude, we had to cross a snow-field on a considerable incline. There were fifteen of us, with Rojacher and myself. Arrived at the middle of the incline, we heard a terrific muffled crack. We had started the *Windsbrett*. For a moment we knew not whether to go on or go back, the next we found that we had escaped with the fright—the avalanche had "settled."

It is not easy to say what are the causes that hold back an avalanche once started. It seems as if the "settling" of a *Windsbrett* only occurs when passed along at its topmost end; at any rate, prudence suggests that it is the only safe path to cross one; for, in the event of its giving way, the best hope of safety is to be on the highest point of the falling mass; there is, at least, the possibility of being able to obtain a foothold above, and thus of not being crushed by the on-coming snow. Should the *Windsbrett*, after being started, remain stationary, it is in all probability due to the fact that the lower part of the snow-field is too massive to be set in motion by the unsettlement of the upper portion, and therefore does not partake in the movement. Thus the former "settles."

The account above given of *Windsbretter* will explain why the inhabitants of the regions where they are to be met with maintain that it is next to impossible to escape with life from them. Once hear the fatal crash, the avalanche is upon them, and there is no escaping from it. Their advice is, to throw oneself prostrate, with hands outstretched, if possible behind some rock or boulder; there is the chance that the *Windsbrett* may pass over him, and if buried in the snow, one would be in the most favourable position to breathe, and therefore stand the best chance of being dug out alive; while to stand upright would be, to a certainty, to be carried with it. There were many such cases among Rojacher's people during my stay on the Sonnblick. This and similar talk made the time pass agreeably enough while I was waiting at Kolm.

While thus employing ourselves, Rojacher spoke through the telephone from time to time to his men in the station (Berghaus), 7870 feet above, asking if some thirteen or fourteen of them could venture down to take up my cases. For the first four days, the invariable answer was that there was too great danger of avalanches to undertake the descent; on the fifth day at noon they decided to venture down upon their *Knappenrossen*.[1] Barely an hour after we saw them come tearing down the declivity behind the Kolm house, or rather saw but a thick cloud of snow coming towards us, amid which an occasional hat, or alpenstock, was discernible. After the men had well warmed themselves, and had invigorated themselves with draughts of hot wine, my traps were distributed among them, and at 3 o'clock we started for the station. Our ascent was effected by means of snow-shoes, we keeping carefully to the rut made by the men on their passage down. There were no deviations, the snow had so completely filled up all uneven places, covering rocks and stones with its thick mantle, that it was one straight path. Our ascent was comparatively easy, and in three hours we had reached the Miner's House (*Knappenhaus*), after having, as already related, had a considerable panic from a *Windsbrett* some twenty minutes before.

The weather, which had, so far, been tolerably favourable, had changed for the worse during the night, and I expressed my fears to Rojacher in the morning, that we should be snowed up there for some days. But his calm reply was, "Once so far, we must reach the Sonnblick before dusk, cost what it may." To my objection that we might run the danger of avalanches, he laughingly said, experience had shown him that they had no love for him. It would be an unheard-of thing for one to travel his road. His confidence reassured me, and I made no further demur to continuing our route.

Rojacher, however, added other ten men to our escort, whose duty was to go first and tread down the snow on the way to the plateau, where he expected to find the fall had been much less heavy, and where the extra men could then load themselves with the store of wood, already stacked, for the use of the house on the Sonnblick. Our party now assumed a somewhat droll appearance, marching along in Indian file, across the vast snow-fields. During the whole way to the top we were enveloped in a dense mist; and our ascent through the stupendous masses of fresh-fallen snow, was a very slow one. The first man, the pioneer, sank up to his hips at every step, despite the snow-shoes; in five minutes his strength was exhausted and he fell out, taking his place as the last but one; I always remaining the twenty-fifth man, which made the ascent comparatively easy to me. As each man placed his left foot exactly in the left foot-print of the one who preceded him, and his right foot in the right foot-print, I, as last man, had firm ground to tread, my one care being to plant my feet well into those spaces, and thus I reached the summit but little fatigued. We had taken four hours to make the ascent; and it had enabled me to form some idea of the incredible bulk of snow that can collect on the Hochgebirge. Even on the upper plateau, the snow of the last four to six days had reached a depth of ten feet. This was proved to us, on coming up to the wood-stack. It had been carried up before the last snow-fall, and stacked to a height of about ten feet. Fortunately the men had had the foresight to mark the spot by an upright pole; without this landmark we should never have found

[1] Miner's sledges, formed of stout boards on runners.

it, for the wood was completely buried, and only a short length of the pole visible. Even Rojacher had not foreseen this, he being convinced that falls of snow were considerably less on the heights. So far he was right. The fall had been lighter above than below; but then below it had been almost unparalleled. To have formed an estimate of the quantity of snow that fell that winter on the Tauern, I should have needed a previous knowledge of the locality in summer; as, unfortunately, I had not that, I was obliged to content myself with Rojacher's computation at various points. The deepest level we could see, was on the lower plateau, some 8200 feet above the level of the sea, where the telephone wire stretches over a little glacier valley. Rojacher knew that this wire was carried 66 feet above ground in the deepest part of the valley. On passing by it, we found that the snow not only reached to the wire, but that the valley had become one even snow-field; thus proving a depth of 66 feet in that part. It is unnecessary to give further instances; no description could afford a true idea of the stupendous masses of snow. They must have been seen to be believed. Rojacher repeatedly said how glad he was that a Vienna Professor should have had the experience; and even went so far, in his good-natured raillery, as to wish that—without prejudice to my scientific researches—I might taste to the full the meaning of a severe winter on those heights.

His wish was granted, even beyond his desires, for I spent a February such as had never been known before, not only as regards snow and avalanches, but of destructive storm and variations of temperature. However, although I could have desired finer weather for my investigations, my stay on the Sonnblick was most enjoyable. The mountain sickness, from which I had hitherto always suffered severely, was very slight, and of not above three days' duration. My provisions were good, and lasted out excellently. In fact, I came to the conclusion, as far as health was concerned, that my winter expedition on the Sonnblick suited me infinitely better than a month in the Riviera would have done.

Shortly before I had started on my expedition there had been such accounts in the Vienna papers of the suffering from cold experienced by the man in charge on the Sonnblick, that I expressed some fears whether I should be able to stand the extreme cold in the house. Experience soon set those fears at rest. Our rooms were most comfortably warmed; the heating apparatus is perfect; indeed we had more than once to open a window to let out the hot air. It is quite a fallacy to suppose that one cannot keep warm on the Sonnblick.

These few remarks may serve to show those to whom their *café*, daily paper, *tarok*, or whist club are not matters of vital importance, that a winter sojourn on the Sonnblick has no great difficulties—when once they get there. As for occupation, there need be no lack; at any rate, so I found. On fine days, of which I counted but nine in the four weeks, I could barely give myself time to eat or sleep; they being entirely devoted to the specific objects of my investigations. On wet ones, I had enough to do examining and verifying the meteorological instruments belonging to the Observatory; and in initiating its solitary occupant, Peter Lechner, still further into their uses. The results of my observations have been since reported to the Imperial Academy of Sciences in Vienna.

It was no light work to get my apparatus suitably adjusted, all my observations having had to be made in the open air; and it is thanks to the skill and indefatigable energy of "the Hermit of the Sonnblick"[1] that I succeeded so well. Lechner is a most devoted servant of science, and carries out all his duties on that solitary peak in the most conscientious manner. He assisted me too in my observations on the radiation of the earth, and the sparkle of the stars. As these required to be made at night, the cold rendered it necessary to be well protected with fur-lined boots, fur travelling coat, fur gaiters and fur cap, well down over the ears; otherwise I could not have withstood those nights, standing and sitting, as we often required to do for hours, in a temperature of -4° F.

The simultaneity of my observations with those of Dr. Trabert were certified by the telephone, which acted admirably. The day after I arrived on the Sonnblick, the interruption between Kolm and Rauris had been repaired, and from that time there was only one day when connection was broken again—that time, unfortunately, between the Sonnblick and Berghaus, so that we were quite cut off. The next day, however, the point of breakage was found, and connection made again. It is no little difficulty to find out the point of breakage on such a height, and when the whole wire is buried under the snow.

Herr Rojacher has found a method, I do not know if in use elsewhere—anyway he found it out for himself. It is, of course, known to electricians that two near telephone stations can speak with each other if instead of one of the earth plates, connection is effected by means of any large mass of metal, as a stove, for instance, with which one of the telephones is connected. By analogy it ought also to be known (I do not know if it is) that in the case of three stations, as Kolm, Bodenhaus, and Rauris, should there be an interruption between Bodenhaus and Rauris, if that interruption has occurred near Rauris, Kolm and Bodenhaus would still be able to speak together, although, through the want of the ground conductor, there is no closed circuit. I have made that experiment myself. Now the above-mentioned larger mass of metal can be made to replace the wire from Bodenhaus to the point of interruption, supposing the wire to be long enough. It was on this last hypothesis that Rojacher founded his method—that of seeking the point where communication ceases up in the snow-fields. Taking a hand telephone with him, he starts from one of the stations between which communication is interrupted, and connects the hand telephone with the wire at one of the *Untersuchungstangen* (test poles) that are placed at intervals, and through which the wire passes, thus raised in triangular form out of the snow. As long as he can still speak with the station whence he has come he knows that the breakage has occurred farther on. When he can no longer speak he fixes a trumpet on to the telephone; if the answer, also spoken through a trumpet, be audible, the point of breakage is not far off. If the trumpet tone reaches his ear no longer, the spot is close, and a little examination enables connection to be re-established. Only by this method could connection be as quickly restored under difficulties so immense; and it is by this means that Rojacher is enabled to send out regular meteorological observations, with scarce a break, through an electrical aparatus perhaps the most perilously placed in the world.

During my stay on the Sonnblick I had opportunity to witness many rare atmospheric effects; and to become more closely acquainted with meteorological phenomena at that altitude. The second day I was there I saw a splendid sight. A white mist enveloped the whole base of the mountain up to within 500 feet of the summit; the shadow of the house on the Sonnblick being clearly projected on it. Suddenly the shadow was surrounded by a

[1] Alone for the most part throughout the year, cut off from all intercourse during the worst of the winter months, his occupation is to speak through the telephone three times daily, to record his readings on the maximum and minimum thermometers, on the sunshine recorder, the psychrometer, the hygrometer, and the hygrograph, on the anemometer, the barometer, and several other instruments; he hears, besides his own voice, generally that of one of his former comrades at the Miner's House where he used to work, inquiring, "Is all well on the Sonnblick?" And then the former silence is resumed.—Translator's note, from *Standard* of December 18, 1889.

triple rainbow of dazzling brightness. Had I not known that my eye was the centre of the exquisite sight, I must have judged the house, or rather its shadow, to be its central point. This I disproved by moving from east to west of the house, when the whole "glory" seemed displaced. I did not succeed in projecting my own shadow upon the mist, and in producing the effect myself; the "glory" remained attached to the shadow thrown by the house. I observed the same atmospheric effect several times afterwards while there, but never with such brilliancy. Another time I was struck on observing a magnificent ring round the sun, accompanied by other lesser rings. The sun was then in the east, about 14° above the horizon, and exactly over the peak of the Kleinen Sonnblick, at no great distance. The solar ring was $23\frac{1}{2}°$ radius, and of indescribably brilliant prismatic colours. At both extremities of the horizontal diameter was a lesser coloured sun of radiant brightness; but the strangest part of it was that I could see the lower portion of the vertical diameter of the solar ring, although it was more than 7° below the horizon. And now there appeared a lesser sun of dazzlingly white appearance, seeming as though rising behind the mountain peak; its dazzling whiteness rayed out high up into the heavens, forming, as it were, a column of light resting upon the Kleinen Sonnblick. On passing a horizontal line through this white secondary sun below the horizon, I found at a distance of $23\frac{1}{2}°$ to right and left of it, two coloured lesser suns, which, being also below the horizon, were projected on to the snow-fields of the Kleinen Sonnblick, and of the Goldberg-Spitze, forming a magical effect—indeed, the whole spectacle was one of entrancing beauty.

One lovely moonlight night, I was standing in front of the house, making observations with the scintillometer. After a time I was conscious of a series of rapid obscurations flitting over the field of my telescope. Looking up irritably, I perceived that small portions of the mist, which reached almost to the summit of the mountain, were being detached and borne swiftly over my head. My irritation, however, was quickly dispelled on looking at the moon through these icy veils of mist. Whenever a fleecy cloudlet passed between the moon and me, there was a gleam and lustre of rainbow hue with such intense brilliancy of the lunar surface that I had never seen the like before. I leave my readers to imagine the effect of this ever-changing moon, now of silver lustre, now iridescent with many-coloured rings, and they will understand that I quite forgot my interrupted observations in the absorbing sight.

The zodiacal light I saw there also, and more brilliantly than ever before. I cannot do better than recommend any one who is a lover of aërial effects to pass a winter on the Sonnblick. And perhaps the finest sight of all is the magnificent view—the grand panorama to be seen from such a height. The view from the Sonnblick, even on a fine summer's day, must be a sufficient reward for the toil of the ascent; on a fine day in winter it surpasses all description. The clearly marked horizon, on which there is no trace of mist or haze, the mountain ridges, even to the most remote, standing out in lines of perfect distinctness from the sky—the grandeur of the whole snow-clad scene is so overwhelming, that I could but express my surprise to Rojacher and his assistant, that no members of the Alpine Club had availed themselves of the hospitality of the house on the Sonnblick, to know and enjoy the delights of a fine winter's day on the Hochgebirge. Formerly the difficulty would have been that without shelter one could only have stayed a few minutes on the summit, and had the weather been unfavourable in those few minutes, the whole ascent would have been fruitless. But now that there is shelter on the summit, and a house so comfortably arranged, the whole difficulty is done away with. I have a strong conviction, moreover, that the ascent in winter is easier than in summer—given a normal winter with average snow-fall. It is far less fatigue to ascend steep places and cross glaciers on a moderate layer of new-fallen snow; one does not become so heated, and consequently breathing is not so difficult as in summer. And then, the infinitely finer view.

I am convinced that it can only be the inconvenience of leaving their business or professional callings at that busy season that has hitherto kept men back. So fascinated was I with the view, that I determined to advise all whose duties would permit them to pass a few winter days on the Sonnblick—the more surely that I can vouch for Herr Rojacher's hospitality removing all doubts on that score.

If phenomena of light most pleased the eye, other meteorological conditions gave me fuller scope for observation. In the first place, the height of the clouds. For the most part, unluckily, we were in them. Often we were above them, and had then the grand sight of the vast sea of cloud surging and swaying beneath us, now rising, now falling, called *Nebel boden* or *Boden nebel*. Several times, for days together, only those mountains whose peaks were higher than 8200 feet rose above the clouds; and we would be walking about in bright sunshine, while the valleys beneath were filled with cloud. At other times the northern valleys would be quite clear, and the southern ones full of cloud, or *vice versâ*. One evening we had the southern valleys a mass of cloud, the next morning, on looking out, they were perfectly clear, and the northern ones were thickly enveloped. It was as if the clouds had travelled over the Alps in the night from south to north.

With the exception of the cirri, I never saw clouds above us. These are easily traced to their source from the Sonnblick. They were more unwelcome to me even than the mist; they disturbed my observations to such an extent.

It is known that the cirri take their rise from the depression centres. Thus they were servieeable to me in determining the situation of the minimum pressure of the air. Nearly the whole of my stay on the Sonnblick depressions formed with curious persistency over the Tyrrhenian sea, passing over southwards. This was distinguishable to us by a heavy bank of cloud in the extreme south-west, whence the cirrus bands stretched out in our direction. With a change of depression to south-east, or east, the radiating point of the cirri shifted accordingly. We had nothing to fear from the southern depression; in fact, it in no way affected the weather on the Sonnblick. But if the cirri rose from the north-west, although from the extreme distance the heavy cloud bank was not visible to us, none the less were we certain within six to twelve hours that storm and mist would be the invariable consequences.

In the many violent storms I witnessed there, I directed my attention chiefly to two questions: Do the winds blow in gusts here on the summits of mountains, standing free as they do in the atmosphere? and What is the relation of the gusty winds to the "pumping" of the barometer? I had formerly been somewhat of opinion that on these free heights there was no sufficient cause for storms to blow in gusts; and in fact in storms from the south-west the gusts appeared to me to be considerably less than in Vienna, although fully perceptible. But with a gale from the north they far exceeded in violence anything on a lower level. I have no time to go more closely into this question, and will only briefly describe those of my observations which bear upon the "pumping" ("oscillations") of barometers during a storm. It is a subject that has been much under discussion of late; I will confine myself to my observations. I made use of four instruments—a mercurial barometer, a very fine Naudet's aneroid, a Richard's

barograph, and a Redier's barograph. My observations, made alternately with these four, came to the same result. If the wind appeared to have lulled for a short time, there would be a sudden fall in the barometer of often more than two millimetres. A violent gust would then follow on the fall in the barometer, its strength varying in proportion to the fall of the barometer. During the gust the barometer would rise nearly as much as it had previously fallen.

From these observations, carried on through whole days, and often far into the night, it seemed to me that the cause of the gusts must be that slight, quickly passing depressions were over us.

If these observations are correct, and I can hardly doubt them, the suction of the wind is of secondary importance in considering the causes of the "pumping" ("oscillations").

I cannot allow myself to enter into all the interesting meteorological subjects that there presented themselves, and my views upon them, without trespassing too largely on the space assigned to me in these pages. I would only refer briefly to what I observed of the marked electrical activity in the telephone. It may seem strange to speak of a strong electrical development in winter, and I must confess to have been surprised on many days to hear a loud crackling at the telephone, so loud that it was almost impossible to speak through it. Still more astonished was I to see electric sparks going off from the electric plate ("*Blitz Platte*"). Unfortunately I had not time to examine this increased electric activity in its relation to the weather; but I fancied that a fall of snow with a south wind had most influence upon it. I requested Lechner to make daily observations of the crackling in the telephone, at a given hour, and to register the four stages—weak = 1, moderate = 2, strong = 3, electric sparks = 4. I have heard from him that he has been recording his observations five times a day, and, he thinks, with good result. A prolonged series of observations will easily determine its cause.

From these hastily collected extracts of my experiments and investigations on the Sonnblick, all must be satisfied of what great importance to science is the Observatory on its summit, and not less to Alpinists. It matters little how highly I prize it; my aim is to make its value known in wider circles.

But it behoves us, scientific men and tourists, not merely to wax enthusiastic over the Sonnblick Observatory, but to take measures to ensure its permanency. I am aware that the Alpine Club has already done its part,[1] and do not doubt but that in future it will shrink from no sacrifice to uphold and support this, its foster child, which, in conjunction with the Meteorological Society, it has brought into life. But I am inclined to think that there are nearer supporters of this our most important mountain Observatory, on whom there exists a prior claim. I am under the impression that certain influential members of the Alpine Club had been called upon to form a special Sonnblick Verein, part scientific, part tourist, who by a small yearly subscription should ensure the keeping up of this invaluable station.

My descent from the Sonnblick began on March 4, amid a storm of north wind, mist, and temperature at −22° F. We rode down on miners' sledges (*Knappenrossen*), but even then had great difficulty in forcing a passage, snow having fallen knee-deep overnight. We often had to call a halt, and wade through the snow, thereby causing great delay; it took us two hours to reach Kolm, a distance usually accomplished in one.

On March 5 I reached Rauris; leaving on the 6th with Dr. Trabert for Lend. Even on these two last days, the weather followed us with unremitting severity. The way from Kolm to Rauris had been made under a heavy snowfall; and in the night of the 5th–6th there were such deep snow-drifts, that we were two hours making our way from Rauris to Landsteg.

On March 7 we reached Vienna.

[1] The corporation of the Alpine Club has just signed an agreement with Herr Rojacher, by which it guarantees him a grant of 5000 fl. towards the enlargement of the Sonnblick Observatory.

BEDFORD COLLEGE.

SOME time ago we drew attention to the fact that Bedford College, which has done so much for the education of women, was in need of funds. The new laboratories are now in use, but they are not yet paid for, and the stock of apparatus is not all that could be desired. Our readers will remember that Mr. Henry Tate had promised a donation of £1000 provided the Council could raise a like amount from other sources. We believe that the College authorities are nearly in a position to claim his generous gift; but though this will free the building itself from debt, at least £500 more is wanted to pay for equipment on a very moderate scale.

The last twelve months have been, in matters educational, a ladies' year; but the true meaning of the successes which have been won at Cambridge and elsewhere will be missed, if they are regarded only as a nine days' wonder, or as proving *ambulando* that the higher levels of undergraduate attainment can be reached by girls. The lesson which has been so strikingly enforced is that no branch of learning is the exclusive property of either sex, and that girls are wronged if we do not afford them the same opportunities for acquiring knowledge which are provided for their brothers.

The founders of Bedford College acted on this principle when it was not so widely accepted and not so self-evident as in 1890, and we can only urge on the friends of the education of women not to forget, in the hour of their triumph, the toilers who have paved the way to their success.

In an unpretending building in an uninteresting London street an effort has for long been made to supply education of the highest class for London girls. Faith in the future and effort in the present have never been wanting, even when the story of the past seemed most discouraging. The College is now undeniably a success, but it is still sadly hampered by want of means. The adequate equipment of its laboratories is surely an object for which an appeal will not be made in vain to those who believe that the benefits which science can confer will never be fully attained till a knowledge of its main principles and methods forms part of the training of all educated men and all educated women alike.

NOTES.

WE regret to have to record the death of Mr. William Kitchen Parker, F.R.S., formerly Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons. Next week we hope to give some account of his services to science.

A REUTER'S telegram from New York states that the remains of the Swedish inventor, John Ericsson, will be conveyed to Sweden by one of the two new American war-vessels, *Baltimore* and *Philadelphia*.

THE Dutch Academy of Sciences in Haarlem has offered a gold medal of the value of 150 gulden for the best work in each of the following subjects:—(1) Researches on the part played by bacteria in the decomposition and formation of nitrogenous compounds in various kinds of soil; (2) Microscopic investigation of the mode in which different parts of plants can unite with one another, and especially the phenomena which accompany healing after the operation of grafting. The papers must be written in German, Dutch, or Latin (not in the handwriting of the author), and must be forwarded to Dr. J. Bosseka, Haarlem, by January 1, 1891.

THE list of Civil List pensions granted during the year ended June 20, 1890, includes the name of Dr. William Huggins, to whom has been awarded a pension of £150. As we have already noted, a pension of £50 has been granted to Mrs. Jane Eleanor Wood, widow of the Rev. J. G. Wood, and a pension of £20 each to the four unmarried daughters of the late Rev. M. J. Berkeley, F.R.S.

MR. DAVID S. CAPPER, Assoc.M.Inst.C.E., has been elected to the Professorship of Mechanical Engineering at King's College, London.

THE annual meeting of the Botanical Society of Italy will take place in Verona during the month of September.

FRENCH papers announce the death of M. Paul Loye, at the early age of 29. He was the author of a memoir on the physiology of death by decapitation, and had published many short notes on physiological questions. He had for some time been engaged in an elaborate study of the excretory functions of birds, concerning which he had collected many facts. M. Loye was assistant to Prof. Brouardel, and *Maître de Conférences* in the Faculty of Sciences of Paris, and had been Paul Bert's last assistant.

THE death of M. Alphonse Favre, at the age of 77, is announced. He was formerly Professor of Geology at Geneva, and was recognized as an authority on the geology of the Alps.

THE half-yearly general meeting of the Scottish Meteorological Society was held in Edinburgh on Monday, July 14. Lord McLaren presided. The following was the programme of business:—(1) Report from the Council of the Society; (2) address by the Chairman on the high and low level observatories of Ben Nevis; (3) on the meteorological conditions of desert regions, with special reference to the Sahara, by Dr. John Murray. In their report the Council express sincere regret at the death of Dr. James Stark, who long held the office of Superintendent of the Statistical Department in the Register House, Edinburgh, and gave very effective aid in founding the Society. The self-recording instruments, furnished by the Meteorological Council for the low level observatory at Fort William, arrived at the end of June, and it is contemplated that the regular work of recording the continuous observations will begin in August. The observations which have been carried on in Fort William by Mr. Livingstone in connection with those made at the top of Ben Nevis will be continued at least till the New Year, in order that a comparison may be made with them and the similar eye-observations made by Mr. Omond at the Observatory adjoining. It is arranged that Dr. Buchan's time will be wholly given, during next year, to the examination and discussion of the observations of the Ben Nevis observatories. In connection with this difficult and laborious undertaking, Mr. Omond will receive from the Meteorological Council three copies of their daily and weekly weather maps, on which he will enter certain of the meteorological data from the high and low level observatories, together with occasional remarks that may from time to time strike him as bearing more particularly on forecasting weather. The weather maps give two daily representations, with remarks, of the weather of Europe at 8 a.m. and 6 p.m. Thereafter, one of the three sets of maps will be sent to the Society's Office, the second to the Meteorological Office of London, and the third will be retained by Mr. Omond.

THE Council also refer to the observations of Mr. Rankin on the number of dust particles in the atmosphere, carried on with the two sets of apparatus invented by Mr. Aitken. Though it would be premature to offer a statement of positive results, the Council think that some interesting conclusions appear to be indicated by the observations. The maximum number of dust particles in a cubic centimetre hitherto observed is 12,862, on March 31, and the minimum 50, on June 15. On March 31, at 4.30 p.m., the summit was clear, and the number of particles was 2785, but shortly thereafter a thickness was seen approaching from south-west, which by 6 p.m. reached the Observatory, and the number of particles rose to 12,862. On June 15 many observations were made during the day, when the number of particles fell from 937 at midnight to 50 at 10.30 and 11.42 a.m. The observations point to a daily maximum during the afternoon minimum barometer, and a minimum during the morning minimum barometer—these being probably intimately connected with the diurnal ascending and descending currents of the atmosphere. Interesting intimate relations are also indicated between the numbers of dust particles and the cyclones and anticyclones over North-Western Europe at the time. The observations also indicate that the dust particles may vary enormously during the presence of mist or fog, without being accompanied by any difference in the apparent density of the fog. The Council consider that the inquiry is an extremely hopeful one; and in view of the relations with cyclones and anticyclones, its bearings as regards the forecasts of the weather will be very specially investigated.

FOR several years past it has been the practice of the Indian Meteorological Department to issue in the month of June a forecast of the prospects of the monsoon rains, based partly on the reported extent and thickness of the Himalayan snows, partly on the distribution of the atmospheric pressure, the small variations of which are found by experience to be remarkably persistent in India, and to serve as an indication of the probable strength of the monsoon, and alternatively of the prevalence of dry land winds. The forecast for the forthcoming season announces that owing to the very slight snowfall of Afghanistan, Baluchistan, and almost the whole of the Himalayan region, the conditions are eminently favourable for a good strong monsoon. The only unfavourable indication is that the past winter has been very severe in Yarkand, and perhaps in other distant parts of Central Asia. The pressure is unusually low this year in Bengal, and above the average in Central India and the northern half of Bombay, and the local pressure conditions considerably resemble those of 1876. It is therefore considered probable that while the eastern half of the Ganges valley, Assam, and Burma will receive early and abundant rain, the rains may be late and scanty over a considerable area of North-Western India.

THE Rev. E. Colin, S.J., Director of the newly-established Royal Observatory of Madagascar, at Tananarivo, has published the monthly results of meteorological observations at that place during 1889. As observations for Madagascar are scanty, we are glad to learn that observations are now taken at four stations in various parts of the island, and that others will shortly be established. The maximum temperature at Tananarivo, 87°·4, occurred on November 14, and the minimum, 41°·0, on July 31. Rain fell on 89 days; by far the greatest quantity falls between November and March. None fell in May 1889. The prevalent wind direction is between south-east and north-east. The Report contains summaries for the three other stations referred to, during 1889, and for Tananarivo from 1872–88. Some of the latter have never been published before, and form an important addition to our knowledge, but, having been made by various persons, may not be so trustworthy as those made at the Observatory.

THE meeting lately held at the Mansion House, under the presidency of the Lord Mayor, for the furtherance of the International Congress of Hygiene, which will assemble in London in 1891, was attended by many influential medical men and students of sanitary science. Sir Douglas Galton explained the

object and organization of the Congress, to which delegates had been already appointed by all the leading Societies of Great Britain and of the Continent. He mentioned that in any case the cost of the Congress would be considerable—probably not less than £5000—and that an appeal would be made to raise the required funds and to make the gathering worthy of Great Britain. Among the subsequent speakers were Lord Wantage, Prof. Humphry, Mr. Ernest Hart, Sir Spencer Wells, Sir Henry Thompson, and Dr. Thorne Thorne. The organizing committee is now taking steps to raise a sum of at least £5000, and no doubt its appeal will receive a liberal response from some of the great Societies and Corporations as well as from private individuals.

IN order to make the Parkes Museum, which is supported by the Sanitary Institute, available to all classes for the purpose of obtaining information on matters relating to hygiene and sanitary appliances, the Council have resolved to throw the Museum open free at all times except when meetings are being held.

THE Medical Academy for Women at St. Petersburg is to be reopened. At its sitting of June 9, the municipality of that city voted a yearly grant of £3000 for the support of the Academy, and decided to give it the use of a house belonging to the municipality, and to open the city hospitals to the students. Private subscriptions fully guarantee the further existence of the Academy. It is hoped, therefore, that the Government will not oppose the reopening of the institution, which has already given to Russia no fewer than 698 lady doctors. The decision of the municipality was based upon a report by Dr. Archangelsky, who speaks very favourably of the work done by the eleven lady doctors who are in the employment of the municipality for the inspection of city schools and the poorer districts of St. Petersburg.

THE joint meeting of the Essex Field Club and the Gilbert Club, held at Colchester on July 5, proved a great success in spite of the continuous downpour of rain which lasted throughout the day. Over fifty members of the two Societies assembled at 11.30 in the Castle Museum, where the Hon. Curator, the Rev. C. L. Acland, and Mr. H. Laver pointed out the objects of interest to the visitors. The party then visited Holy Trinity Church, wherein lie the remains of Gilbert, and which contains a mural tablet erected to his memory by his brothers. After inspecting the house in which Gilbert was born, and other places of local interest, the visitors adjourned to luncheon at the Red Lion Hotel, the chair being taken by Lord Rayleigh, who was supported by the Mayor of Colchester, the President of the Essex Field Club, and many well-known men of science and local residents. Among those present were Profs. D. E. Hughes, F.R.S., G. D. Liveing, F.R.S., J. Perry, F.R.S., R. Meldola, F.R S., and S. P. Thompson, Messrs. G. Kapp, J. Paxman, Conrad Cooke, and F. H. Varley. The Chairman made a short speech, in the course of which he alluded to the importance of Gilbert's work, and pointed out that, although it is to Gilbert that we are indebted for the theory that the earth is a great magnet, we are not much in advance of this position at the present time, as nobody has yet explained the origin of terrestrial magnetism. The Mayor of Colchester then took the opportunity of welcoming the two Societies to the town on the part of the inhabitants. After luncheon some of the party drove to the Vale of Dedham, rendered famous in art by the paintings of Constable, who was born at Flatford Mill in this district. In the evening a reception was given at the Town Hall by the Mayor and Mayoress. Many electrical novelties were exhibited, and an incandescent light installation was supplied from premises on the other side of the road, where plant had been erected by Messrs. Christy, Son, and Norris, of Chelmsford. An interesting piece of apparatus, constructed on the pattern of Crookes's radiometer, but working in air instead of in a vacuum, was exhibited by its inventor, Mr. C. E. Benham, who attributed its rotation to the action of convection currents. There were also on view exhibits by Messrs. Crompton, of Chelmsford, lathes and sewing-machines worked by an electric motor, and other objects of interest. Prof. S. P. Thompson delivered an interesting lecture on the early magnetic experiments of Gilbert, illustrating his subject by experiments shown with the projecting lantern. A vote of thanks was proposed by the Mayor, and seconded by Mr. J. Paxman, who remarked that he should like to see Gilbert honoured not only by a statue in his native town but also in a more useful way, such as by the foundation of a Gilbert Scholarship in connection with one of the Universities. A vote of thanks was proposed by Prof. Meldola on behalf of both the Clubs to the Mayor and Mayoress, to Dr. Laver, and Mr. J. C. Shenstone, all of whom had by their exertions contributed to the success of the day's proceedings.

IN a paper on ornithophilous flowers,[1] contributed to the *Annals of Botany*, Mr. G. F. Scott-Elliot records the very interesting observation that the Cinnyridæ or sun-birds, which play an important part in the fertilization of flowers in South Africa, have the same habit as the Apidæ in other countries—that is, of not "mixing their honey," but, on the same journey, confining their visits pretty much to the same species of flower. The species of sun-birds which are especially good fertilizers in South Africa are *Nectarinia chalybea*, *N. bicollaris*, and *Promerops caper*. In accordance with the view of Darwin, but opposed to that of Wallace, Mr. Scott-Elliot believes that the identity of colour (an unusual shade of red) in the majority of ornithophilous flowers and on the breasts of species of *Cinnyris* is an important element in pollination by birds.

A NEW little magazine, which ought to be of service to those who devote attention to questions relating to manual training, has just been started. It is called *Sloyd or Hand-Craft*. Its primary object is to acquaint the members of the Home Sloyd Union, and all those who are interested in the development of a distinctively English form of manual instruction, with the progress of the Sloyd system as practised in this country. But it is by no means intended to exclude what is being done in other directions for the purpose of making education more practical by means of hand and eye training, more especially as regards children from eleven to fifteen years of age.

BY an Order in Council, dated June 30, 1890, which has been issued as a Parliamentary paper, it is prescribed that the following monuments in Ireland shall be deemed to be ancient monuments to which the Ancient Monuments Protection Act, 1882, applies:—

Monument.	County.	Parish.
(1) Cahernamactierech and Bee Hive Structures on the Promontory of Dingle ...	Kerry ...	Drumquim and Ballinroher.
(2) Round Tower, Lusk	Dublin ...	Swords.
(3) Round Tower, Kells	Meath ...	Kells.
(4) Stone Cashel with Galleries ...	Sligo ...	Cashelmore.
(5) Stone Circles and Pillar Stones ...	Fermanagh	Enniskillen.
(6) Round Tower of Tulloheran ...	Kilkenny ...	Tulloheran.
(7) Round Tower of Rathmichael, Church and Stone Cross	Dublin ...	Rathmichael.

A CALIFORNIAN salmon (*Oncorhynchus quinnat*, Günther) has recently been caught in the Mediterranean, near Banyuls. Probably it found its way thither from the River Aude, into which many young fish of this species have been introduced, in the hope that they may be acclimatized in France.

A PORTRAIT of the African explorer Captain Gaetano Casati forms the frontispiece of the May number of the *Bulletin of the Italian Geographical Society*. Casati reached Cairo early in May, and letters in the *Bulletin* deal with his journey to the coast with Emin and Stanley. An itinerary of his nine years of travel shows that he left Suakin for Berber and Khartoum in January 1880. In July of the same year he started in a sailing-boat down the White Nile to Mishra-el-Rek, and thence on foot to Wau, where he met with Gessi at the end of September. He then threaded his way southwards among the feeders of the Bahr-el-Ghazal to the Congo basin, and for some time made Tangasi, on the Welle or Makua branch, a centre for exploration. Close by, at Mboro, in June 1881, he met with Dr. Junker. Finally, he made his way to Ladò, on the main stream of the White Nile; and there, at the end of March 1883, he met Emin Pasha for the first time. Thence he walked up the left bank to Wadelai, and continued the voyage up the Albert Nyanza by steam-boat. It was not until April 28, 1888, that the meeting between Emin Bey, Casati, and Stanley took place on the plateau above Kavalli to the south-west of the lake. The journey down the Semliki valley, the exploration of Lake Albert-Edward, and the return to Zanzibar, are recent history. The remaining papers of the number deal mainly with South America. The most interesting of these is that of Count Orsi di Broglia di Mombello on the sculpture of the primitive inhabitants of the Upper Orinoco. Many carvings on the stones of tombs have been discovered among the villages of this district; the sculpture is rough and fantastic, but evidently aims at reproducing certain natural objects. Thus, at the Grotto of Caicara, near the right bank of the Orinoco, many rocks carved in the primitive manner of the slate sketches of school-days, evidently exhibit an attempt to figure a tiger that is very common in this district. In neighbouring caves were found mummies closely resembling Egyptian ones; this the author regards as further evidence of the common origin of the two races, previously suggested by the striking similarity in shape of the skulls of the South American Indians and those found in the tombs of Egypt.

A SWEDISH Expedition to Cameroon is being arranged by the Academy of Sciences in Stockholm. The object of those who are to take part in it will be to study the fauna of the Western Cameroon Mountains, and to make scientific collections for the Academy. Herr Yngve Sjöstedt is to be in command of the Expedition, which is expected to be absent for about fifteen months.

WE have received the following details of the researches in which Prof. Bastian is engaged on behalf of the Anthropological Museum of Berlin. In December last he forwarded to Berlin the results of excavations made at Tashkent; the terra-cotta vases and utensils all bearing strong evidence of Greek influence in their workmanship. During January he spent some time in Zanzibar and Mauritius, and at the latter place he was enabled to make a collection of Mascarene curiosities. From Tinivelly, in Southern India, he forwarded some bronze idols in February. March was spent at Malabar, April at Mysore, the beginning of May in Beloochistan, and the latter end of the month in Peshawar. Prof. Bastian has sent interesting ethnographical collections from all these districts.

SIR ARTHUR GORDON lately received from the Pandits and Buddhists of Ceylon addresses in which, among other things, he was praised for the encouragement he had given, during his term of office, to science and learning. "Your Excellency," said the Buddhists, "with the laudable wish of preserving the philosophy and sciences contained in that most noble language the Pali, which is regarded by Eastern nations as the original language and the depository of the teachings of the blessed Buddha, as well as those found in the Sanskrit—the language of the gods—has caused many works, such as the 'Mahawansa' and others, to be translated into English, and given an incentive to the publication of Pali and Sanskrit works by allowing them to be printed at the Government Press." The Pandits took occasion to express a hope that Sir Arthur might still continue to exercise his influence on their behalf:—"It is with great pleasure that, whilst we gratefully express our thanks for the benefits already received at your Excellency's hands, we at the same time seize this opportunity of begging your Excellency not to relax your efforts on behalf of our literature and archæology, but to impress upon your Excellency's successor, as well as on Her Majesty's Government, the need not only to continue, but to increase, the exertions that are now being made to preserve the recollection of our glorious past, as an incentive to our countrymen of the present day to noble aims and heroic efforts in the future."

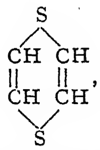
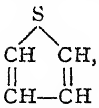
SOME discussion has been going on in Ceylon of late over the question of the language spoken by the Veddahs, the aborigines of that country. The subject (says the *Colonies and India*) would seem to be one well worthy the attention of philologists, and the brothers Sarasin, who have been pursuing their anthropological researches in Ceylon lately, express the opinion that if a philologist were to take the matter up great service would be rendered to all those engaged in the work of scientific research in the island. Tennant says of the Veddahs, "Their language, which is limited to a very few words, is a dialect of Singhalese without any admixture from the Sanskrit or Pali—a circumstance indicative of their repugnance to intercourse with strangers." Prof. Schmidt, of the Leipzig University, who visited the Veddahs last year, says, "Their language is similar in construction to the Dravidian languages—that is, similar in grammatical construction; but they have adopted a great number of Singhalese words," which enabled him to hold converse with them by means of a Singhalese interpreter. The Drs. Sarasin also managed to make themselves understood by means of Singhalese.

IN the last issue of the *Records* of the Geological Survey of India, Mr. Griesbach's mission to Afghanistan is thus referred to:—"Mr. Griesbach returned to India last July. His work with the Ameer was, as is now so very largely the case in the Survey, geologico-industrial, though this was greatly retarded by unforeseen political complications in the State. During his journey in 1888, up the Logar Valley to the Khurd Kabul Valley, Upper Wardak, Cherkh, Kharwar, Zanakhan, Ghazni, &c., the most interesting geological work was the recognition of at least three horizons: the Rhætic with *Lithodendron* (in Kharwar), the Upper Jurassic (or possibly Neocomian) plan-beds near the Shutargardan; and, finally, well-developed nummulitics (in Kharwar and Shilghar). He examined the copper lodes of the Logar and Khurd Kabul areas, the magnesite of the Logar and the entrance to the Taugi Wardak, the graphite of Cherkh, the iron and lead ores of Kharwar, and the argentiferous lead ore of Zanakhan near Ghazni. It turns out, also, that the entire Upper Surkh-ab Valley from near Doab-i-Mekzari to near Dahana Iskar is practically one big coal-field with numerous thick seams of good coal of Triassic and Rhætic age."

THE official Report of the survey work done towards the close of the Chin-Lushai Expedition shows, according to the Allahabad *Pioneer*, that the Boinu River, which flows only six miles

west of Haka, is undoubtedly the main stream of the Koladyne, which was so familiar in connection with General Tregear's movements. Captain Bythell, R.E., who was at first on the Chittagong side, accompanied General Symons on his tour southwards from Haka, and traced the stream to within twelve miles of where he had last seen it from the Blue Mountain side. It is satisfactory to have this confirmation of the statements sent by correspondents with the Field Force, particularly as the upper course of the Koladyne was unknown to our geographers. The total area of topography, by the way, covered by the operations of Captain Bythell's party, is put down at about five thousand square miles, while the surveyors on the Burmah side must also be credited with work on a similar scale. The new maps of the Chin-Lushai hills, when they come to be published, will no longer show those great blank spaces which have hitherto been so noticeable in the old issues.

IN Grinnell Land, at sea-level (81° 44′ N. lat.), the mean day temperature is above freezing-point from about June 13 to August 23, *i.e.* 72 days. It has been recently pointed out by Dr. Hann, that on the top of the Sonnblick, at a height of about 10,000 feet, and in 47° N. lat., the temperature returns above freezing-point about the same time (viz. June 8); but it is not till the end of September or beginning of October that it goes below that point again. On the other hand, the mean summer temperature on the Sonnblick is considerably lower than in Grinnell Land at sea-level.

BIOPHENE, , an entirely new compound intermediate between the fatty and aromatic series, and somewhat resembling thiophene in properties, has been prepared by Dr. Louis E. Levi, of the Massachusetts Institute of Technology, Boston (*Technology Quarterly*, May 1890). Thiophene, , was discovered by Prof. Victor Meyer in 1883; the discovery attracted considerable attention at the time, and has since led to the preparation of a whole series of derivatives analogous in many cases to those of benzene. Dr. Levi worked for some time in the laboratory of Prof. Meyer, and has subsequently followed up ideas then discussed, which have now resulted in the preparation of biophene. Just as thiophene is obtained by the action of phosphorus trisulphide upon succinic acid, so biophene is found to be produced by the action of trisulphide of phosphorus upon thio-diglycollic acid, $COOH—CH_2—S—CH_2—COOH$. A mixture of five grams of thio-diglycollic acid with ten grams of phosphorus trisulphide is heated, together with 15-20 c.c. of ether, in a sealed tube for two hours at a temperature of 170° C. After cooling, the end is opened at the blowpipe, when a great rush of accumulated sulphuretted hydrogen gas occurs. The contents of the tube are separated in the usual manner by means of a tap funnel, and washed with caustic potash solution. After withdrawing the alkali, the remaining oil is dissolved in ether and dried by means of fused calcium chloride. The ether is finally evaporated, and the residual oil fractionally distilled. As the result of this latter process, a liquid is eventually obtained boiling between 165° and 170°, which on analysis yields numbers agreeing with the formula of biophene, $C_4H_4S_2$. When biophene is mixed with sulphuric acid and a crystal of isatine added, a beautiful violet coloration is produced, a reaction which appears to be analogous to that of thiophene, which produces with sulphuric acid and isatine a dark blue coloration. Biophene also reacts with acid chlorides in presence of aluminium chloride like thiophene, thus with acetyl chloride aceto-bienone or bienyl acetyl ketone, $C_4H_3S_2 . CO . CH_3$, is produced, hydrochloric acid being eliminated. This ketone is a thick, heavy liquid which may be distilled in steam and possesses an aromatic odour somewhat resembling that of aceto-thienone. Heated alone aceto-bienone boils, but with decomposition, at 300°. Sunlight rapidly turns it dark brown. Aceto-bienone also reacts with phenylhydrazine with formation of a compound of the composition $C_4H_3S_2 . C\begin{matrix} \diagup CH_3 \\ \diagdown N_2HC_6H_5 \end{matrix}$. The similarity of biophene in all the above reactions to thiophene and benzene is very striking, the replacement of two of the CH groups of benzene by sulphur not being accompanied by any very great change in chemical behaviour. The formation of biophene from thio-diglycollic acid, also affords another instance of the passage from the fatty series to bodies of aromatic properties, and biophene itself will stand as an additional link between the two series.

THE additions to the Zoological Society's Gardens during the past week include a Great Anteater (*Myrmecophaga jubata* ♀) from British Guiana, presented by the Directors of the Botanical Gardens, Demerara; an Egyptian Gazelle (*Gazella dorcas*) from Suakim, presented by Commander W. Crofton, R.N.; a Cape Ratel (*Mellivora capensis* ♀) from Suakim, presented by Captain J. F. M. Prinsep; a Jackal Buzzard (*Buteo jacal*), a —— Hawk Eagle (*Nisaetus spilogaster*) from Cape Colony, presented by Mr. W. H. Wormald; a Guillemot (*Lomvia troile*), British, presented by Mr. T. H. Nelson; a Greater Spotted Woodpecker (*Dendrocopus major*), British, presented by Mr. W. H. B. Pain; an Australian Crow (*Corvus australis*) from Australia, deposited; two Chinchillas (*Chinchilla lanigera*) from Chili, an Indian Chevrotain (*Tragulus meminna* ♂) from Ceylon, an Elate Hornbill (*Ceratogymna elata*), a White-necked Crow (*Corvus scapulatus*) from West Africa, a Large Grieved Tortoise (*Podocnemis expansa*) from the Amazon River, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on July 17 = 17h. 42m. 41s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4361	—	—	17 57 9	−24 21
(2) G.C. 4415	—	—	18 22 44	+74 31
(3) 74 Herculis	5·5	Yellowish-red.	17 17 15	+46 21
(4) σ Ophiuchi	4	Yellow.	17 21 6	+ 4 14
(5) α Ophiuchi	2	White.	17 29 48	+12 38
(6) D.M. + 36° 3168 ...	8	Red.	18 28 31	+36 55
(7) R Lyræ	Var.	Reddish-yellow.	18 51 59	+43 48

Remarks.

(1) The spectrum of this remarkable nebula has not yet been completely examined. In 1868, Captain Herschel observed two lines in the spectrum, and, in addition, a decided continuous spectrum from the brightest point, which is "*not* stellar." These lines are stated to be ill-defined; and now that it is asserted by some observers that the nebula lines are always sharp, they should be re-examined with special reference to this point. Seeing that the brightest point is not a star, it will be well also to look for maxima of brightness in the con-

tinuous spectrum, the usual flame comparisons being employed if necessary. In the General Catalogue the following description is added:—"A very remarkable object; very bright; exceedingly large; extremely irregular figure; with large cluster." Webb refers to it as "a splendid galaxy object, visible to the naked eye."

(2) This nebula was discovered by Tuttle in 1859, and, according to D'Arrest's observations in 1863, it would appear to be variable. It is oval in shape, 2′ long and 80″ broad, and is said to be "pretty bright." No attempt has been made, as far as I know, to determine its spectrum, to say nothing of any variations of spectrum which may accompany the supposed changes in brilliancy.

(3) This star is one of Group II. at a very late stage. Dunér states that "the bands 2, 3, 7, 8 are visible, but they are difficult to recognize as bands, because of their little width. The spectrum is almost of the same type as that of α Tauri." A special study should be made of the lines which accompany the bands, with special reference to how they differ either in position or intensity from the darkest lines in the solar spectrum.

(4 and 5) According to Vogel, these stars have very well developed spectra of the solar type and of Group IV. respectively.

(6) The spectrum of this star is a well-marked one of Group VI., the principal bands being very wide and dark. There is possibly also a trace of band 4 (λ 589).

(7) This variable will reach a maximum about July 24. Its spectrum is of the Group II. type, and is stated by Dunér to be one of the finest in the heavens. The range of variation is small—4·3–4·6—in a period which is not yet completely determined (46? days, according to Gore). Observations similar to those suggested for other variables of the same type should be made.

A. Fowler.

Photographs and Drawings of the Sun.—The Memoirs of the Royal Astronomical Society, vol. xlix. Part 2, 1887–89, have just been issued, and contain, with other papers, one presented by the late Father Perry in June 1889 on the above subject.

The areas of spots derived from the solar photographs of 1887, and published in the "Greenwich Observations," have been compared with similar values computed from the measures of the drawings made at Stonyhurst College Observatory. The area computed from the photographs, however, shows a decided general excess over those obtained from the drawings. An idea of the difference may be obtained from the values of the mean daily spotted area, that for 1887 taken from the photographs being 179, while the drawings give 171.

On 29 days penumbra are found in the drawings and not in the photographs, whilst such records occur on the photographs alone only 16 times; hence the greater area obtained from the photographs cannot be explained by a failure in the drawings to record faint spots and penumbral markings.

An attempt was made to compare the faculæ recorded on the drawings and on the photographs, but unsuccessfully, owing to the enormous excess obtained from the former over that computed from the latter. To eliminate this difference Father Perry suggested that the conditions necessary to obtain good photographs of faculæ may differ from that which is best for spots, and that, therefore, a twofold series of photographs may be necessary, one for spots and the other for faculæ. Two plates, showing sun-spot drawings in 1887, from the Stonyhurst series, accompany the memoir.

Observations of the Zodiacal Light.—Prof. Arthur Searle, in *Astron. Nachr.*, No. 2976, contributes a note on zodiacal light observations made at Harvard College Observatory during the last fifty years. With respect to the permanence of the ordinary western zodiacal light, the observations support the results obtained by previous observers, viz. that it must be considered as a very permanent phenomenon, and one subject only to slight variations in its degree of visibility, apart from atmospheric causes. Another principal subject of investigation was the normal distribution of light in the zodiac and its vicinity, and it is noted that the zodiacal bands, apparently forming a prolongation of the ordinary zodiacal light, were never seen at Harvard College. A number of permanent bands or belts of faint light, however, not confined to the zodiac, although certain portions of them follow the course of the ecliptic, are described in the records. A comparatively large number of observations of the phenomena of a feeble maximum of light in opposition to the sun, commonly known as *Gegenschein*, have been obtained. Prof. Searle thinks that the photometric observations of Müller and Parkhurst, which show that as an average asteroid approaches opposition its brightness increases by about 0·03 of a magnitude for every degree by which its phase is increased, may afford an explanation of this slight maximum of light in opposition to the sun, the light being reflected in this case from the meteoritic matter dispersed through the solar system. Indeed, if the amount of light received from a meteoritic particle be supposed to increase even proportionally to its phase, a maximum appears at opposition, while the law of increase in light assumed for the asteroids, was approximately proportional to the fourth power of the phase.

Ring Nebula in Lyra.—The current number of *Comptes rendus* (July 7) contains a note by M. G. Rayet on a photograph of this nebula obtained at Bordeaux Observatory with an exposure of three hours. The photograph shows all the stars observed near the ring by Lord Rosse in 1844; the star with the signification 3, however, is double, whereas that astronomer, and later Prof. Hall, mapped it as triple. There is also a very definite indication of a nebulous star of the 14th or 15th magnitude, almost in the centre of the ring. Although this star has been observed by many astronomers (*e.g.* Hahn, Secchi, Lassell, Schultz, and Holden) and has been photographed by Gothard, other astronomers (viz. Herschel, D'Arrest, Lord Rosse, Hall, and Vogel) have observed the nebula when the star was not visible, and it does not appear on the photographs taken by the Brothers Henry previous to 1886. M. Rayet therefore concludes that the star is variable, and hopes to make such observations and obtain such photographs as will enable him to demonstrate the fact. Stars in or near nebulæ and clusters seem from recent investigations to be more subject to variability than those not so situated.

Photographs of Stellar Spectra.—In the same number of *Comptes rendus* a note occurs by Admiral Mouchez, on some photographs of stellar spectra taken by the Brothers Henry at Paris Observatory, and presented by him to the Academy. Some of the photographs were obtained by means of a prism having an angle of 45° placed in front of the object-glass of the photographic equatorial, others by means of a prism having an angle of 22°; and Admiral Mouchez remarked that, although the results represented the first attempts in this direction, they compared very favourably with those obtained in America, where work of the same nature has been carried on for some time. It is noted that the Brothers Henry attribute the fuzziness of the lines in the spectra of stars like Altair to a high velocity of rotation and a great amount of agitation at the surface. The photographs are the beginning of an investigation into the chemical composition of stars and motion in line of sight, recently begun at this Observatory.

ON THE SUPERFICIAL VISCOSITY OF WATER.[1]

THE idea that liquids are endowed with a viscosity peculiar to the surface is to be found in the writings of Descartes and Rumford; but it is to Plateau that its general acceptance is due. His observations related to the behaviour of a compass needle, turning freely upon a point, and mounted in the centre of a cylindrical glass vessel of diameter not much more than sufficient to allow freedom of movement. By means of an external magnet the needle was deflected 90° from the magnetic meridian. When all had come to rest the magnet was suddenly removed, and the time occupied by the needle in recovering its position of equilibrium, or rather in traversing an arc of 85°, was noted. The circumstances were varied in two ways: first, by a change of liquid, *e.g.*, from water to alcohol; and, secondly, by an alteration in the level of the liquid relatively to the needle. With each liquid observations were made, both when the needle rested on the surface, so as to be wetted only on the under side, and also when wholly immersed to a moderate depth. A comparison of the times required in the two cases revealed a remarkable dependence upon the nature of the liquid. With water, and most aqueous solutions, the time required upon the surface was about *double* of that in the interior; whereas, with

[1] Paper read before the Royal Society, by Lord Rayleigh, Sec.R.S., on June 5, 1890.

the liquids of Plateau's second category, alcohol, ether, oil of turpentine, &c., the time on the surface was about *half* of the time in the interior. Of liquids in the third category (from which bubbles may be blown), a solution of soap behaved in much the same manner as the distilled water of the first category. On the other hand, solutions of albumen, and notably of saponine, exercised at their surfaces an altogether abnormal resistance.

These experiments of Plateau undoubtedly establish a special property of the surface of liquids of the first and third categories; but the question remains open whether the peculiar action upon the needle is to be attributed to a viscosity in any way analogous to the ordinary internal viscosity which governs the flow through capillary tubes.

In two remarkable papers,[1] Marangoni attempts the solution of this problem, and arrives at the conclusion that Plateau's superficial viscosity may be explained as due to the operation of causes already recognized. In the case of water and other liquids of the first category, he regards the resistance experienced by the needle as mainly the result of the deformation of the meniscuses developed at the contacts on the two sides with the liquid surface. This view does not appear to me to be sound: for a deformation of a meniscus due to inertia would not involve any dissipation of energy, nor permanent resistance to the movement. But the second suggestion of Marangoni is of great importance.

On various grounds the Italian physicist concludes that "many liquids, and especially those of Plateau's third category, are covered with a superficial pellicle; and that it is to this pellicle that they owe their great superficial viscosity." After the observations of Dupré[2] and myself,[3] supported as they are by the theory of Prof. Willard Gibbs,[4] the existence of the superficial pellicle cannot be doubted; and its mode of action is thus explained by Marangoni[5]:—"The surface of a liquid, covered by a pellicle, possesses two superficial tensions; the first, which is the weaker and in constant action, is due to the pellicle; the second is in the latent state, and comes into operation only when the pellicle is ruptured. Since the latter tension exceeds the former, it follows that any force which tends to rupture the superficial pellicle upon a liquid encounters a resistance which increases with the difference of tensions between the liquid and the pellicle." In Plateau's experiment the advancing edge of the needle tends to concentrate the superficial contamination, and the retreating edge to attenuate it; the tension in front is thus inferior to the tension behind, and a force is called into operation tending to check the vibration. On a pure surface it is evident that nothing of this sort can occur, unless it be in a very subordinate degree, as the result of difference of temperature.

This is an important distinction, discussed by Willard Gibbs, according as the contamination, to which is due the lowering of tension, is merely accidentally present upon the surface, or is derived from the body of the liquid under the normal operation of chemical and capillary forces. In the latter case, that, for example, of solutions of soap and of camphor, the changes of tension which follow an extension or contraction of the surface may be of very brief duration. After a time, dependent largely upon the amount of contaminating substance present in the body of the liquid, equilibrium is restored, and the normal tension is recovered. On the other hand, in the case of a surface of water contaminated with a film of insoluble grease, the changes of tension which accompany changes of area are of a permanent character.

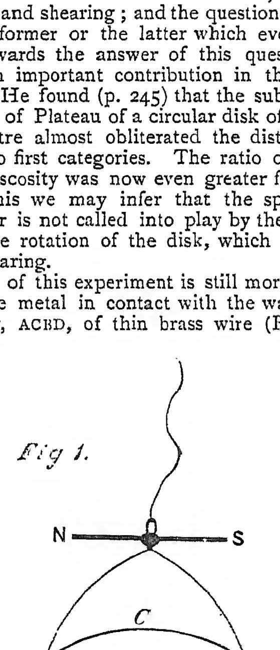
It is not perfectly clear how far Marangoni regarded his principle of surface elasticity as applicable to the explanation of Plateau's observations upon distilled water; but, at any rate, he applied it to the analogous problem of the effect of oil in calming ripples. It is unfortunate that this attempt at the solution of a long-standing riddle cannot be regarded as successful. He treats the surface of the sea in its normal condition as contaminated, and therefore elastic, and he supposes that, upon an elastic surface, the wind will operate efficiently. When oil is scattered upon the sea, a non-elastic surface of oil is substituted for the elastic surface of the sea, and upon this the wind acts too locally to generate waves. It is doubtless true that an excess of oil may render a water surface again inelastic; but I conceive that the real explanation of the phenomenon is to be found by a precisely opposite application of Marangoni's principle, as in the theories of Reynolds (Brit. Assoc. Rep., 1880) and Aitken (Edinburgh Roy. Soc. Proc., 1882–83, vol. xii. p. 56). Marangoni was, perhaps, insufficiently alive to the importance of *varying degrees* of contamination. An ordinary water surface is indeed more or less contaminated; and on that account is the less, and not the more, easily agitated by wind. The effect of a special oiling is, in general, to increase the contamination and the elasticity dependent thereupon, and stops short of the point at which, on account of saturation, elasticity would again disappear. The more elastic surface refuses to submit itself to the local variations of area required for the transmission of waves in a normal manner. It behaves rather as a flexible but inextensible membrane would do, and, by its drag upon the water underneath, hampers the free production and propagation of waves.

The question whether the effects observed by Plateau upon the surface of distilled water are, or are not, due to contamination must, I suppose, be regarded as still undecided. Oberbeck, who has experimented on the lines of Plateau, thus sums up his discussion:—"Wir müssen daher schliessen, entweder, dass der freien Wasseroberfläche ein recht bedeutender Oberflächenwiderstand zukommt, oder dass eine reine Wasseroberfläche in Berührung mit der Luft überhaupt nicht existirt" (*Wiedemann's Annalen*, vol. xi. 1880, p. 650).

Postponing for the moment the question of the origin of "superficial viscosity," let us consider its character. A liquid surface is capable of two kinds of deformation, dilatation (positive or negative) and shearing; and the question at once presents itself, Is it the former or the latter which evokes the special resistance? Towards the answer of this question Marangoni himself made an important contribution in the earlier of the memoirs cited. He found (p. 245) that the substitution for the elongated needle of Plateau of a circular disk of thin brass turning upon its centre almost obliterated the distinction between liquids of the two first categories. The ratio of the superficial to the internal viscosity was now even greater for ether than for water. From this we may infer that the special superficial viscosity of water is not called into play by the motions of the surface due to the rotation of the disk, which are obviously of the nature of shearing.

A varied form of this experiment is still more significant. I have reduced the metal in contact with the water surface to a simple (2″) ring, ACBD, of thin brass wire (Fig. 1). This is

Fig 1.



supported by a fine silk fibre, so that it may turn freely about its centre. To give a definite set, and to facilitate forced displacements, a magnetized sewing needle, NS, is attached with the aid of wax. In order to make an experiment, the ring is adjusted to the surface of water contained in a shallow vessel. When all is at rest, the surface is dusted over with a little fine sulphur,[1] and the suspended system is suddenly set into rotation by an external

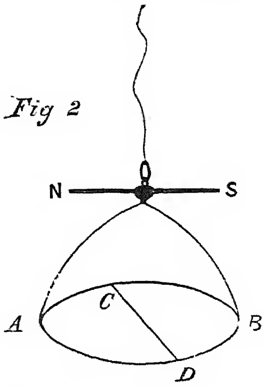
1 *Nuovo Cimento*, Ser. 2, vols. v.–vi., April 1872: Ser. 3, vol. iii., 1878.
2 "Théorie Mécanique de la Chaleur," Paris, 1869, p. 377.
3 "On the Tension of Recently Formed Liquid Surfaces," Roy. Soc. Proc., vol. xlvii., 1890, p. 281 (*supra*).
4 Connecticut Acad. Trans., vol. iii., Part II., 1877–78. In my former communication I overlooked Prof. Gibbs's very valuable discussion on this subject.
5 *Nuovo Cimento*, vols. v.–vi., 1871–72, p. 260 (May 1872).

1 Sulphur seems to be on the whole the best material, although it certainly communicates some impurity to the surface. Freshly heated pumice or wood-ashes sink immediately; and probably all powders really free from grease would behave in like manner.

magnet. The result is very distinct, and contrasts strongly with that observed by Plateau. Instead of the surface enclosed by the ring being carried round with it in its rotation, not the smallest movement can be perceived, except perhaps in the immediate neighbourhood of the wire itself. It is clear that an ordinary water surface does not appreciably resist shearing.

A very slight modification of the apparatus restores the similarity to that of Plateau. This consists merely in the addition to the ring of a material diameter of the same brass wire, CD (Fig. 2). If the experiment be repeated, the sulphur in-

Fig 2



dicates that the whole water surface included within the semicircles now shares in the motion. In general terms the surface may be said to be carried round with the ring, although the motion is not that of a rigid body.

Experiments of this kind prove that what a water surface resists is not shearing, but local expansions and contractions of area, even under the condition that the total area shall remain unchanged. And this is precisely what should be expected, if the cause of the viscosity were a surface contamination. A shearing movement does not introduce any variation in the density of the contamination, and therefore does not bring Marangoni's principle into play. Under these circumstances there is no resistance.

It remains to consider liquids of the third category in Plateau's nomenclature. The addition of a little oleate of soda does not alter the behaviour of water, at least if the surface be tolerably fresh. On the other hand, a very small quantity of saponine suffices to render the surface almost rigid. In the experiment with the simple ring the whole interior surface is carried round as if rigidly attached. A similar effect is produced by gelatine, though in a less marked degree.

In the case of saponine, therefore, it must be fully admitted that there is a superficial viscosity not to be accounted for on Marangoni's principle by the tendency of contamination to spread itself uniformly. It seems not improbable that the pellicle formed upon the surface may have the properties of a solid, rather than of a liquid. However, this may be, the fact is certain that a contracting saponine surface has no definite tension alike in all directions. A sufficient proof is to be found in the well-known experiment in which a saponine bubble becomes wrinkled when the internal air is removed.

The quasi-solid pellicle on the surface of saponine would be of extreme thinness, and, even if it exist, could hardly be recognizable by ordinary methods of examination. It would moreover be capable of re-absorption into the body of liquid if unduly concentrated by contraction of surface, differing in this respect from the gross, and undoubtedly solid, pellicles which form on the surface of hard water on exposure to the atmosphere.

Two further observations relative to saponine may here find a place. The wrinkling of a bubble when the contained gas is exhausted occurs also in an atmosphere (of coal gas) from which oxygen and carbonic acid are excluded.

In Plateau's experiment a needle which is held stiffly upon the surface of a saponine solution is to a great extent released when the surface is contaminated by grease from the finger or by a minute drop of petroleum.

To return to the case of water, it is a question of the utmost importance to decide whether the superficial viscosity of even distilled water is, or is not, due to contamination with a film of foreign matter capable of lowering the tension. The experiments of Oberbeck would appear to render the former alternative very improbable; but, on the other hand, if the existence of the film be once admitted, the observed facts can be very readily explained. The question is thus reduced to this: Can we believe that the water surface in Plateau's apparatus is almost of necessity contaminated with a greasy film? The argument which originally weighed most with me in favour of the affirmative answer is derived from the experiments of Quincke upon mercury. It is known that, contrary to all analogy, a drop of water does not ordinarily spread upon the surface of mercury. This is certainly due to contamination with a greasy film; for Prof. Quincke (*Poggendorff's Annalen*, vol. cxxxix., 1870, p. 66) found that it was possible so to prepare mercury that water would spread upon it. But the precautions required are so elaborate that probably no one outside Prof. Quincke's laboratory has ever witnessed what must nevertheless be regarded as the normal behaviour of these two bodies in presence of one another. The bearing of this upon the question under discussion is obvious. If it be so difficult to obtain a mercury surface which shall stand one test of purity, why may it not be equally difficult to prepare a water surface competent to pass another?

The method by which I have succeeded in proving that Plateau's superficial viscosity is really due to contamination consists in the preparation of a pure surface exhibiting quite different phenomena; and it was suggested to me by an experiment of Mr. Aitken (*loc. cit.*, p. 69). This observer found that, if a gentle stream of air be directed vertically downwards upon the surface of water dusted over with fine powder, a place is cleared round the point of impact. It may be added that on the cessation of the wind the dust returns, showing that the tension of the bared spot exceeds that of the surrounding surface.

The apparatus, shown in Figs. 3 and 4, is constructed of sheet brass. The circular part, which may be called the *well*, has the dimensions given by Plateau. The diameter is 11 cm., and the depth 6 cm. The needle is 10 cm. long, 7 mm. in breadth at the centre, and about 0·3 mm. thick. It is suspended at a height of $2\frac{1}{2}$ cm. above the bottom of the vessel. So far there is nothing special; but in connection with the well there is a rectangular trough, or tail-piece, about $2\frac{1}{2}$ cm. broad and 20 cm. long. Between the two parts a sliding door may be inserted, by which the connection is cut off, and the circular periphery of the well completed. The action of the apparatus depends upon a stream of wind, supplied from an acoustic bellows, and discharged from a glass nozzle, in a direction slightly downwards, so as to strike the water surface in the tail-piece at a point a little beyond the door. The effect of the wind is to carry any greasy film towards the far end, and thus to purify the near end of the tail-piece. When the door is up, this effect influences also the water surface in the well upon which the jet does not operate directly. For, if the tension there be sensibly less than that of the neighbouring surface in the tail-piece, an outward flow is generated, and persists as long as the difference of tensions is sensible. The movements of the surface are easily watched if a little sulphur be dusted over; when the water in the well has been so far cleansed that but little further movement is visible, the experiment may be repeated without changing the water by contaminating the surface with a little grease from the finger or otherwise. In this way the surface may be freed from an insoluble contamination any number of times, the accumulation of impurity at the far end of the tail-piece not interfering with the cleanness of the surface in the well.

Another device that I have usually employed facilitates, or at any rate hastens, the cleansing process. When the operation is nearly complete, the movement of the surface becomes sluggish on account of the approximate balance of tensions. At this stage the movement may be revived, and the purification accelerated, by the application of heat to the bottom of the well at the part furthest removed from the tail-piece. It may, perhaps, be thought that convection currents might be substituted altogether for wind; but in my experience it is not so. Until a high degree of purity is attained, the operation of convection currents does not extend to the surface, being resisted by the film according to Marangoni's principle.

When the apparatus was designed, it was hoped that the door could be made a sufficiently good fit to prevent the return of the greasy film into the well; but experience showed that this could not be relied upon. It was thus necessary to maintain the wind during the whole time of observation. The door was, however, useful in intercepting mechanical disturbance.

A very large number of consistent observations have been recorded. The return of the needle, after deflection to 90°, is timed over an arc of 60°, viz. from 90° to 30°, and is assisted by a fixed steel magnet acting in aid of the earth's magnetism. A metronome, beating three times per second, facilitates the time measurement. As an example, I may quote some observations made on April 11.

The apparatus was rinsed and carefully filled with distilled water. In this state the time was 12 (beats). After blowing for a while there was a reduction to 10, and after another operation to 8. The assistance of convection currents was then appealed to, and the time fell to $6\frac{3}{4}$, and after another operation to 6. This appeared to be the limit. The door was then opened, and the wind stopped, with the result that the time rose again to 12. More water was then poured in until the needle was drowned to the depth of about half an inch. Under these conditions the time was $6\frac{3}{4}$.

It will be seen that, while upon the unprepared surface the time was nearly twice as great as in the interior, upon the purified surface the time was somewhat less than in the interior.

For the sake of comparison, precisely similar observations were made upon the same day with substitution for water of methylated alcohol. Before the operation of wind the time was 5; after wind, 5; on repetition, still 5. Nor with the aid of convection currents could any reduction be effected. When the needle was drowned, the time rose to $7\frac{1}{4}$. The alcohol thus presents, as Plateau found, a great contrast with the unprepared water; but comparatively little with the water after treatment by wind and heat.

An even more delicate test than the time of vibration is afforded by the behaviour of the surface of the liquid towards the advancing edge of the needle. In order to observe this, it is necessary to have recourse to motes, but all superfluity should be avoided. In a good light it is often possible to see a few motes without any special dusting over. In my experience, an unprepared water surface always behaves in the manner described by Plateau; that is, it takes part in rotation of the needle, almost from the first moment. Under the action of wind a progressive change is observed. After a time the motes do not begin their movement until the needle has described a considerable arc. At the last stages of purification, a mote, situated upon a radius distant 30° or 40° from the initial direction of the needle, retains its position almost until struck; behaving, in fact, exactly as Plateau describes for the case of alcohol. I fancied, however, that I could detect a slight difference between alcohol and water even in the best condition, in favour of the former. With a little experience it was easy to predict the "time" from observations upon motes; and it appeared that the last degrees of purification told more upon the behaviour of the motes than upon the time of describing the arc of 60°. It is possible, however,

Fig 3.

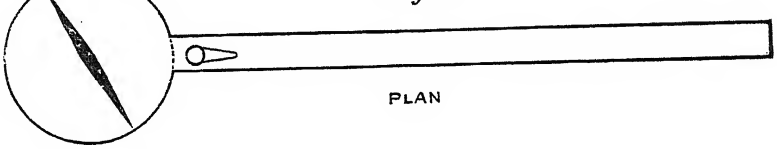
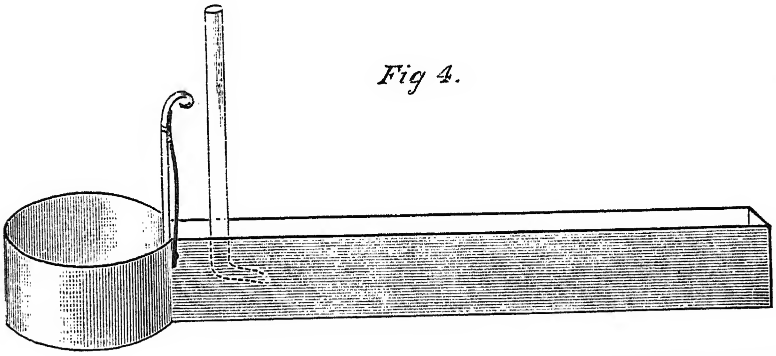


Fig 4.



that a different range from that adopted might have proved more favourable in this respect.

The special difficulties under which Plateau experimented are well known, and appealed strongly to the sympathies of his fellow workers; but it is not necessary to refer to them in order to explain the fact that the water surfaces that he employed were invariably contaminated. Guided by a knowledge of the facts, I have several times endeavoured to obtain a clear surface without the aid of wind, but have never seen the time less than 10. More often it is 12, 13, or 14. It is difficult to decide upon the source of the contamination. If we suppose that the greasy matter is dissolved, or, at any rate, suspended in the body of the liquid in a fine state of subdivision, it is rather difficult to understand the comparative permanence of the cleansed surfaces. In the case of distilled water, the condition will usually remain without material change for several minutes. On the other hand, with tap water (from an open cistern), which I have often used, although there is no difficulty in getting a clean surface, there is usually a more rapid deterioration on standing. The progressive diminution of the tension of well-protected water surfaces observed by Quincke (*Poggendorff's Annalen*, vol. clx., 1877, p. 580) is most readily explained by the gradual formation of a greasy layer composed of matter supplied from the interior, and present only in minute quantity; although this view did not apparently commend itself to Quincke himself. If we reject the supposition that the greasy layer is evolved from the interior of the liquid, we must admit that the originally clean free surface, formed as the liquid issues from a tap, is practically certain to receive contamination from the solid bodies with which it comes into contact. The view, put forward hypothetically by Oberbeck, that contamination is almost instantly received from the atmosphere is inconsistent with the facts already mentioned.

Some further observations, made in the hope of elucidating this question, may here be recorded. First, as to the effect of soap, or rather oleate of soda. A surface of distilled water was prepared by wind and heat until the time was $5\frac{1}{2}$, indicating a high degree of purity. The door being closed, so as to isolate the two parts of the surface, and the wind being maintained all the while, a few drops of solution of oleate were added to the water in the tail-piece. With the aid of gentle stirring, the oleate found its way, in a few minutes, under the door, and reached the surface of the water in the well. The time gradually rose to 13, 14, 15; and no subsequent treatment with wind and heat would reduce it again below 12. In this case there can be no doubt that the contamination comes from the interior, and is quickly renewed if necessary; not, however, so quickly that the tension is constant in spite of extension, or the surface would be free from superficial viscosity.

In like manner, the time upon the surface of camphorated dis-

tilled water could not be reduced below 10, and the behaviour of motes before the advancing needle was quite different from that observed upon a clean surface. A nearly saturated solution of chloride of sodium could not be freed from superficial viscosity; while, on the other hand, an addition of $\frac{1}{3}$ per cent. of alcohol did not modify the behaviour of distilled water.

The films of grease that may be made evident in Plateau's apparatus are attenuated in the highest degree. In a recent paper (*supra*, p. 364) I have estimated the thickness of films of olive oil competent to check the movements of camphor fragments as from one to two micro-millimetres; but these films are comparatively coarse. For example, there was never any difficulty in obtaining from tap-water surfaces upon which camphor was fully active without the aid of wind or special arrangements. I was naturally desirous of instituting a comparison between the quantities necessary to check camphor movements and the more minute ones which could be rendered manifest by Plateau's needle; but the problem is of no ordinary difficulty. A direct weighing of the contamination is out of the question, seeing that the quantity of oil required in the well of the apparatus, even to stop camphor, would be only $\frac{1}{64}$ milligram.

The method that I have employed depends upon the preparation of an ethereal solution of olive oil, with which clean platinum surfaces are contaminated. It may be applied in two ways. Either we may rely upon the composition of the solution to calculate the weight of oil remaining upon the platinum after evaporation of the solvent, or we may determine the relative quantities of solution required to produce the two sorts of effects. In the latter case we are independent of the precise composition of the solution, and more especially of the question whether the ether may be regarded as originally free from dissolved oil of an involatile character. In practice, both methods have been used.

The results were not quite so regular as had been hoped, the difficulty appearing to be that the oil left by evaporation upon platinum was not completely transferred to the water surface when the platinum was immersed, even although the operation was performed slowly, and repeated two or three times. On the other hand, there was no difficulty in cleansing a large surface of platinum by ignition in the flame of a spirit-lamp, so that it was absolutely without perceptible effect upon the movement of the needle over a purified water surface.

The first solution that was used contained 7 milligrams of oil in 50 c.c. of ether. The quantities of solution employed were reckoned in drops, taken under conditions favourable to uniformity, and of such dimensions that 100 drops measured 0·6 c.c. The following is an example of the results obtained:—On April 25, the apparatus was rinsed out and recharged with distilled water. Time = 13. After purification of surface by wind and heat, $5\frac{1}{2}$; rising, after a considerable interval, to 6. After insertion of a large plate of platinum, recently heated to redness, time unchanged. A narrow strip of platinum, upon which, after a previous ignition, three drops of the ethereal solution had been evaporated, was then immersed, with the result that the time was at once increased to $8\frac{1}{2}$. In subsequent trials, two drops never failed to produce a distinct effect. Special experiments, in which the standard ether was tested after evaporation upon platinum, showed that nearly the whole of the effect was due to the oil purposely dissolved.

The determination of the number of drops necessary to check the movements of camphor upon the same surface seemed to be subject to a greater irregularity. In some trials 20 drops sufficed; while in others 40 or 50 drops were barely enough. There seems to be no doubt that the oil is left in a rather unfavourable condition,[1] very different from that of the compact drop upon the small platinum surface of former experiments; and the appearance of the platinum on withdrawal from the water often indicates that it is still greasy. Under these circumstances it is clearly the smaller number that should be adopted; but we are safe in saying that $\frac{1}{15}$ of the oil required to check camphor produces a perceptible effect upon the time in Plateau's experiment, and still more upon the behaviour of the surface before the advancing needle, as tested by observation of motes. At this rate the thickness at which superficial viscosity becomes sensible in Plateau's apparatus is about $\frac{1}{10}$ of a micro-millimetre, or about $\frac{1}{6000}$ of the wave-length of yellow light.

[1] It should be stated that the evaporation of the ether, and of the dew which was often visible, was facilitated by the application of a gentle warmth.

A tolerably concordant result is obtained from a direct estimate of the smaller quantity of oil, combined with the former results for camphor, which were arrived at under more favourable conditions. The amount of oil in two drops of the solution is about 0·0017 milligram. This is the quantity which suffices to produce a visible effect upon the needle. On the large surface of water of the former experiments the oil required to check camphor was about 1 milligram. In order to allow for the difference in area, this must be reduced 64 times, or to 0·016 milligram. According to this estimate the ratio of thicknesses for the two classes of effects is about as 10 : 1.

Very similar results were obtained from experiments with an ethereal solution of double strength, one drop of which, evaporated as before, upon platinum, produced a distinct effect upon the time occupied by the needle in traversing the arc from 90° to 30°.

I had expected to find a higher ratio than these observations bring out between the thicknesses required for the two effects. The ratio 15 : 1 does not give any too much room for the surfaces of ordinary tap water, such as were used in the bath observations upon camphor, between the purified surfaces on the one side and those oiled surfaces upon the other, which do not permit the camphor movements.

It thus became of interest to inquire in what proportion the film originally present upon the water in the bath experiments requires to be concentrated in order to check the motion of camphor fragments. This information may be obtained, somewhat roughly it is true, by dusting over a patch of the water surface in the centre of the bath. When a weighed drop of oil is deposited in the patch, it drives the dust nearly to the edge, and the width of the annulus is a measure of the original impurity of the surface. When the deposited oil is about sufficient to check the camphor movements, we may infer that the original film bears to the camphor standard a ratio equal to that of the area of the annulus to the whole area of the bath. Observations of this kind indicated that a concentration of about six times would convert the original film into one upon which camphor would not freely rotate.

Another method by which this problem may be attacked depends upon the use of flexible solid boundary. This was made of thin sheet brass, and is deposited upon the bath in its expanded condition, so as to enclose a considerable area. Upon this surface camphor rotates, but the movement may be stopped by the approximation of the walls of the boundary. The results obtained by this method were of the same order of magnitude.

If these conclusions may be relied upon, it will follow that the initial film upon the water in the bath experiments is not a large multiple of that at which superficial viscosity tends to disappear. At the same time, the estimate of the total quantity of oil which must be placed upon a really pure surface in order to check the movements of camphor must be somewhat raised, say from 1·6 to 1·9 micro-millimetre. It must be remembered, however, that on account of the want of definiteness in the effects, these estimates are necessarily somewhat vague. By a modification of Plateau's apparatus, or even in the manner of taking the observations, such as would increase the extent of surface from which the film might be accumulated before the advancing edge of the needle, it would doubtless be possible to render evident still more minute contaminations than that estimated above at one-tenth of a micro-millimetre.

[P.S. *June* 4.—In order to interpret with safety the results obtained by Plateau, I thought it necessary to follow closely his experimental arrangements; but the leading features of the phenomenon may be well illustrated without any special apparatus. For this purpose, the needle of the former experiments may be mounted upon the surface of water contained to a depth of 1 or 2 inches in a large flat bath. Ordinary cleanliness being observed, the motes lying in the area swept over by the needle are found to behave much as described by Plateau. Moreover, the motion of the needle under the action of the magnet used to displace it is decidedly sluggish. In order to purify the surface, a hoop of thin sheet brass is placed in the bath, so as to isolate a part including the needle. The width of the hoop must, of course, exceed the depth of the water, and that to an extent sufficient to allow of manipulation without contact of the fingers with the water. If the hoop be deposited in its contracted state, and be then opened out, the surface contamination is diminished in the ratio of the areas. By this simple device there is no difficulty in obtaining a highly purified surface, upon which motes lie quiescent, almost until struck by the oscillating needle. In

agreement with what has been stated above, an expansion of three or four times usually sufficed to convert the ordinary water surface into one upon which superficial viscosity was tending to disappear.

I propose to make determinations of the actual tension of surfaces contaminated to various degrees; but in the meantime it is evident that the higher degrees of purity do not imply much change of tension. In the last experiment, upon a tolerably pure surface, if we cause the needle to oscillate rapidly backwards and forwards through a somewhat large angle, we can clear away the contamination from a certain area. This contamination will, of course, tend to return, but observation of motes shows that the process is a rather slow one.

The smallness of the forces at work must be the explanation of the failure to clean the surface in Plateau's apparatus by mere expansion. For this experiment the end wall was removed from the tail-piece (Fig. 3), and a large flexible hoop substituted. By this means, it was hoped that when the whole was placed in the bath it would be possible, by mere expansion of the hoop, to obtain a clean surface in the well. The event proved, however, that the purification did not proceed readily beyond the earlier stages, unless the passage of the contamination through the long channel of the tail-piece was facilitated by wind.]

UTILIZATION OF NIAGARA FALLS.

A SYNDICATE in the United States have acquired a considerable area of land on the American side of the Niagara River, at some distance above the great Falls. They propose to use it for mill sites, and to supply the mills with power by utilizing a small fraction of the water-power which is available on the Falls. The actual fall of level at Niagara is about 200 feet. Suppose that about 4 per cent. of the water going over the Falls is taken, and an effective fall of 140 feet, irrespective of losses in the tail race, obtained, there might be utilized 120,000 horse-power. It is proposed to take the water by a short lateral canal, to allow it to descend vertically in shafts in which turbines will be placed, and then to discharge it by a tunnel tail race passing beneath the present town of Niagara, at a point below the Falls. It is part of the plan to transmit a portion of the power to the important manufacturing town of Buffalo, eighteen miles distant.

The project involves problems of very great complexity. The hydraulic motors will be of a size not hitherto constructed, and the governing conditions are different from those commonly met with where water power is utilized on streams of variable and limited flow. Then in the distribution of the power further problems arise. Power can be distributed to great distances by Hirn's system of wire ropes, as at Schaffhausen; by water or air under pressure, as in the compressed air systems of Paris and Birmingham and the Hydraulic Power Company's system in London. In Switzerland and America progress has been made in distributing large power to great distances electrically. The choice amongst such methods of those which are most economical and most likely to suit the wants of mill-owners, requires very careful consideration.

Hence the Cataract Company have resolved to invite from certain selected engineers and engineering firms, plans for the utilization at Niagara of 120,000 horsepower, and to submit the plans for an authoritative opinion to the judgment of a Scientific International Commission. The Commission will consist of Sir William Thomson, F.R.S., as President; Prof. Mascart, Member of the Institut, and Director of the Bureau Central Météorologique, Paris; Colonel Theodore Turrettini, who was director of the works of the Saint Gothard Tunnel, and is director of the works for the utilization of the motive power of the Rhone at Geneva; and, lastly, Dr. Coleman Sellers, formerly of the firm of Messrs. Sellers and Co., of Philadelphia, and now Professor of Engineering at the Stevens Institute, Hoboken, and at the Franklin Institute of Pennsylvania. Prof. W. C. Unwin, F.R.S., is the Secretary to the Commission.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, June 19.—Prof. Charles Stewart, President, in the chair.—Mr. W. H. Beeby exhibited a specimen of *Rumex propinquus* new to Britain, and procured in Shetland. —Mr. Thomas Christy exhibited and made remarks upon a specimen of *Callistemon rigidum*.—Mr. E. M. Holmes exhibited some marine Algæ new to Britain, including *Ascocyclus reptans*, *Halothrix lumbricalis*, *Harveyella mirabilis*, *Sorocarpus uvæformis*, and *Vaucheria litorea*; also specimens of *Rhodymenia palmata* with antheridia, and *Punctaria tenuissima* in fructification, the last two not having been previously recorded to occur in this state in Great Britain.—The following papers were then read:—Observations on the protection of buds in the tropics, by M. C. Potter.—On the distribution of the South American Bell-birds belonging to the genus *Chasmorhynchus*, by J. E. Harting.—On the vertical distribution of plants in the Caucasus, by Dr. Gustav Radde.—Notes on the *Forficulidæ*, with descriptions of new genera and species, by W. F. Kirby.—This meeting terminated the Session of 1889-90.

Entomological Society, July 2.—Prof. J. O. Westwood, Hon. Life-President, in the chair.—Lord Walsingham exhibited some rare Micro-Lepidoptera collected by himself at Cannes, including *Eudemis helichrysana*, *Conchylis rubricana*, Millière; a new *Depressaria* from *Opoponax cheironium*, which is about to be described by M. A. Constant, and *Bucculatrix helichrysella*: and also a volume of drawings of larvæ of the genus *Eupithecia*, by Mr. Buckler, which formerly belonged to the late Rev. H. Harpur Crewe.—Mr. McLachlan exhibited larvæ and cocoons of *Mecyna deprivalis*, Walk., sent by Mr. W. W. Smith, of Ashburton, New Zealand; the species feeds commonly on *Genista capensis*, an introduced plant.—Mr. S. Stevens, in speaking of a tour which he had lately made in Devonshire, remarked on the extreme scarcity of insects on the coast of that county as compared with the coasts of Kent and Sussex; there were very few larvæ, and the vegetation was very luxuriant and very little eaten: he thought it possible that the reason of the scarcity was the heavy rainfall of South Devon, which washed off and destroyed the young larvæ. Mr. Barrett said that his experience had been the same, and that he put it down to the violence of the winds which beat the insects from the trees. Mr. Blandford remarked that he had found Coleoptera abundant on the Braunton Burrows, near Barnstaple, but very scarce in other localities. Mr. Mason and others took part in the discussion which followed.—Prof. Westwood read a paper on a species of Aphis affecting the bread-fruit tree, which he had named *Siphonophora artocarpi*: at the conclusion of his paper he alluded to the use of Paris-green as a destructive agent for insects. Mr. Blandford then made some remarks as to the use of London-purple (another arsenic compound) as an insecticide in the place of Paris-green; he stated that the compound was a waste product, and one-tenth the cost of Paris-green, and further that it was more soluble and more easily applied; he was also of opinion that arsenic compounds do not greatly affect sucking insects, such as Aphides, the ordinary kerosene preparations being more suitable for their destruction. Several Fellows took part in the discussion that followed.

EDINBURGH.

Royal Society, June 16.—The Hon. Lord M'Laren, V.P., in the chair.—A list of West Australian birds, showing their geographical distribution throughout Australia, by Mr. A. J. Campbell, Melbourne, was communicated.—Dr. Buchan discussed a difference between the diurnal barometric curves at Greenwich and at Kew.—Dr. Sang communicated a paper on the general formulæ for the passage of light through a spherically arranged atmosphere.—Dr. Buchan gave an account of a remarkable barometric reading at the Ben Nevis Observatory on April 8, 1890.—Prof. Crum Brown read the third part of a paper, written by himself in conjunction with Dr. James Walker, on synthesis by means of electrolysis.

July 7.—Sir William Thomson, President, in the chair.—The Victoria Jubilee Prize for 1887-90 was presented to Prof. Tait for his work in connection with the *Challenger* Expedition and his other researches in physical science. The Keith Prize for 1887-89 was presented to Prof. Letts for his researches into the organic compounds of phosphorus. The Neill Prize for 1886-89 was awarded to Mr. Robert Kidston for his researches in fossil botany.—Sir W. Thomson read a paper on the submarine cable problem, with electromagnetic induction. The solution of the problem with intermittent or alternating currents of period so long that the distribution of current over a given cross-section of the core is uniform, is already well known. Sir W. Thomson extends the solution, through all intermediate stages, to the

case in which the period is so short that the current is confined to an exceedingly thin surface-layer of the core. He has worked out the conditions which obtain with a core and sheath of any forms. The thickness of the layer depends only, other things being equal, upon the period of alternation—the law being that given by Fourier for the penetration of the annual and diurnal heat-waves into the earth's crust. The distribution of density throughout the layer depends upon the form and relative position of the core and the sheath.—Prof. Crum Brown and Dr. James Walker, in continuation of their research on the formation of dibasic acids by electrolysis, communicated a paper on the synthesis of suberic acid and a new acid $(CH_2)_{12}(COOH)_2$.—Prof. Tait exhibited some graphic records of impact, obtained by the method described in a previous paper.—Dr. James Geikie read a paper by Mr. R. Kidston, on the fossil flora of the Potteries coal-field.—The Hon. Lord M'Laren read a paper on the reduction of certain algebraic equations.—Prof. Tait read an account, by Prof. A. C. Mitchell, of a preliminary experiment on the thermal conductivity of aluminium, which he makes out to be almost exactly equal to that of the best copper.—Dr. Ralph Stockman and Mr. D. B. Dott communicated a paper on the pharmacology of morphine and its derivatives.—Dr. W. Somerville made a communication on *Larix europæa* as a breeding-place for *Hylesinus piniperda*.

PARIS.

Academy of Sciences, July 7.—M. Hermite in the chair.—Photographic stellar spectra obtained by MM. Henry at Paris Observatory, by Admiral Mouchez. (See Our Astronomical Column.)—On the oxidation of the sulphur of organic compounds, by MM. Berthelot, André, and Matignon. The authors give a general method for the estimation of sulphur in all organic bodies containing that element, consisting in burning the body either alone or mixed with camphor in an atmosphere of compressed oxygen in the presence of about 10 c.c. of water, with subsequent precipitation of the sulphuric acid in the usual manner.—Heats of combustion of some sulphur compounds, by MM. Berthelot and Matignon.—Heats of combustion of erythrite, arabinose, xylose, raffinose, and inosite, by MM. Berthelot and Matignon.—New experiments on the silent discharge, by M. P. Schutzenberger.—The active elasticity of muscle, and the energy used in its creation in the case of static contraction, by M. A. Chauveau.—Note on the difficulty in recognizing the Cysticerca of *Tænia saginata* or *inermis* in the muscles of the calf and cow, by M. A. Laboulbène.—On the propagation of sound in cylindrical tubes, by M. V. Neyreneuf.—The theory of periodic comets, by M. O. Callandreau. The author finds that the "capture" theory of periodic comets is sufficient to explain the characteristic properties of their orbits and the objections that have been opposed to it.—On a photograph of the ring nebula in Lyra obtained at Bordeaux Observatory, by M. G. Rayet. (See Our Astronomical Column.)—Partial eclipse of the sun of June 17, by M. J. Léotard. The times of first and last contact are given.—Occultation of the double star β Scorpii by the moon on June 29, by the same author.—On the anomalous propagation of waves, by M. Gouy.—Action in the dry way of different arsenates of potassium and sodium on the sesquioxides of some metals, by M. C. Lefèvre.—On a new method of preparing basic nitrate of copper and some crystallized subnitrates, by M. G. Rousseau. The basic nitrates are obtained in large crystals from the hydrates of corresponding neutral salts.—On double bromides of phosphorus and iridium, by M. G. Geisenheimer.—On some chromoiodates, by M. A. Berg.—The artificial production of boracite in the wet way, by M. A. de Gramont.—On the nitroprussides, by M. Prud'homme.—On the cause of the alteration which certain compounds of the aromatic series undergo under the influence of air and light, by M. André Bidet.—Transformation of glucose into sorbite, by M. J. Meunier.—On the hydrogenation of sorbine and the oxidation of sorbite, by MM. Camille Vincent and Delachanal.—Syntheses by means of cyanacetic ether: dicyanacetic ethers, by M. A. Haller.—The preparation of certain ethers by means of fermentation, by M. Georges Jacquemin.—On the physiological action of thallium salts, by Mr. J. Blake.—On the pretended circulatory system and genital organs of Neomenidæ, by M. G. Pruvot.—On the *rôle* of the bud-shaped pedicles of sea-urchins, by M. Henri Prouho.—On the histological constitution of some Nematoids of the order Ascaris, by M. Léon Jammes.—On the comparative physiology of the sense of smell, by M. Raphael Dubois.—The basaltic eruptions of the valley of the Allier, by M. Marcellin Boule.—On the mineralogical composition of the volcanic rocks of the islands of Martinique and Saba, by M. A. Lacroix.—On the relation between joints and some surface wrinklings near Doullens, by M. Henri Lasne.

BERLIN.

Physical Society, June 27.—Prof. von Helmholtz, President, in the chair.—Dr. Dubois spoke on magnetic closed circuits, whose theory constitutes, in addition to hysteresis, the most important advance which magnetism has made in recent times. He gave a short historical review of the more important published works on the subject, pointing out that they were at first the result rather of an endeavour to make the requisite calculations connected with dynamos for technical purposes, and had only attracted the attention of physicists in a secondary and subordinate degree. The works of Faraday, Maxwell, Sir W. Thomson, Hopkinson, Lord Rayleigh, and the experimental researches of Rowland, were briefly mentioned; Hopkinson's formulæ and Lord Rayleigh's graphic representations were then more fully treated; and, finally, the formula for the magnetization of a closed circuit was developed.—Dr. Raps described an arrangement of Topler's mercurial air-pump, by means of which he had made it work automatically; he further described a compensated air-thermometer which he had constructed, and exhibited both instruments to the Society.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The International Annual of Authony's Photographic Bulletin, vol. 3, 1890–91 (Iliffe).—Reflections on the Motive Power of Heat: N. L. S. Carnot; edited by R. H. Thurston (Macmillan and Co.).—Hypnotism: A. Moll (W. Scott).—Light: E. W. Tarn (Lockwood).—Elementary Mechanics (Blackie).—Timbers, and How to Know Them: Dr. R. Hartig; translated by W. Somerville (Edinburgh, Douglas).—Introduction to Fresh-water Algæ: Dr. M. C Cooke (K. Paul).—Short Logarithmic and other Tables, 4th edition: W. C. Unwin (Spon).—Walks in the Ardennes, new edition: P. Lindley (London).—Tourist Guide to the Continent: P. Lindley (London).—Sectional Map of South Dakota (Chicago, Rand).—Pocket Map, &c., of Michigan (Chicago. Rand).—Confidential Chats with Mothers: Mrs. Bowdick (Baillière).—British Cage Birds, Part 3: R. L. Wallace (L. Gill).—Canary Book, Part 3: R. L. Wallace (L. Gill).—Mathematical and Physical Papers, vol. 3: Sir Wm. Thomson (Cambridge University Press).—Electric Light Fitting: J. W. Urquhart (Lockwood).—Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History), Part 4: R. Lydekker (London).—L'Esprit de Nos Bêtes: E. Alix (Paris, J. B. Baillière)—Journal of the Royal Agricultural Society, vol. 1 (third series), Part 2; General Index to ditto, second series, (Murray).—Transactions of the Royal Society of Victoria, vol. 1, Part 2 (Melbourne).—Proceedings of the Royal Society of Edinburgh, vol. 16, pp. 385 to 846; vol. 17, pp. 1 to 128 (Edinburgh).—Transactions of the Royal Society of Edinburgh, vol. 33, Part 3; vol. 35, Parts 1 to 4 (Edinburgh).

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THURSDAY, JULY 24, 1890.

THE COLOURS OF ANIMALS.

The Colours of Animals: their Meaning and Use especially considered in the case of Insects. By Edward Bagnall Poulton, M.A., F.R.S., &c. With Chromolithograph Frontispiece and Sixty-six Figures in Text. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1890.)

THIS new volume of the International Scientific Series gives an excellent summary of the most recent researches as to the varied uses of the colours of animals, and more especially of those admirable observations and experiments on variable protective colouring with which Mr. Poulton's name is associated, and which mark an era in this branch of natural history. The main outlines of the subject are so well known, both to naturalists and to general readers, that it will only be necessary here to indicate some of the more important of the matters now first treated in a popular work, and to make a few remarks on some of the more difficult problems discussed in the volume.

The first chapter gives a short but very clear statement of the physical cause of animal colours, and contains some valuable observations on the effect of thin films of air or of liquids in the production of iridescent colours. In some cases dried insects lose some of their metallic colours, but these reappear when the specimen is dipped in water. Even living beetles have been observed to lose their lustre after hybernation, and to regain it after drinking water. Then we have a sketch of the general uses of colour to animals, and it is shown that the frequent dark colour of arctic insects has probably a physiological use in enabling them to absorb as much heat as possible during the brief period of their existence under an arctic sun. This is supported by some direct observations; but the further suggestion that the white colour of so many arctic birds and mammals has also a physiological use in checking the loss of heat through radiation is less satisfactory. Not only is there no evidence to show that the loss of animal heat is at all influenced by the colour of the fur or feathers, but it is evident that the same result could be brought about by a very slight increase in the texture or thickness of the covering, such as actually occurs in all arctic animals. In the seventh chapter there is a very interesting discussion on the way in which the white winter coat of arctic animals is produced, and it is shown that in the American arctic hare the brown hairs of the summer coat turn white at the tips, while a quantity of new white hairs grow among them, producing at once the thickening of the coat for warmth and the change of colour for protection. That this last is the only function of the colour is well indicated by the case of the raven, which is found in the extreme north of the polar regions, even during the most intense colds of winter, wherever the reindeer and musk-sheep range. Yet it is here as black as elsewhere, although the occasional occurrence of pied and even of white ravens in various parts of Europe and America shows that a white race could be produced if that colour was of any advantage to the bird in its arctic habitat.

Two chapters are devoted to a subject which Mr. Poulton has made especially his own, the variable protective colouring of insects. This was first noticed by the late Mr. T. W. Wood, the well-known natural history artist who furnished many of the best illustrations for Darwin's "Expression of the Emotions in Man and Animals," and the result of his experiments were brought before the Entomological Society of London in 1867. Since then a few other observations have been made by several naturalists, but little was known of the extent or of the exact causes of the adaptation till Mr. Poulton carried out his experiments for several years in succession, and on so extensive a scale that in one year over 700 larvæ of the small tortoiseshell butterfly (*Vanessa urticæ*) were observed under various surroundings, and the colours of the resulting chrysalides recorded. In this way pupæ were obtained varying from black to nearly white or metallic golden colours, in each case corresponding more or less closely to the coloured surfaces on which they were suspended. By changing the coloured surroundings at different stages of the process, and by blinding some of the larvæ, it was ascertained that the period of susceptibility is the quiescent stage just before the change to the pupa state, and that in this case vision has nothing to do with the change of colour. By a number of ingenious experiments, it was ascertained that the whole surface of the skin is sensitive to the action of variously-coloured light, and the effect on the pupa-skin is produced, not directly, as by some photographic action, but by a physiological process acting through the nervous system. In some cases even the cocoons spun by the larvæ are modified by the surrounding colours; and still more curious changes are effected in the larva itself when, as in so many cases, the same species feeds on several plants having differently-coloured leaves. Even the presence of numerous dark twigs has been shown to cause a corresponding change of colour in the larva of the peppered moth (*Amphidasis betularia*). These two chapters afford a beautiful example of a very difficult and interesting inquiry leading to an explanation of some of the most curious colour-phenomena in the animal kingdom. Mr. Poulton points out the essential difference between this mode of colour-adjustment and that of the chameleon, and of some crustacea, frogs, and fishes, which can rapidly adjust their colours to new surroundings, in the following passage:—

"The essential difference between the two kinds of adjustment is that, in the one case, the pigmented part of certain cells contracts in obedience to nervous stimuli, and thus alters the general appearance; while in the other case the coloured part is actually built up of the appropriate tint, or loses its colour altogether and becomes transparent in obedience to the same stimuli. The frog or fish has a series of ready-made screens which can be shifted to suit the environment; the insect has the power of building up an appropriate screen. In many cases, however, the green colour of caterpillars is due to the ready-made colour of the blood, which becomes effective when pigment is removed from the superficial cells, but which disappears when the latter are rendered opaque. Here, however, the superficial cells form the screen which has to be built up, or from which the colour must be dismissed; and in certain species

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even the colour of the blood is entirely changed in the passage from a green to a dark variety, or *vice versâ*. Hence it is to be expected that the changes occurring in an insect will occupy a considerable time as compared with those which take place in a frog. Another difference between the two processes is that the stimulus from the environment falls upon the eye in the one case, and probably upon the surface of the skin in the other."

Mr. Poulton's work is of special importance for the numerous experimental proofs he gives of the protective value of many of the peculiarities in the colour markings or attitudes of insects. Thus the green lizard (*Lacerta viridis*) generally failed to detect a "stick caterpillar" in its position of rest, although the insect is seized and greedily devoured directly it moves. The value of the tufts of hair, called "tussocks," on many caterpillars was also proved experimentally.

"A caterpillar of the common vapourer moth (*Orgyia antiqua*) was introduced into a lizards' cage, and when attacked, instantly assumed the defensive attitude, with the head tucked in and the 'tussocks' separated and rendered as prominent as possible. An unwary lizard seized the apparently convenient projection; most of the 'tussock' came out in its mouth, and the caterpillar was not troubled further. The lizard spent a long and evidently most uncomfortable time in trying to get rid of its mouthful of hairs."

There is a most excellent account of the larva of the lobster moth (*Stauropus fagi*) which is protected by its marvellous resemblance to a withered beech-leaf and its stipules, and is also able to assume a terrifying attitude, when it resembles some large and strangely formed spider. When one of these larvæ had assumed the terrifying attitude, a marmoset monkey was much impressed by the alarming sight, and only ventured to attack after the most careful examination, and even then in the most cautious manner. A lizard exhibited the same caution before the larva was attacked. The same insect is also to some extent protected from ichneumons by two black marks, exhibited only when attacked, which resemble those produced by the stings of ichneumons, and thus prevent an attack, since these parasites always avoid larvæ which are already occupied.

Two chapters are devoted to an excellent account of the various forms of mimicry, a subject which, however interesting, has been so often treated that there is comparatively little new to be said upon it; and then we have two chapters on sexual colours, which will offer material for a few remarks, as the whole subject is full of difficulties, and requires much more observation and experiment before the problems it presents can be satisfactorily settled.

Mr. Poulton fully accepts Darwin's theory of female choice as the source of the greater part of the brilliant colour, delicate patterns, and ornamental appendages that exist among animals, and especially among birds and insects. Much stress is laid on the observations of two American writers on the courtship of spiders. These show that spiders resemble birds in the strange postures and long-continued antics of the male during courtship, and that he always exhibits whatever portion of his body is most conspicuously coloured.

"The female always watches the antics of the male intently, but often refuses him in the end, 'even after dancing before her for a long time.' Such observations strongly point towards the existence of female preference based on æsthetic considerations."

To the last four words we demur, as being altogether unproved. Why *æsthetic* considerations? Why not a deficiency in activity, or in size, or in some exciting odour, or in the excitability of the female at the moment? Any of these causes, or others unknown to us, may determine the acceptance or rejection of a male spider; and it is to be noted that the long-continued and careful observations of these American authors have not enabled them to adduce a single case in which any deficiency of colour was observed in a rejected male. There is, indeed, one case in which two well-marked male varieties of a species exist—one red, the other black; and these assume different attitudes in courtship. Messrs. Peckham say: "the *niger* form, evidently a later development, is much the more lively of the two, and whenever the two varieties were seen to compete for a female, the black one was successful." On this Mr. Poulton remarks: "It must be admitted that these facts afford the *strongest support* to the theory of sexual selection"; but there is not a particle of proof that the black colour was the cause of the selection rather than the "superior liveliness" which all breeders of animals believe to be the most attractive characteristic a male can possess.

Mr. Poulton speaks continually of the possession of an "æsthetic sense by those creatures in which sexual ornament occurs, but no proof whatever of this is given, other than the fact that insects do recognize diversities of colour, and that a few birds collect bright objects, as in the case of the bower-birds. This habit, existing in a few species only of one of the highest groups of birds, can hardly be held to be a proof that in all birds, even in such comparatively low types as ducks and Gallinaceæ, slight variations of colour in the male determine the choice of the female.

This æsthetic sense is supposed to exist even in insects, and some very doubtful facts are alleged in support of this view. It is stated that if all the brightly-coloured butterflies and moths in England were arranged in two divisions, the one containing all the beautiful patterns and combinations of colours, the other including the staring, strongly-contrasted colours and crude patterns, we should find that the latter would contain, with hardly an exception, the species in which independent evidence has shown, or is likely to show, the existence of some unpleasant quality. The former division would contain the colours displayed in courtship and when the insect is on the alert. And it is added that there is an immense difference between the two divisions—the one most pleasing, the other highly repugnant, to our æsthetic sensibilities, because the pleasing colours have been determined by the insect's sense of what is *beautiful*, the displeasing colours by the need for what is *conspicuous* to a vertebrate enemy. If there is, indeed, any such great and constant difference due to these causes, it must exist in all countries, and in all groups where these causes have come into play; but it is very doubtful whether any such difference does exist. In looking over a general collection of butterflies few would decide that the Danaidæ, Acræidæ, and Heliconidæ showed any deficiency in beauty and harmony of colour; yet they are pre-

eminently the groups in which warning colours are predominant. So, also, the American and Eastern sections of the genus Papilio which are both subjects of mimicry and have all the other characteristics of protected groups with warning colours, are all exquisitely beautiful, with their rich green or crimson spots on a velvety black ground. And if we turn to birds, in which, as there are no known warning colours, all that are not protective are supposed to be due to sexual selection, we find, among much that is beautiful, great numbers of the harshest contrasts and most inharmonious combinations of colour that it is possible to conceive. Such are the blues and yellows and reds of the macaws and of a great number of other parrots; the equally harsh colours of the barbets and the toucans; the contrasted blue and purple or magenta and black of many of the chatterers. In many of these, no doubt, the texture of the surface is so delicate and the colours so bright and pure that we cannot but admire the tints themselves, although it is impossible to claim for the mode in which they are combined even the rudiments of æsthetic beauty. On the other hand, we find really beautiful combinations of colour and marking where sexual selection has certainly not come into play. Such are the exquisite tints and patterns of the cones, cowries, olives, harps, volutes, pectens, and innumerable other molluscan shells; while many of the sea-anemones, and considerable numbers of the caterpillars with warning colours, are equally beautiful.

Still more doubtful and more opposed to reasonable probability is the statement that "our standards of beauty are largely derived from the contemplation of the numerous examples around us, which, strange as it may seem, have been created by the æsthetic preferences of the insect world"—alluding, of course, to the colours and structures of flowers as being due to the need of attracting insects to fertilize them. Here objection may be taken, first, to the term *preferences* as applying to mere beauty in the flower, and still more emphatically to the term *æsthetic*, which there is not a particle of evidence for believing to enter at all into an insect's very limited mentality. Insects visit flowers wholly and solely, so far as we know, to obtain food or other necessaries of their existence, and every fact connected with the colours of flowers can be explained as due to the advantage of conspicuousness amid surrounding foliage, and distinctness from other flowers which are especially suited to different species of insects. When cows and horses refuse to eat the acrid buttercup, we do not say that the glaring yellow colour is repugnant to their æsthetic sensibilities, and that their dislike to the plant as food is the result; yet this would be less improbable than that bees and butterflies have any admiration of or liking for flowers independent of the supply of their physical wants. Moreover, a large part of the beauty *we* see in flowers is independent of colour, and is due to the graceful forms of individual flowers, their elegant groupings, and their charming contrast to the foliage which surrounds them. We now know that much of the variety in the form and position of flowers is dependent on their own physical needs, the protection of the pollen and the germ from rain, wind, or insect enemies, and that it has been produced by natural selection acting under the limitations due to the fundamental laws of vegetable growth. The purity and intensity of the colours are due to the fact that such colours offer a greater contrast to the ever-varying tints of foliage, twig, and bark, seen under constant modifications of light and shade, than would be offered by more sober hues; and thus it is that flowers usually exhibit the purest and brightest colours, which, combined with their elegant or curious forms, and the exquisite setting of green foliage which surrounds them, produce a general effect which is to us inexpressibly charming. But we have no reason to believe that any of the lower animals are affected in the smallest degree by these truly æsthetic feelings, and the use of the term as applied to them is simply begging the question, and is, therefore, not scientific.

It is because Mr. Poulton himself admits that the theory of sexual selection is still to some extent *sub judice* that the preceding remarks have been made in the way of protest against the use of terms which themselves tend to prejudge the case. In his chapters on this subject he has brought many arguments in its favour, some of which are ingenious and novel; but they all appear to rest on very slender evidence or to admit of another interpretation. They will, however, be useful as an incitement to further observation on this most interesting question, which, in all probability, will not be finally settled by the present generation of naturalists.

The book is well illustrated by numerous excellent woodcuts and a coloured plate, and there appear to be few if any misprints, the only one calling for remark being the placing of the cut at p. 34 upside down, so that the resemblance to a catkin is lost. Mr. Poulton is to be congratulated on having produced so readable and suggestive a volume on one of the most attractive departments of natural history, and on having by his own researches contributed so largely to the solution of some of the more interesting problems which it presents.

ALFRED R. WALLACE.

A HAND-BOOK OF ASTRONOMY.

Hand-book of Astronomy. Parts II. and III. By George F. Chambers, F.R.A.S. (Oxford: Clarendon Press, 1890.)

IN commenting upon the first part of this revised edition of Mr. Chambers's "Descriptive and Practical Astronomy," we pointed out the utter insufficiency of the portion devoted to the study of the sun, inasmuch as it left solar spectroscopy altogether out of consideration. Such an arrangement is a breach in the continuity of scientific inquiry, and a grievous fault in a hand-book that makes some pretence to give facts in historical sequence.

The second volume deals with instrumental and practical astronomy, and in it we find spectroscopical astronomy interpolated; the work that has been done in this direction following the description of the instruments employed. This circumstance, however, at once exhibits an inconsistency, for, if spectroscopy properly follows a description of the spectroscope, then telescopy should follow a description of the telescope; whereas in the former volume the aspects of the heavenly bodies were described, and in this the instruments by means of which they are observed.

A cause for the omission of all spectroscopic information from the sections to which it respectively and properly belongs, and its relegation to a couple of chapters in another volume, would be difficult to find. To these chapters, however, for which Mr. Maunder, of Greenwich Observatory, is made responsible, all matters spectroscopical have been referred, and so far as space permitted, Mr. Maunder has furnished a very comprehensive summary of the subject he had in hand; hence it may be that Mr. Chambers has acted wisely in intrusting the discussion of spectroscopic labours to a practical man. But the first duty of the compiler of such a volume as the one before us is to chronicle facts without comment or bias, and to lay before his readers the conclusions that have been drawn from them, leaving them to stand or fall on their own merits. This, however, has not been done; many observations are introduced with disparaging remarks, and conclusions deduced from them are said to be "most ingenious, but far from satisfying," without any evidence being adduced of their fallacy.

We also note that the sequence of spectra observed in comets as they approach to or recede from the sun, and supporting the meteoritic origin of these bodies, is mildly objected to. The shift in the position of the citron comet band, which admits of a ready explanation when the masking effect of the first flutings of manganese and lead is considered, is questioned, and the sequence is said to be

"partly founded on discrepancies as to the positions of some of the bands, which may prove to be significant but which, more probably, are simply due to the difficulties of observation, and partly to the fact that the yellow band of the carbon series in cometary spectra does not always show the same exact correspondence with the carbon band as do those in the green and blue. In particular, it shows at times two or more maxima within its borders, and its redward edge is rather diffused. The positions of these maxima are variously given, but appear to be about 5570 and 5450. There are not a few instances, also, in which this yellow, or, rather, citron, band has been recorded as having its sole maximum at one or other of these wave-lengths. Lockyer ascribes these divergences to the influence of the flutings of manganese and lead, but, bearing in mind the great difficulty of many of these observations, and that the citron band is much the faintest of the three, it seems scarcely safe at present to draw such an inference."

It is here acknowledged that the citron comet band has not a fixed position in the spectrum, and that its appearance is not always the same. Whether it is safe to conclude that the two maxima at λ 557 and λ 545 are due respectively to the flutings of manganese (558) and lead (546), it is not now our place to discuss. Since, however, the shift is real, it is hardly scientific to assert that the measures of its wave-length given by various observers are discrepancies of observation. Again, it is to be regretted that in the survey of cometary spectra no mention is made of Dr. Huggins's important observations in 1866–67 of "a bright line between *b* and F, about the position of the double line of the spectrum of nitrogen," in the spectrum of each of two small comets that appeared in those years. This is also the position of the chief line in the spectrum of the nebulæ, and suggests the connection that exists between the two bodies.

The standard of excellence deemed necessary to establish a sequence in the spectra of comets, as they approach to or recede from the sun, has not been applied all through the work. We find a table showing in parallel columns the general agreement between the motions of stars in line of sight as measured by Dr. Huggins, Mr. Maunder, and at Rugby. To one unacquainted with instrumental difficulties, the motion of stars in line of sight would appear to be a quantity that may be determined with some accuracy; but to those who know the pitfalls, by far the greater number of such observations appear worthless, for the accuracy attained in the majority of measurements is not sufficiently fine to allow any reliance to be placed upon them. In many cases a star, according to observation, has been moving towards the earth at the rate of about 50 miles a second, whilst another observation, made, perhaps, two minutes afterwards, indicated that it was receding from the earth with the same velocity. It is hardly just, therefore, to select certain determinations and arrange them in parallel columns to demonstrate the efficiency of the method adopted. At the end of the discussion of these motions, a note occurs on Algol. It is shown that the satellite theory of this star's variability, propounded by Goodricke and developed by Pickering, is supported by the fact that observations of its motion in line of sight, may be divided into groups, which indicate that at one time it is approaching our system, and at another receding from it owing to its orbital velocity. With these results we have nothing to do; but, if we remember rightly, Prof. Vogel was the first to demonstrate the periodic shift of the F line towards the red and the violet end of the spectrum, and in a communication to the Berlin Academy he gave the elements of the orbit traversed. This being so, it is curious to find that Prof. Vogel's discussion of his photographs has been omitted, although some months intervened between the communication and the publication of this volume, whilst Mr. Maunder's later division of his grievously discrepant observations into groups has been included.

There are a few other points to which we would call the author's attention. In the portion devoted to chronological astronomy, the dates of the commencement of the seasons and their consequent lengths, are given for 1860, the corresponding dates for 1890 being inserted in a footnote. It would have cost but little trouble to substitute the latter times when bringing the book up to date, and no purpose is served by the present arrangement.

A new feature, and one to be commended, is the insertion of plans and specifications for small observatories; this will doubtless be appreciated by amateur astronomers, since the directions and measurements which accompany them are supposed to be such that any builder of ordinary intelligence will be enabled to undertake the construction. It was hardly necessary, however, to give the description and sketch of an observatory on the tower of a dwelling-house and surrounded by chimneys, such as that possessed by the author. The position is certainly not conducive to accurate observations, and the dome described appears to offer every opportunity for being lifted off by a high wind and deposited in the garden.

But although the first and second volumes of this work possess a few commendable features, the third volume

has none. It bears the mark of hurried revision, and stands condemned as one of the most incomplete and incorrect productions of its kind. The title of the volume is "The Starry Heavens," and had it been written a quarter of a century ago might have contained most of the matter that is now given. In the face of this fact, which can be well substantiated, Mr. Chambers remarks, "The contents of the volume have been thoroughly revised and brought up to date, and when necessary extended and re-arranged;" yet the only reference to the important and increasing application of photography to the delineation of nebulæ is that in the case of the nebula in Andromeda: "Mr. I. Roberts has recently obtained photographs of this object which seem to combine the features exhibited by Sir J. Herschel in the engraving appended to his 'Outlines of Astronomy,' with the rifts recorded by Bond."

The curtness with which Mr. Chambers disposes of the long-exposure photographs which mark an era in the progress of astronomy is lamentable, and the comparison of them with previous observations is misleading, for the features shown in the engraving at the end of Sir J. Herschel's "Outlines" were never observed; and if Mr. Chambers has seen the photograph he must have noticed that Bond's rifts are considerably extended, and appear as divisions between masses of nebulous matter sweeping round the nucleus. At any rate a person who had not seen the photograph would scarcely be able to appreciate its beauty from the description.

We do not, of course, wish to say that, since photography has so considerably extended our knowledge of the structure of celestial species, all drawings of them should be discarded. The photographic plate only adds to their value because, by a cumulative effect, it grasps and renders manifest faint light which the eye alone can never appreciate; but this is such an important development that the hand-book in which nebulæ are described and their forms dilated upon without giving it full consideration must be stigmatized as terribly incomplete.

Again, the selection of drawings of nebulæ which Mr. Chambers made for the first edition of this work in 1867, and which is still retained, is not a happy one by the common consent of all observers; and we should have supposed that, since many elementary text-books contain reproductions of some of the photographs of nebulæ, a work of such pretensions as this, in which drawings of nebulæ may be counted by the score, would have had at least one photographic representation of their form to enrich its pages.

Also, with respect to the nebula in Andromeda, Mr. Chambers records: "Huggins has noticed the spectrum to be continuous (though cut off at the red end), and therefore, whatever it is seemingly, it is *not gaseous*." That the spectrum was observed by Dr. Huggins in 1864 to be crossed "evidently either by lines of absorption or by bright lines," and that it has been shown to have the same spectrum as that of a comet at a mean distance from the sun, are matters with which Mr. Chambers is apparently not acquainted. It is good to see it asserted, in an italicized expression however, that the nebula does not consist of gaseous matter.

Following the chapters devoted to star clusters, nebulæ, and the Milky Way, and making up the greater portion of the work, we find catalogues of naked-eye, red, variable, and binary stars, which may be found useful. The indexes to both volumes leave much to be desired; indeed, the author notes that they are not complete by themselves, and are designed for use in connection with the table of contents. The disadvantages of this division are obvious, since reference is rendered unnecessarily difficult, a circumstance which, in the eyes of those accustomed to use works of this character, detracts considerably from its merit. At the end of the third volume a general index to the whole work is inserted which is said to be comprehensive. In this we find the names given of all the minor planets, although in the vast majority of cases the cognomen of these unimportant bodies is only known to the discoverer, and to index them is an utter waste of space. The principle, however, of including what might have been omitted and of omitting what should have been included, seems to have been followed by Mr. Chambers through each of the three volumes. We should advise, therefore, that in the case of a future edition a more careful consideration of what constitutes astronomical progress should be made. If this were done, and the facts were arranged in a rather better order, the compilation would be more useful as a hand-book of astronomy.

ANNALS OF THE MUSEUM OF BUENOS AYRES.

Annales del Museo Nacional de Buenos Aires para dar a conocer los objectos de historia natural nuivos ó poco conocidos conservados en este establecimento. Por German Burmeister, Med. Dr., Phil. Dr., Director del Museo Nacional de Buenos Aires. Entrega decimasexta. (Buenos Aires, 1890.)

THE veteran man of science, Dr. H. Burmeister, of Buenos Ayres, continues to issue the "Annals" of the Museum under his charge with unfailing regularity, and now sends us a copy of the 16th part of this excellent serial. Upon the present occasion he deserts for a while his favourite subject of the fossil animals, which the Argentine Tertiaries produce in such countless abundance and of so strange a character, and gives us an account of a scientific expedition into Patagonia, recently carried out by his son, Sr. Carlos V. Burmeister, one of the assistant naturalists of the Museum.

The scientific staff of the expedition to Patagonia left Buenos Ayres in November 1888, and proceeded by railway to Bahia Blanca, and thence by diligence to Carmen on the Rio Negro, where the rest of the party was assembled. The next point attained was Trelew, the chief town of the Welsh settlement on the Rio Chubut, which is now connected by a railway, 70 kilometres in length, with Port Madryn on the Atlantic. By this route, various additional stores, forwarded direct from Buenos Ayres by steamer, were received, and the Expedition, being fully equipped, finally started for the interior of Patagonia on January 9, 1889. The route taken was up the valley of the Chubut until its junction with its tributary, the Rio Chico, whence the latter was followed to its source in the great Lake Colhue. Although the country surrounding this sheet of water is now utterly devoid of trees of any sort, this was certainly not the case in past times, as

enormous trunks of fossil trees were observed on the shores of the lake. From Lake Colhue the Rio Singuer which flows into it was ascended, until a point was reached where this stream takes an abrupt bend to the north-west. Thence the route lay for many days through the unknown uplands of the interior, until the upper waters of the Rio Chico de Santa Cruz were struck in lat. 48° 55′ 15″ S., on the last day of February. Descending the Rio Chico de Santa Cruz, the Expedition reached Beagle Bluff at the mouth of the great Santa Cruz, on March 9. Beagle Bluff, we may remind our readers, was so named from H.M.S. *Beagle*, which visited the spot in 1834, and first explored the River Santa Cruz. Darwin, who accompanied the boats of the *Beagle* in their survey of this stream, came to the conclusion that the river-valley of the Santa Cruz was formerly a strait dividing South America right across at this point, like the Straits of Magellan do now further south (see Darwin's "Naturalist's Voyage," chap. ix.).

The interior of Patagonia traversed by Sr. Burmeister's Expedition appears to be almost deserted at the present time. No natives seem to have been met with between the Chubut and the Rio Chico de Santa Cruz until the lower part of that river was reached.

From Port Santa Cruz the Expedition returned northwards along the Atlantic coast to Port Deseado in lat. 47° 56′, and thence, ascending the river of the same name, rejoined their former route on the Rio Singuer.

Besides the accurate survey made during the expedition, a large number of photographic views were taken, a selection of which will be published subsequently. These will be of interest in connection with the question of the origin of the singular "basaltic terraces" of this country, of which Darwin gave us the first indication, and which are frequently referred to by Señor Burmeister. Large collections were also made in natural history, most of which await further examination. But articles on the mammals and birds obtained during the expedition are appended to the present Report. Most of these are referred to species already fairly well known, although an exception must be made in favour of *Canis griseus*—the smaller of the two native foxes of Patagonia, of which little, if anything, has been recorded since its accurate description by Dr. H. Burmeister was published some years ago. The remaining collections still to be worked out will probably be found to contain objects of greater rarity; but there can be no doubt that the Patagonian fauna, though of great interest, is rather meagre.

OUR BOOK SHELF.

The Triumph of Philosophy. By James Gillespie (Ealing: West Middlesex Printing and Publishing Co. 1890.)

The author has endeavoured to correct the Copernican theory of astronomy, and propounds instead the Gillespian or true system of the universe, which asserts that the earth, as well as the sun, is fixed in space and all the stars revolve round it in a year.

One of the objections to the present arrangement reads as follows:—

"Can any man in his sober senses believe that the earth could fly through space at the rate of 1000 miles a minute. Would it not drive all the atmosphere either away from the earth or like the tail of a comet? Could the moon keep her constant path round the earth at 273,000 miles distant, if she (the earth) was flying at this terrific speed?"

To understand this argument, it is necessary to believe with Mr. Gillespie that gravitation has nothing to do with the motions of any of the heavenly bodies. In his words:

"I admit gravitation on the earth, but it only extends a certain distance from the earth, and it is quite powerless at the moon's distance, otherwise the moon—if she has weight at all—would fall crash on to the earth."

The greater portion of the work is taken up with observations of Mr. J. B. Dimbleby, of the British Astronomical Society (*sic*). This gentleman, whose genius seems shrouded in obscurity, is styled "Transit Medallist, Professor of Chronology, first calculator of all eclipses and transits from Adam, and the discoverer of five lines of astronomical time." We give a short extract, in which some of his researches are referred to:—

"He has proved, by a long and by a true calculation, that the earth, the sun, the moon, Mercury, and Venus were all in one direct line at creation, and it is almost likely that the other planets were in the same position, and there they would stand like a team of racehorses till the Divine signal was given, and off they went each on his own course; and it has been proved by eclipses and transits, ancient and modern, that they have not varied a single minute since that great day."

It will be readily understood that to try to convince Mr. Gillespie of the unsoundness of his arguments would be the height of absurdity, since he has not even an elementary knowledge of physical laws. As in all similar productions, strong words and hearty abuse are indulged in to patch up weakness of argument; no one is disturbed by the tirade, however, and the Gillespian doctrine of the universe will doubtless pass away with Mr. Dimbleby and its originator.

Watch and Clock Making in 1889. By J. Trippling, F.R.A.S., &c. (London: Crosby Lockwood and Son, 1890.)

This little book consists of an account and comparison of the exhibits in the horological section of the French International Exhibition.

In England there is very little literature on this subject, but on the Continent, and in France especially, a great many works on it have been published. The chief textbook is that by M. Saunier, who has done much towards the elevation of the social position of watchmakers, and whose books are the standard works of reference on the Continent.

Twelve technical schools competed against one another at the Exhibition; great importance being attached to the technical teaching of this class of subject abroad. An excellent programme of the work which is done during the student's course is given by the author, and shows the method of teaching that is adopted.

Chronograph makers are next dealt with; of these there were twenty representatives, four being English. For performance, finish, and the number of instruments produced, England was awarded the palm. The tests which instruments of this kind have to undergo are more severe in England than in Switzerland, owing to the greater variation in temperature. For instance, one English chronometer went for twenty-eight weeks with a variation never exceeding 1·4 seconds; while a Swiss chronometer, cited as being an exceptionally good one, varied as much as 2·2 seconds in two weeks.

The next section treats of the manufacture of watches, and in this one hundred and fifty firms exhibited. This number was divided into two classes—"factory system" and "garret system"; the former consisting of those who manufactured them by using steam and hydraulic power

for the output of the whole or part of the watch on the interchangeable system, the latter of those who still kept to the old mode of making them "under a system of sub-letting to small makers who work at their own homes."

Messrs. Rotheram and Sons, of Coventry, about the oldest and the largest firm of watch manufactures in England, headed the list, and seem to have had a fine display, theirs being one of the most striking exhibits in the Exhibition.

In the remaining pages the author gives an account of the merits and exhibits in the manufacture of clocks, turret clocks, tools, watch-cases, &c., concluding with a short summary.

On the whole, the British section seems to have fared very well, and to have held its own against foreign competition, and to those interested in the subject this work will afford a good insight into the present condition of watch and clock making.

The Harpur Euclid. Books V., VI., and XI. By E. M. Langley, M.A., and W. Seys-Phillips, M.A. (London: Rivingtons, 1890.)

THIS is an edition of Euclid's Elements revised in accordance with the reports of the Cambridge Board of Mathematical Studies and the Oxford Board of the Faculty of Natural Science.

The books dealt with are V., VI., and XI.

In most of the works on this subject Book V. is generally omitted, and only the definitions are learnt; but the authors have thought it advisable for the reader to acquaint himself with the terms used and with some of the theorems which are established in it. Although he is allowed to use these theorems as axioms, proofs are given depending on the definitions, the notation used being that recommended by De Morgan and adopted by the Association for the Improvement of Geometrical Teaching in its Syllabus and Elements.

Preceding Book XI. is a good and well worked out series of propositions on loci, harmonic division, similarity, maxima and minima; and a few miscellaneous problems, such as the nine-point circle, &c.

The proofs in Book XI. differ slightly from those ordinarily given in text-books, but are made shorter and perhaps clearer by the adoption of symbols.

The method throughout of dotting all construction lines is a great help to the reader, and is to be heartily recommended, the figures in Book XI. showing this off to advantage.

A large number of exercises are given here and there for the student to practise his ingenuity on.

The International Annual of Anthony's Photographic Bulletin, 1890-91. Edited by W. Jerome-Harrison and A. H. Elliott. (London: Iliffe and Son, 1890.)

THIS is the third volume that has been published of this most interesting Bulletin, and, glancing through its pages, we conclude that it is one of the best publications of its kind that we have come across. The articles, written in great part by men of acknowledged ability, contain a large amount of useful knowledge, forming a store of information from which workers in every rank of the art may obtain something that will interest them.

One of the chief features of the volume is the great increase in number of illustrations, which are printed by the various kinds of processes now available, and which show the advancement made in the application of photography for purposes of illustration.

The usual collection of tables is presented at the end. Among them may be mentioned Dr. Woodman's table of view angles, tables for the simplification of emulsion calculations, and tables of comparative exposures. The work concludes with a revised list of the Photographic Societies of the British Isles, British Colonies, America, and most of those on the Continent.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Discharge of Electricity through Gases.

IN the Bakerian lecture on "The Discharge of Electricity through Gases," in the last number of the Proceedings of the Royal Society, Prof. Schuster says:—"I do not see how the insulating power of air at the ordinary temperature is consistent with the presence of ions, however few in number, for ultimately a diffusion to the electrodes and a discharge would necessarily take place. This seems to me to be fatal to J. J. Thomson's view of the disruptive discharge."

This statement implies a misconception of the theory of the electric discharge advanced by me in the *Philosophical Magazine*, June 1883, for there is nothing in the theory of the discharge there given which makes the presence of free ions in air at ordinary temperatures and pressures essential. I will quote two sentences from the paper to show what the theory is:—"In order to make the spark pass through an elementary gas, we have to decompose the molecules into atoms. Thus the stronger the connection between the atoms in the molecules, the greater the electric strength." "Chemical decomposition is not to be considered merely as an accidental attendant on the electrical discharge, but as an essential feature of the discharge, without which it could not occur."

The misconception has, no doubt, arisen from my using in the same paper the Clausius-Williamson hypothesis of the interchange of atoms among the molecules to account for the difference of pressure in different directions in the electric field. But this hypothesis is not essential to the theory of the *discharge* given in the paper, for on that theory the discharge does not take place until *ordinary* dissociation of the molecules is produced by the electric field. The existence or non-existence of the quasi-dissociation of the Clausius-Williamson hypothesis which does not produce any chemical effects, does not affect the theory of the discharge, though it does that of the inequality of pressure.

J. J. THOMSON.

Cambridge, July 19.

Birds and Flowers.

IN your note on Mr. G. F. Scott-Elliot's paper on this subject (NATURE, July 17, p. 279) you remark: "In accordance with the view of Darwin, but opposed to that of Wallace, Mr. Scott-Elliot believes that the identity of colour (an unusual shade of red) in the majority of ornithophilous flowers and on the breasts of species of *Cinnyris* is an important element in pollination by birds." There must be, I think, some misapprehension here. I am not aware that Darwin has anywhere referred to the colours of birds as being generally similar to those of the flowers they frequent. Mr. Grant Allen has done so in his work on "The Colour-Sense," and I have opposed his views in NATURE (vol. xix. p. 501), because he founds the resemblance on the theory of sexual selection, and because the facts do not support any such general relation. That such a relation does sometimes occur I have shown, by quoting Mrs. Barber in my "Darwinism" (p. 201) as to the scarlet and purple colours of a sun-bird being highly protective when feeding among the similarly coloured blossoms of the *Erythrina caffra*, which, at the time, has no foliage. I have also called attention (in the same work, p. 319) to the numerous flowers now known to be fertilized by birds, and to the numerous large tubular flowers of a red and orange colour in Chile and the Andes, which are apparently adapted to be fertilized by humming-birds. The general uniformity of colour would be advantageous as an indication of bird-flowers as distinguished from insect-flowers; but there is no similarity to the colours of the birds. Curiously enough, the common Chilian Eustephanus is green-coloured in both sexes, while its close ally in Juan Fernandez is red in the male. Yet the flowers it frequents in the island are not red, but mostly white and yellow (see "Tropical Nature," p. 272). It is evident, therefore, that the prevalent colours of the flowers do not determine the colours of the birds which frequent them, unless those colours are so predominant that a similar colour becomes protective, as is more generally the case in the scantily-wooded plains of South Africa than anywhere else.

ALFRED R. WALLACE.

Reduplication of Seasonal Growth.

From time to time instances of this in the case of foliage have been recorded by correspondents in the pages of Nature. This year I have noticed not only an unusually early appearance of this in the development of new foliage-laden twigs, as in former years in the oaks, the hornbeams, the elms, and other forest trees; but, what is more rare with this somewhat exceptional summer, the fruit-trees seem to be expending their reserve energy in a second season of *flowering*. At this moment an apple-tree in my garden presents the curious sight of apple-blossoms side by side with apples more than half-grown, and a rowan-tree laden with nearly ripe fruit has a corymb of flowers on one of its higher boughs. The plum-trees have presented similar abnormal phenomena within the last week or two. The facts are of interest as pointing to considerable interference with the normal cycle of functional change by variations in environment.

Wellington College, Berks, July 18. A. Irving.

Chimpanzees and Dwarfs in Central Africa.

Perhaps Mr. Stanley or Surgeon Parke, if applied to, could throw some more light on the extraordinary statement made by Emin Pasha, recently referred to, which, if it be true, is the most important statement in the whole book.

It is probable that when Emin Pasha witnessed the torch-bearers, whether chimpanzees or young negroes or dwarfs, he was not alone, and, even though very short-sighted, he would have been able to verify his observation of the torch-bearing animals by reference to those near him. An experienced naturalist like Emin Pasha is not likely to have made the mistake Prof. Romanes thinks he did make—but it is possible.

Bearing in mind that a large ape is now undoubtedly acting as a signalman (under direction) on a railway at Natal, who can say what the limits of intelligence are in the tribes of Simians?

J. F.

The Perseid Meteors.

According to Mr. Denning, the radiant of the famous Perseid meteor-shower (which, in his opinion, commences early in July) shifts night after night until August 20, the principal change being an increasing R.A. The declination also increases, but more slowly.

I have some reason to think that the true explanation of the phenomena is that there are several radiants almost simultaneously in action, but which do not attain their maxima at the same data. For this reason I would ask those of your readers who are interested in the subject to watch these meteors carefully on the present occasion. W. H. S. Monck.

Dublin, July 15.

P.S.—Mr. Denning's Catalogue in *Monthly Notices* for May suggests to me the existence of four radiants (each of some continuance) whose approximate positions are $6° + 52°$, $20° + 57°$, $32° + 53°$, and $44° + 56°$.

"Wind Avalanches."

Some of the readers of Dr. Pernter's paper, "A Winter Expedition to the Sonnblick," may perhaps be interested by the following extracts from the *Alpine Journal* of June 1864. They are taken from a painfully interesting paper by Mr. Gosset, describing a fatal accident on the Haut-de-Cry in February of that year.

A party of six were crossing a wide *couloir*, "about 150 feet broad at the top and 400 or 500 at the bottom." The actual fall of the avalanche is thus described:—"Bennen advanced; he had made but a few steps when we heard a deep, cutting sound. The snow-field split in two about 14 or 15 feet above us. The cleft was at first quite narrow—not more than an inch broad. An awful silence ensued . . . broken by Bennen's voice: 'Wir sind alle verloren.' . . They were his last words. I drove my alpenstock into the snow, and brought the weight of my body to bear on it. . . . I turned my head to see whether Bennen had done the same thing. To my astonishment, I saw him turn round, face the valley, and stretch out both arms. (So in Dr. Pernter's paper, "Their advice is to throw oneself prostrate, with hands outstretched.") The ground on which we stood began to move slowly, and I felt the utter uselessness of any alpenstock. I soon sank up to my shoulders, and began descending backwards. . . . The speed of the avalanche increased rapidly, and before long I was covered up with snow. I was suffocating when I suddenly came to the surface again. I was on a wave of the avalanche, and saw it before me as I was carried down. . . . The head alone was preceded by a thick cloud of snow-dust; the rest of the avalanche was clear. Around me I heard the horrid hissing of the snow, and far before me the thundering of the foremost part of the avalanche. . . . At last I noticed that I was moving slower; then I saw the pieces of snow in front of me stop at some yards' distance; then the snow straight before me stopped. . . . I felt that I also had stopped, . . . but the snow behind me was still in motion; its pressure on my body was so strong, that I thought I should be crushed to death."

Mr. Gosset further remarks:—"The upper stratum of snow was eleven days old. . . . The snow was thawing, and the whole snow-field in a state of uncertain equilibrium. By cutting through the snow at the top of the *couloir* we cut one of the main points by which the snow of the two different layers held together. . . . The avalanche may have taken a minute to descend; I can give no correct estimation on this point."

The vividness of the above description, and its complete accord with Herr Rojacher's account given in Dr. Pernter's paper, will, I hope, excuse the length of the extracts.

Otham, Maidstone. F. M. Millard.

ON THE METEOROLOGICAL CONDITIONS OF DESERT REGIONS, WITH SPECIAL REFERENCE TO THE SAHARA.[1]

The arid regions of the world are, speaking roughly, distributed in two bands north and south of the equator. They comprehend all inland drainage areas, or areas where the streams have no connection with the sea, which are also regions where evaporation is in excess of precipitation, for if the latter were in excess the water would rise till it could flow into the sea, as in the case of the great lake region of North America, and the area would no longer be one of inland drainage. The largest of the deserts, the Sahara, is about $3\frac{1}{2}$ million square miles in area, and the area of all the deserts of the world together about 11,500,000 square miles. In other words, over one-fifth of the land of the world has no outlet for drainage to the sea, and in all that area evaporation is greater than precipitation. These areas correspond very closely with the regions of the world where the rainfall is less than 10 inches annually.

In no place in the world can there be found such enormous ranges of temperature as in these deserts. In the Sahara the temperature sometimes falls from 100° during the day to the freezing-point during the night, due to the great dryness of the atmosphere and to the radiation that takes place from the soil after the sun has set. These inland drainage areas correspond very much in their barometric phenomena. In all desert regions during summer all the winds blow in upon them. In winter the reverse takes place—the winds flow out of them, and that holds good both for the northern and the southern hemispheres. This occasions the low rainfall, for the great majority of these regions are more or less bounded by high hills. The winds arrive at the deserts over these hills, and the vapour is precipitated from the atmosphere by the hills, with the result that when the winds reach the interior regions there is nothing left to be deposited. If there are not hills all round any desert area, then, as in the case of Northern Asia, the winds pass from a colder to a warmer climate, and as they get to warmer regions they are able to contain more vapour, and, consequently, no rain is precipitated.

The author then gave an account of his own views and impressions as to the Sahara. When staying in May last in Algeria, he was anxious to make a trip to the desert, principally with the object of examining the sand and other deposits. During the *Challenger* expedition they had found in the bed of the Atlantic for a long distance

[1] Abstract of a Paper read by Dr. John Murray at the meeting of the Scottish Meteorological Society held in Edinburgh on July 14.

west of the African coast opposite the Sahara, and in the bed of the Indian Ocean to the south of Australia, small grains of red quartz sand, and they had found scarcely a trace of such in the sea-bed in any other part of the world. He suspected this quartz sand had been blown out from the Sahara in the one case, and from the Australian desert in the other.

In the south of Algeria he got a light carriage which could traverse the desert, such as was now in use for the post just established by the French to Tougourt, in the Sahara. Taking bedding and food with him, he first skirted a large area covered with salt, and then passed on through the long belt of oases which the French have planted on the way to Tougourt. Along this route numerous artesian wells had been sunk, and an abundant supply of water thereby obtained for the palm-trees which had been planted. There were now three companies in existence, who had dug artesian wells, and were planting thousands of palm-trees, with the view of getting a valuable return in a few years.

At Tougourt the real sandy part of the desert began, and he made excursions into it, with that town as his head-quarters. He exhibited to the meeting a specimen of the sand, of a light yellowish-brown colour, and exceedingly fine in the grains. There were a good many clay particles in it, and the quartz particles, which were also numerous, were identical with those they had got in the bottom of the Atlantic. There was no doubt that the winds from the desert carried the sand a long way out to sea. He had also examined the region geologically, and the formation of the rocks was entirely that of fresh water, and of Quaternary date.

The great majority of geographers and geologists had expressed the belief that the Sahara was an old sea-bed, but he was of opinion that it had never as a whole been covered by the sea since Cretaceous or Devonian times, and no part of it had been covered by the ocean since Tertiary times. All the assertions as to the discovery of shells rested upon one common species being found very rarely in one region of the desert. He thought that, owing to recent researches, the opinion as to the Sahara being an old sea-bottom was likely soon to disappear from our text-books. He considered that the features of the region had been produced by atmospheric conditions. The sand was the product of the disintegration of the rocks *in situ*, which engirdle the Sahara. The existing rock was not far below the surface, and, by digging down to it, the hard sandy particles were found embedded in the stone. The sun shone on the rocks, and they expanded. The sudden cooling at night broke them up, the wind carried away the smaller particles, and so continually the rocks were being disintegrated by means of changes other than water, although water perhaps had in times past played a greater *rôle* there than it did now.

There was a range of hills in the desert to the south 7000 feet high, and for three months in the year their summits were covered with snow. Descending the hills were river-courses, some of great length. Much of the region, he considered, had once been a large fresh-water lake. Speaking of the commercial aspect of the Sahara, he said it was difficult to go there without becoming enthusiastic about it. There seemed to be no limit to the amount of water that was to be got by sinking artesian wells. The head of the water must be a long distance away in the higher lands surrounding the desert.

The cultivation of palms was extending to an enormous extent, and the French expected to carry on their railway to Tougourt (at present nearly a week's journey from Algeria) in the next few years. The French were also hopeful that France would tap all the trade of the North Soudan across the Sahara, by making a railway across the desert. He did not think it was at all impossible to build and keep open such a railway. There was plenty of water to be had, and the sand never drifted to such an extent as to bury a railway. The climate, though very warm, was at the same time very healthy. If the French built the railway, they would then have no cause to complain about Britain remaining in Egypt.

WILLIAM KITCHEN PARKER, F.R.S.

WILLIAM KITCHEN PARKER was born at Dogsthorpe, near Peterborough, June 23, 1823, and died suddenly, of syncope of the heart, July 3, 1890. He was visiting his second son, Prof. W. N. Parker, at Cardiff, and, whilst cheerfully talking of late discoveries and future work in his favourite biological pursuits, he ceased to breathe. Accustomed to outdoor life, he was a true lover of Nature from the first; the forms, habits, and songs of birds, especially, he knew at an early age. Village schooling at Dogsthorpe and Werrington, and a short period at Peterborough Grammar School, prepared him for an apprenticeship, at 15 years of age, to Mr. Woodroffe, chemist and druggist at Stamford; and three years afterwards he was apprenticed to Mr. Costal, medical practitioner, at Market-Overton. At Stamford he studied botany earnestly, and used to persuade a fellow-apprentice to leave his bed in early mornings to go afield in search of plants. Both when living at his father's farm, and in his holidays afterwards, he kept many pet animals, and dissected whatever he could get, including a donkey and many birds. Of the latter he prepared skeletons; and of these he made many large drawings, at Market-Overton, which of late years he had some thought of publishing as an atlas of the osteology of birds. In 1844–46 he studied at King's College, London; and became student-demonstrator to Dr. Todd and Mr. (now Sir William) Bowman there. He also attended at Charing Cross Hospital in 1846 and 1847, and, having qualified as L.S.A., he commenced practice, in 1849, at Tachbrook Street, Pimlico; and soon afterwards married Miss Elizabeth Jeffery. His wife's patient calmness under all difficulties and trials was a true blessing to a man of Mr. Parker's excitable temperament; and her unselfish life and widespread influence for good are well known in and beyond the family circle. Unfortunately he was left a widower about four months ago. His family consists of three daughters and four sons. Of the latter, one is Professor of Zoology and Comparative Anatomy in the University of Otago, New Zealand; the second is Professor of Biology in the University College at Cardiff, South Wales; the third is an able draughtsman and lithographer; and the fourth has lately taken his diplomas of L.R.C.P. and M.R.C.S.

Mr. Parker had a good father, courteous and gentle by nature, conscientious, and earnest in business, who had worked hard to be able to give even his youngest son, Mr. W. K. Parker, "a start in life." From his placid and thoughtful mother he probably inherited much of his love of reading and his talent for learning.

Always energetic, in spite of constant ill-health, Mr. Parker enthusiastically carried on his medical work and his natural-history studies, especially in the microscopic structure of animal and vegetable tissues. Polyzoa and Foraminifera, collected on a visit to Bognor, and from among sponge-sand and Indian sea-shells, especially attracted his attention. Having sorted, mounted, and drawn numbers of these microzoa, he was induced, about 1856, by his friends W. Crawford Williamson and T. Rupert Jones, to work at the Foraminifera systematically. His paper on the *Miliolitidæ* of the Indian Seas (Trans. Micros. Soc., 1858), and a joint paper (with T. R. Jones) on the Foraminifera of the Norwegian coast (*Annals N. H.*, 1857) resulted; and the latter formed the basis of a memoir on the Arctic and North-Atlantic Foraminifera (Phil. Trans., 1865). With T. Rupert Jones, and after-

wards with W. B. Carpenter and H. B. Brady, Mr. Parker, down to 1873, described and illustrated many groups and species of Foraminifera, recent and fossil (see C. D. Sherborn's "Bibliography of Foraminifera" for these papers and memoirs), thereby establishing more accurately a natural classification of these microzoa, determining their bathymetrical conditions, and therefore their value in geology. That he did not neglect anatomical research is shown by memoirs in the Proceedings and Transactions of the Zoological Society on the osteology (chiefly cranial) and systematic position of Balæniceps (1860), Pterocles (1862), Palamedea (1863), Gallinaceous Birds and Tinamous (1862 and 1866), Kagu (1864 and 1869), Ostriches (1864), Microglossa (1865), Common Fowl (1869), Eel (NATURE, 1871), skull of Frog (1871), of Crow (1872), Salmon, Tit, Sparrow-hawk, Thrushes, Sturgeon, and Pig (1873). In the meantime the Ray Society had brought out his valuable "Monograph on the structure and development of the Shoulder-girdle and Sternum in the Vertebrata" (1868); and his Presidential addresses to the Royal Microscopical Society (1872, 1873), and notes on the Archæopteryx (1864), and the fossil Bird bones from the Zebbug Cave, Malta (1865 and 1869), had been published. Subsequently the Royal Society's Transactions contained his abundantly illustrated memoirs on the skull of the Batrachia (1878 and 1880), of the Urodelous Amphibia (1877), the Common Snake (1878), Sturgeon (1882), Lepidosteus (1882), Edentata (1886), Insectivora (1886), and his elaborate memoir on the development of the wing of the Common Fowl (1888). In the "Reports of the *Challenger*" is his memoir on the Green Turtle (1880); and those on Tarsipes (Dundee, 1889), and the Duck and the Auk (Dublin, 1890), are his last works.

In former times a skull was taken as little more than a dry, symmetrical, bony structure; or, if it were the cartilaginous brain-case of a shark, it was to most a mere dried museum specimen. When, however, the gradations of the elements of the skull, from embryonic beginnings, were traced until their mutual relations and their homologues in other Vertebrates were established, light was thrown on the wonderful completeness of organic uniformity and singleness of design. How such studies can be carried on both by minute dissection and the modern art of parallel slicing, and not by one method alone, is to be gathered from his teaching.

Mr. Parker was elected a Fellow of the Royal Society in 1865, and in the year following he received a Royal Medal for his comprehensive, exact, and useful researches in the developmental osteology, or embryonal morphology, of Vertebrates. Some few years afterwards the Royal Society gave him an annual grant to aid in the prosecution of his studies; and, when that was discontinued, a pension from the Crown was graciously and appropriately awarded to him. A generous friend, belonging to a well-known Wesleyan family, more than once presented £100 towards the cost of some of the numerous plates illustrating his grand memoirs in the Philosophical Transactions.

In 1873 he received the diploma as Member of the Royal College of Surgeons, and was appointed Hunterian Professor, Prof. Flower being invalided for a time; and afterwards both held the Professorship conjointly. His earnestness and wide views were well appreciated, opening up the modern aspect of comparative anatomy, and showing that both in Man and the lower Vertebrates the wonderful structural development of their bony framework should be studied in a strictly morphological rather than a teleological method, and that its stages and resultant forms could be regarded only in the Darwinian aspect.

These lectures, given in abstract in the medical journals, became the basis of his "Morphology of the Skull," in writing which, from his dictation and notes, Mr. G. T. Bettany kindly assisted him; and again, in a semi-popular book, "On Mammalian Descent," another friend (Miss Arabella Buckley, now Mrs. Fisher) similarly helped him. In the latter work, his own usual style frequently predominates, full of metaphor and quaint allusions, originating in his imaginative and indeed poetic mind, fully impregnated with ideas and expressions frequent in his favourite and much-read books—Shakespeare, Bacon, Milton, some of the old divines, and, above all, the old English Bible.

Separating himself from the trammels of foregone conclusions, and from the formulated, but imperfect, misleading conceptions of some of his predecessors in Biology, whom he left for the teaching of Rathke, Gegenbaur, and Huxley, Prof. W. K. Parker earnestly inculcated the necessity of single-sighted research, and the following up of any unbiassed elucidations, to whatever natural conclusion they may lead. Simple and firm in Christian faith, resolute in scientific research, he felt free from dread of any real collision between science and religion. He insisted that "our proper work is not that of straining our too feeble faculties at system-building, but humble and patient attention to what Nature herself teaches, comparing actual things with actual" (Proc. Zool. Soc., 1864); and in his "Shoulder-girdle, &c.," p. 2, he writes: "Then, in the times to come, when we have 'prepared our work without, and made it fit for ourselves in the field,' we shall be able to build a 'system of anatomy' which shall truly represent Nature, and not be a mere reflection of the mind of one of her talented observers."

Again, at p. 225, in illustration of some results of his work, he says:—"The first instance I have given of the Shoulder-girdle (in the Skate) may be compared to a clay model in its first stage, or to the heavy oaken furniture of our forefathers, that 'stood pond'rous and fixed by its own massy weight.' As we ascend the vertebrate scale, the mass becomes more elegant, more subdivided, and more metamorphosed, until, in the Bird Class and among the Mammals, these parts form the framework of limbs than which nothing can be imagined more agile or more apt. So also, as it regards the Sternum; at first a mere outcropping of the feebly developed costal arches in the Amphibia, it becomes the keystone of perfect arches in the true Reptile; then the fulcrum of the exquisitely constructed organs of flight in the Bird; and, lastly, forms the mobile front-wall of the heaving chest of the highest Vertebrate."

Prof. W. K. Parker was a Fellow of the Royal, Linnean, Zoological, and Royal Microscopical Societies; Honorary Member of King's College, London, the Philosophical Society of Cambridge, and the Medical Chirurgical Society. He was also a Member of the Imperial Society of Naturalists of Moscow, and Corresponding Member of the Imperial Geological Institute of Vienna, and the Academy of Natural Sciences of Philadelphia. In 1885 he received from the Royal College of Physicians the Bayly Medal, "Ob physiologiam feliciter excultam."

In conversations shortly before his death, he often spoke of looking forward throughout his life-time (alas! how quickly shortened!) to continued application of all the energy he could devote to his useful work—at once a consolation to him and a duty.

He has well expressed his own view of biological pursuits, at p. 363 of the "Morphology of the Skull":—"The study of animal morphology leads to continually grander and more reverend views of creation and of a Creator. Each fresh advance shows us further fields for conquest, and at the same time deepens the conviction that, while results and secondary operations may be discovered by human intelligence, 'no man can find out the work that God maketh from the beginning to the end.' We live as in a twilight of knowledge, charged with revelations of order and beauty; we steadfastly look for a perfect light, which shall reveal perfect order and beauty."

An unworldly seeker after truth, and loved by all who

knew him for his uprightness, modesty, unselfishness, and generosity to fellow-workers, always helping young inquirers with specimens and information, he was suddenly lost to sight as a friend and father, but remains in the minds of fellow-workers, of those whom he so freely taught, and of his stricken relatives, as a great and good man, whose beneficent influence will ever be felt in a wide-spreading and advancing science, and among thoughtful and appreciative men in all time.

ALPHONSE FAVRE.

BY the death of Prof. A. Favre, Switzerland has been deprived of one of her foremost men of science, and geology has lost a very assiduous and successful cultivator. His death appears to sever the last remaining link between the present generation of Swiss geologists and that older and famous one which included Bernhard Studer, Arnold Escher von der Linth, Peter Merian, and Oswald Heer. The late Prof. Favre, who had reached the age of seventy-seven at the time of his death, was the author of numerous papers, the earliest of which, "On the Anthracites of the Alps," was published as long ago as 1841. He will perhaps be best remembered by the part he took in the famous controversy concerning the supposed admixture of fossils, belonging to different geological horizons, which were said to occur in the same beds in the Alps. In opposition to M. Scipion Gras and others who asserted that such intermixture of fossils did actually occur, Favre was able to show, by a series of patient investigations, that the apparent reversals of succession, and intimate union of Carboniferous, Jurassic, and Tertiary strata, could all be accounted for by repeated interfoldings and complicated overthrust faults. It is interesting to note that at the time when Favre was thus successfully contending for such an interpretation of supposed anomalies in the Alpine rocks, James Nicol in this country was engaged in a precisely similar controversy with Murchison and his followers, concerning the rocks of our own Highlands. But whereas the triumph of Favre's views was immediate and complete, and their author lived to see the justice of his interpretation universally admitted, Nicol was fated to witness the influence of great authority exerted for a long time in preventing the truth of his conclusions from being accepted; and only after his death was the retraction made which showed how much Scotland owes to this able interpreter of the geological structure of his native land. History may be relied upon, however, to do equal justice to the successful Swiss geologist and the disappointed Scotch one. Prof. Favre, besides papers on a great variety of geological questions, wrote several works dealing with the geology of the parts of Savoy, Piedmont, and Switzerland of which Mont Blanc forms the centre. During the later years of his life he had retired from his Professorship of Geology at Geneva, but up to the time of his death Favre held the post of President of the Federal Commission having charge of the geological map of Switzerland. As long ago as 1874 he was elected a foreign member of the Geological Society, and he was also a correspondent of the Institute of France.

AID TO ASTRONOMICAL RESEARCH.

PROF. PICKERING, of the Harvard College Observatory, has issued the following notice:—

"Miss C. W. Bruce offers the sum of six thousand dollars ($6000) during the present year in aiding astronomical research. No restriction will be made likely to limit the usefulness of this gift. In the hope of making it of the greatest benefit to science, the entire sum will be divided, and in general the amount devoted to a single object will not exceed five hundred dollars ($500). Precedence will be given to institutions and individuals whose work is already known through their publications, also to those cases which cannot otherwise be provided for, or where additional sums can be secured if a part of the cost is furnished. Applications are invited from astronomers of all countries, and should be made to the undersigned before October 1, 1890, giving complete information regarding the desired objects. Applications not acted on favourably will be regarded as confidential. The unrestricted character of this gift should insure many important results to science, if judiciously expended. In that case it is hoped that others will be encouraged to follow this example, and that eventually it may lead to securing the needed means for any astronomer who could so use it as to make a real advance in astronomical science. Any suggestions regarding the best way of fulfilling the objects of this circular will be gratefully received. "EDWARD C. PICKERING.

"Harvard College Observatory, Cambridge, Mass., U.S.A., July 15, 1890."

NOTES.

THE American Association for the Advancement of Science will meet this year at Indianapolis, under the presidency of Prof. Goodale. The first meeting will be held on August 19. The subject selected in advance for special discussion is "The Geographical Distribution of North American Plants," and papers upon it will be presented by Messrs. Watson, Macoun (of Ottawa), Sargent, Britton, Underwood, Halsted, and Coulter.

A ROYAL COMMISSION has been appointed to inquire and report "what is the effect, if any, of food derived from tuberculous animals on human health, and, if predjudicial, what are the circumstances and conditions with regard to the tuberculosis in the animal which produce that effect upon man." Lord Basing is chairman. The other Commissioners are Prof. G. T. Brown, Dr. George Buchanan, Mr. Frank Payne, and Prof. Burdon Sanderson.

THE Turin Academy of Medicine has proposed the following theme for the Riberi Prize of about £750: "Researches on the nature and the prophylaxis of one or several infectious diseases of man." Works may be sent printed or in manuscript; they may be in Italian, French, or Latin; and printed works must have appeared since 1886. The date limit is December 31, 1891.

THE failure of the Government to carry its scheme for the extinction of some public-house licences is likely to result in an important advantage to education. In his statement on Monday with respect to the money which was to have been applied to this object, Mr. Goschen said:—"As regards England we propose to add the amount set free by the abandoned licensing clauses to the residue which, under the Bill as it stands, goes to the county councils, accompanying this inclusion by an intimation that possibly new charges may, by and by, be put upon them, with reference to intermediate, technical, or agricultural education. It seems very desirable, if more is to be done in this respect, that the localities, and especially county councils, should be interested in the work. In England there is at present little machinery available for carrying out such an object, and it would be impossible to create such a machinery at this period of the session. But in Wales and in Monmouthshire the machinery does exist. County councils may supply funds to the joint committee for intermediate education under the Act of last year out of the county rate, but to the extent of a halfpenny of such rates only. We shall propose that the county councils in Wales

should have authority to increase the sum out of the additional funds now placed at their disposal. . . . As regards Ireland we shall propose that the £40,000 which falls to her share should be utilized for the further promotion of intermediate education, and for this purpose should be placed at the disposal of the Intermediate Education Board for Ireland, a body which, I believe, commands the confidence of the Irish public generally, irrespective of political and religious differences." The Government propose that the £50,000 which falls to the share of Scotland shall be handed over unconditionally to the county councils; but Mr. Campbell-Bannerman has given notice that he will move an amendment to the effect that the money be devoted directly to the completion of a scheme of free primary education.

THE Drapers' Company, London, has contributed £3000 towards the cost of the new buildings for technical instruction in connection with Nottingham University College. This branch of the College will be under the care of the recently-appointed Professor of Mechanical Engineering and Technology, Mr. William Robinson, late chief assistant at the City and Guilds Technical College, Finsbury.

A PUBLIC MEETING was held at the Town Hall, Kensington, yesterday, under the presidency of the Hon. and Rev. E. Carr Glynn, to consider measures whereby the technical and scientific education of apprentice and other plumbers may be ensured.

LAST week the Institute of Electrical Engineers held a series of meetings at Edinburgh in connection with the International Exhibition. The series began on Tuesday, when Dr. Hopkinson occupied the chair. Dr. Walmsley read a paper on some of the principal features of the Exhibition, in which he referred particularly to the telegraph and electric light apparatus and gas-engines. Mr. A. R. Bennett read a paper on "Foreign Currents in Telegraph and Telephone Lines." He described experiments he had carried out with overhead wires, and pointed out their effect in wet weather. Mr. W. H. Preece said that the foreign currents found in electric wires were far more readily perceptible in telephone than in telegraph wires. The currents were due often to the swing of the wires, and greatly to the alternating system of generating electric light recently introduced. Mr. Bennett said that disturbances might be caused by the introduction of electric tramways. In the evening the members of the Society attended a *conversazione* given in their honour in the grand hall of the Exhibition. On Wednesday, when the chair was taken by Mr. W. H. Preece, a paper on "The Working Efficiency of Secondary Cells" was read. This paper, of which we hope to give some account, was the joint work of Messrs. W. E. Ayrton, C. G. Lamb, E. W. Smith, and M. W. Woods. On Thursday, Mr. Spagnoletti was in the chair, and Mr. A. R. Bennett read a paper on "Experiments on Radiometry." Some discussion followed, in which Dr. Walmsley, Mr. Stroh, Mr. Fairfax, and others took part.

AT the instance of a number of Magdeburg manufacturers, an electro-technical experimental station is about to be founded in that town, to afford to companies or private persons opportunity of experimenting as to the practicability and cost of various electrical arrangements, and of testing machines, apparatus, &c. The station will be arranged on the pattern of one already in existence at Munich, but expanded in several directions. Dr. M. Krieg, editor of the *Electrotechnical Echo*, will be at its head. Among other matters which will come under consideration, are the examination of arrangements for illumination, transmission of force, and metallurgical purposes, determination of the luminous power of arc and glow lamps, and of constants, such as intensity and tension of current, testing of carbon rods, of measuring-instruments, accumulators, primary batteries, &c., examination of conducting and insulating materials, lightning conductors, private telephone arrangements, and so on. Youths devoting themselves to electro-technical work will have opportunities of gaining thorough practical knowledge in the place.

THE death of Mr. John Ralfs, at Penzance, on the 14th inst., at the age of 83, removes one of the last survivors of a past generation of botanists. His "British Desmidieæ," published in 1848, remains to the present time unsurpassed in botanical literature for the beauty and accuracy of its coloured plates. As it was the first British work (except Hassall's "British Fresh-water Algæ," published three years earlier) which did any justice to this beautiful class of fresh-water organisms, so it remained the only one until the appearance of Dr. Cooke's "British Desmids" in 1887. Mr. Ralfs also contributed several papers on the Mosses, Fungi, and Algæ of his native county to the Transactions of local scientific Societies. Of a retiring disposition, and practising as a surgeon in Penzance, he was but little known personally to his fellow-workers. Within the last two years he was elected an Honorary Fellow of the Royal Microscopical Society.

MR. G. W. RAFTER has contributed to the Transactions of the American Society of Civil Engineers an interesting paper on freshwater Algæ, and their relation to the purity of public water-supplies. He finds that a number of Algæ may assist in rendering drinking-water unpotable, producing a nauseous or "fishy" smell, generally due to the decomposition of their mucilaginous envelope, or of the starch or oil contained in their cells. In addition to the well-known Fungus or Schizomycete *Beggiatoa*, which has the remarkable property of withdrawing sulphur from sulphates in solution, the following freshwater Algæ are especially deleterious when occurring in large masses:—*Cladophora*, *Vaucheria*, *Batrachospermum*, *Draparnaldia*, *Chætophora*, *Volvox*, *Eudorina*, *Pandorina*, *Hydrodictyon*, *Palmella*, *Crenothrix*, *Oscillaria*, and diatoms generally, especially *Meridion circulare*. Desmids appear to be usually innocuous.

THE British Vice-Consul at Los Angeles, in California, in his last Report, has some observations on the vine and orange pests in that region. The vine-disease now seriously menaces the existence of the viticultural industry in the vicinity of Los Angeles. At first it attacked chiefly the "mission" vines; now, other varieties of red vines are dying, and the white varieties are also suffering. The disease first appeared in its present dangerous form in the southern part of California, and destroyed many vineyards. Prof. Dowlen, an expert employed by the Viticultural Commission to ascertain its cause, and, if possible, discover a remedy, inclines to the opinion that it is due to a fungus. On the other hand, Mr. Wheeler, Chief Executive Officer of the Viticultural Commission, reports that he is fully convinced that the fungus found on the dead vines is not the prime cause of their decadence, and that it attacks them only when they have been weakened by other causes. As to the Icerya, or "white scale," which has ravaged the orange-groves, the Vice-Consul says that a year ago many of the principal orange-growers in the vicinity of Los Angeles had abandoned their efforts to exterminate this pest, concluding that their trees must die. Fortunately, it was learned that an Australian parasite, the *Vedolia cardinalis*, had exterminated the white scale in Australia. A colony of the bugs was imported, and placed on the trees in an orchard in Los Angeles; they multiplied so rapidly that in a few months the scale was entirely exterminated in the district; many trees, which a year ago were nearly dead, have revived and borne half a crop this season.

ARTIFICIAL musk is a recent chemical achievement. A process for its production has been patented in Germany, the inventor being Herr A. Bauer, of Gisparsleben, in the Erfurt

district. It is a familiar experience in organic chemistry, that on introduction of nitro groups (NO_2) into organic bodies, by action of nitric acid, a smell like that of musk is often noticed. In the present case, pure butyl-toluol is treated with a mixture of sulphuric and nitric acid, and the nitro-compound is purified by crystallization from alcohol, the yellowish-white crystals smelling strongly like musk. According to Dr. Paul (*Humboldt*), the smell is not perfectly pure, and it can be distinguished from that of musk by the perfumer, but not by the general public. Curiously, a 1 per cent. alcoholic solution has not the smell of musk; only after dilution with water does this come out, and the dilution may be carried far before the smell is lost; with 1 in 5000 it is still quite distinct. Certain properties of the new product seem to render it very useful in the perfuming of soap.

THE small toe in man has recently (we learn from *Humboldt*) been made a subject of study by Herr Pfitzner. It is well known that thumbs and great toes are two-jointed, and the other fingers and toes generally three-jointed. In many human skeletons, however, the small toe is found to be two-jointed, the middle and end phalanges being fused into one piece, though still distinguishable. This variety occurs in about 36 per cent. of cases, and, as a rule, in both toes simultaneously; and there are more instances among women (41·5 per cent.) than among men (31·0 per cent.). One naturally thinks here of shoe-pressure causing union of two bones originally separate. But it appears that in children, from birth to the seventh year, and in embryos from the fifth month, the fusion occurs about as often as in adults. Further, the material of examination was not from a class of people who wear tight shoes. Herr Pfitzner concludes that the small toe in man is in course of degeneration (*Rückbildung*), and that without apparent adaptation to external mechanical influences. Processes of reduction are also observed in the connected muscular system. The question arises, Has the tendency reached its limit, or have we merely the first act of a total degeneration of the fifth toe? The author inclines to the latter view, but desires an extension of these researches among peoples who do not wear shoes or sandals, or have only of late begun to wear them. In living persons, it is not difficult to determine, by stretching and bending, whether the small toe is two- or three-jointed; and in this way adequate data might be had for determining any percentage differences in occurrence of the old and the new form in different races; also for investigating the inheritance of acquired characters, members of several successive generations being examined.

DEFECTIVE sight is becoming more general in the United States, and blindness, particularly among the poor, shows a steady growth. So says the British Consul at Philadelphia, whose statements are advanced on the authority of oculists. Purulent ophthalmia of infancy is prevalent in charitable institutions, poor-houses, &c. The disease shows itself within a fortnight after birth. A recent investigation of the blind in the country almshouses and asylums of an adjoining State showed that one out of every five cases of blindness was due to ophthalmia, and that the cases could have been cured if they had been properly treated in time. The disease is said to be contagious, and few or no special precautions have been taken in any of the institutions to prevent its spreading. The increase of the blindness throughout the country has been so marked of late years—four times as great as the increase of population—that it has been made the subject of special investigation by the American Ophthalmological Society, the investigation including a study of the ophthalmia so prevalent in Egypt, to which the ophthalmia of infancy is closely akin.

A VERY odd result of rivalry between two tiger-snakes is recorded by Mr. D. Le Souef, Assistant Director of the Melbourne Zoological Gardens, in the May number of the *Victorian Naturalist*. One of the snakes was large, the other small. Not long ago both happened to fasten on the same mouse, one at each end. Neither would give way, and the larger snake not only swallowed the mouse, but also the smaller snake. In about ten minutes nothing was seen of the smaller snake but about two inches of its tail, and that disappeared next day.

IN the new quarterly statement of the Palestine Exploration Fund, Mr. Flinders Petrie gives a short report of his recent excavations at Tell Hesy, in Palestine. These have proved to be remarkably interesting. The remains of Tell Hesy consist of a mound which is formed of successive towns, one on the ruins of another, and an enclosure taking in an area to the south and west of it. The lowest wall of all—28 feet 8 inches thick, and formed of clay bricks, unburnt—is believed to be that of Lachish, the ancient Amorite city, erected probably 1500 years B.C. Phœnician pottery of about 1100 B.C. is found above its level. Later constructions are the supposed wall of Rehoboam, and remains of the fortifications made in the reigns of Asa, Jehoshaphat, Uzziah, Jotham, and Manasseh. The pottery discovered on the spot is very valuable. "We now know for certain," says Mr. Petrie, "the characteristics of Amorite pottery, of earlier Jewish and of later Jewish influenced by Greek trade, and we can trace the importation and the influence of Phœnician pottery. In future all the tells and ruins of the country will at once reveal their age by the potsherds which cover them."

M. P. MÉGNIN is engaged in an elaborate study of the varieties of dogs. He has published two volumes on the subject, and a third is to appear shortly. The author tries to give an account of the origin of the varieties at present known.

HERR A. HARTLEBEN, of Vienna, Pest, and Leipzig, has begun the publication, in "Lieferungen," of two works which promise to be very good and useful. They are "Das Luftmeer," by Prof. F. Umlauft, and "Physik und Chemie," by Dr. von Urbanitzky and Dr. S. Zeisel. Both works are illustrated. The former will present an exposition of the principles of meteorology and climatology; the latter is to contain a general account of physical and chemical phenomena in their relation to practical life.

DURING the last few months a fortnightly *Meteorological Bulletin* has been published at Madrid, by a person under the *nom de plume* "Noherlesoom," professing to give the principal features of the weather for the coming fortnight, illustrated by isobaric charts for special days. Some pages of text contain extracts from various orthodox works bearing upon weather prediction. The present state of the science does not warrant predictions of this nature, nor is it stated upon what principles they are made; yet the weather predicted for the first half of July corresponded in some respects to the very unseasonable conditions experienced during that period in this country.

THE new meteorological observatory of San José de Costarica is to be considered a welcome gain to science, seeing that (as Dr. Hann points out) between Mexico in 19° N. lat. and Rio de Janeiro and Cordoba in 22° and 35° S. lat., there has been no observatory of the first rank, either in Central or South America. Recent data from Prof. Pittier there, reveal a remarkable daily period of rainfall. Thus in the five months August to December, while only 1·5 inch of rain fell between midnight and midday, 35 inches, or more than twenty times as much, fell between midday and midnight. Comparing the hours, 6 to 11 a.m., with 2 to 7 p.m., the quantities are 0·3 in. and 27·6 in. Nearly the whole of the rainfall occurs within six hours ($75\frac{1}{2}$ per cent.). And the largest amount is towards sunset, not (as commonly supposed about the tropics) in the early hours of the afternoon.

Writing on the subject of medicine in China, the *North China Herald* of Shanghai observes that medicine in China is very old. In the year 579 B.C. the moxa and acupunture were already practised by Chinese physicians, for it is in that year that this treatment is first mentioned in any book, Chinese or foreign. In addition to this there was the celebrated Pien-tsio, who some time during the period from the eighth century before Christ to the sixth performed remarkable cures by feeling the pulse first and basing his treatment upon the indications. On one occasion he was in attendance upon a prince who was in a state of unconsciousness for five days, and he depended on pulse-feeling for his knowledge of the patient's condition. The great books of Chinese medicine belonged to the age of the sages. They are the classics of Chinese medicine, and in them its theories and principles are enshrined. In these books we find such statements as that metal and water combine, in accord with the influence of Venus and Mars. The soul is spoken of as something distinct from though included in the body. Madness, fever, apoplexy, paralysis, cholera, are all described. The five elements are represented as revolving powers, and they correspond to the five planets in the heavens. The earth moves westward through space which surrounds it below as well as above and around. Ignorance of astrology is stated to be a cause of disease and death. Interlaced with the doctrine of the five elements is found the doctrine of the dual principles of darkness and light, each divided into greater and lesser. The veins and arteries are here described as canals originating in the skin, which, consequently, is that part at which all disease commences its invasion of the human frame. The possibility of the human subject securing immortality by Taoist methods is discussed, and the affirmative is believed. The "Soo wên," having in it these and other curious things, such as the rotundity of the earth and the doctrine of a universal and primæval vapour, and having a distinct tincture of the Mesopotamian astrology, constitutes in itself a convincing proof that China was receptive of Western knowledge to a large extent in the fifth, fourth, and third centuries before Christ. From that time during more than two thousand years China has been under the dominion of the philosophy of this book. The writer predicts that a history of Chinese medicine, being the result of the uninterrupted experience of two thousand five hundred years, in spite of its Babylonian theory, now exploded by modern discoveries, would prove deserving of high respect for its practical utility in many important ways.

The Cambridge Local Lectures Syndicate held a Conference of Local Lecturers and Committees in the Senate House on July 9 and 10, the Vice-Chancellor, Dr. Butler, Master of Trinity College, presiding. The subjects discussed were: (1) the affiliation of lecture centres to the University; (2) the relations with the Education Department; (3) State aid for local lectures, a subject started at Oxford last year, and introduced on this occasion by the request of some of the centres, not as part of the programme of the Syndicate; (4) local finance; (5) work in rural districts; (6) district associations. The subjects were all of them actively discussed, "State aid" being referred back to the Committee which is working in the matter. The whole party, numbering about 180, lunched in the Hall of King's and dined in the Hall of Trinity, as guests of the Syndicate, and visited the Library, the Museums of Science, and the Fitzwilliam Museum, at each of which an expository address was delivered.

The Syndicate have invited a limited number of their students in various parts of the Kingdom to spend the month of August in Cambridge, for the purpose of quiet and serious study. For some years, individual invitations of this kind have been given by persons interested in the work. The Syndicate have received favourable accounts of the work done and of the effect produced, and they are now making it part of their official business. They had contemplated from 30 to 40 students, but the number of those desirous of coming considerably exceeds that. The principle of the Syndicate is to give to the students opportunities which they could not have in the lecture centres, and on this account the ordinary subjects of local lectures are not included in the curriculum. The work is to last from August 5 to 30 inclusive. Newnham College will give a collegiate home to the women students, and Selwyn to the men, on very moderate terms. The mornings will be given to the science students, whose work will consist of courses of experimental demonstrations in the laboratories of chemistry, physics, geology, &c. The afternoons will be given to the art students, whose work will consist of series of lectures on Greek art, early English sculpture and inscriptions, early engraving, and architecture, all illustrated from the art collections and the buildings in Cambridge. Single lectures will be given, by leading residents in Cambridge, on subjects of which they have special knowledge, and this, no doubt, will be a feature of unusual interest and profit. The University Library, the Philosophical Library, and the Library of Art and Archæology, will all be open to the students by special arrangement for reading and study. Special lectures will be given on King's Chapel and Ely Cathedral, and the manuscript and other treasures of some of the College libraries will be shown and described in detail. Visits will be paid to the Observatory by day, for the inspection of the astronomical instruments, and Prof. Adams or his representative will receive small parties of the students at night. It is proposed to give to those who go satisfactorily through the regular course of study some record of what they have done. Several of the lecture centres have given scholarships of £10 to their best students to enable them to go to Cambridge, and the Syndicate are meeting all prizes of this kind by a remission of the small lecture fee. The advantage of working in small parties of 10 or 12 at such subjects as those indicated, and under such circumstances, can scarcely be exaggerated. The determination of the Syndicate is that the whole course of study shall be serious and quiet, but social amenities will not be disregarded.

The Report of the Cambridge Local Lectures Syndicate, recently issued, is unusually encouraging. The number of students and of courses of lectures is larger than ever, and the proportion of serious students to the whole number attending the lectures shows a remarkable increase. It is easy to get a large number of people to come to popular lectures, but to make people who come to lectures into serious students is a different matter. Nearly half of the whole number of 11,500 students have attended not the lecture only, but also the "class" for more detailed work by question and answer which always precedes or follows the lecture in the Cambridge system. More than a fifth part of the whole have written papers weekly for the lecturer, and the examinations at the end of the respective courses have been attended by nearly one in six. This is an interesting record of solid work done. A specially satisfactory feature of the year's work has been the manner in which the centres have supported the Syndicate in keeping up the lectures in each course to the full number of twelve, which is an integral part of the Cambridge system. Of 125 courses only five have been "half-courses" of six lectures, given under special circumstances and without the privilege of an examination. Thus the total number of lectures given has been about 1470, and the number of attendances at lectures not far off 140,000.

The additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♀ ♀) from India, presented by Captain C. Taylor; a Hawfinch (*Coccothraustes vulgaris*), British, presented by Mr. L. C.

Wharton; a Snow Bunting (*Plectrophanes nivalis*), European, presented by Mr. J. Young, F.Z.S.; a Common Boa (*Boa constrictor*) from Venezuela, presented by Mr. R. J. Money; a White-thighed Colobus (*Colobus vellerosus* ♂) from West Africa, a Cape Ratel (*Mellivora capensis* ♂) from South Africa, an Arctic Fox (*Canis lagopus* ♀) from the Arctic Regions; four Spoonbills (*Platalea leucorodia*), European, a Short-toed Lark (*Calendrella brachydactyla* ♂) from Algeria, purchased; four Australian Wild Ducks (*Anas superciliosa*), two Slender Ducks (*Anas gibberifrons*), eight Chilian Pintails (*Dafila spinicauda*), six Summer Ducks (*Æx sponsa*), four Mandarin Ducks (*Æx galericulata*), two Red-crested Pochards (*Fuligula rufina*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on July 24 = 18h. 10m. 17s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4390	—	—	18 6 45	+ 6 49
(2) G.C. 4403	—	—	18 14 17	−16 13
(3) D.M. + 43° 2890 ...	8	Reddish-yellow.	18 3 29	+43 26
(4) η Serpentis	3	Yellow.	18 15 36	− 2 56
(5) γ Ophiuchi	3	White.	17 42 16	+ 2 45
(6) D.M. + 36° 3243 ...	8	Red.	18 39 0	+36 50

Remarks.

(1) This small bright nebula was thought by W. Struve to be one of the most curious objects in the heavens. The G.C. description is: "A planetary nebula; very bright; very small; round; a little hazy." According to D'Arrest, its diameter is about 7″. The observations of Dr. Huggins and Captain Herschel show that the spectrum consists of the three chief nebula lines, and a faint continuous spectrum. Dr. Huggins also notes that "the lines are exceedingly sharp and well-defined." This latter observation requires confirmation, and the spectrum should also be examined for other lines, as we know that a greater number of lines are seen in other nebulæ of the same class.

(2) This is the so-called "Horse-shoe Nebula," which is thus described by Herschel: "A very remarkable object; bright; extremely large; extremely irregular figure; 2-hooked." The spectrum has been observed both by Dr. Huggins and Captain Herschel. The former noted in 1866 that the line near λ 500 was visible, in addition to a faint continuous spectrum, and added: "When the slit was made as narrow as the intensity of the light would permit, this bright line was not so well defined as the corresponding line in some of the other nebulæ under similar conditions of slit, but remained nebulous at the edges." It will be seen that this observation gives the chief line a very different character to the preceding one (4390), and it is very desirable that the discrepancy should be cleared up, especially as Dr. Huggins has recently stated that the line is always seen sharp and well defined, although there is no evidence to show that he has reobserved the nebulæ in which he formerly recorded it as ill defined. It is important that both nebulæ should be examined as nearly as possible at the same time with the same instrumental conditions. Captain Herschel simply writes: "Bright object; bright lines."

(3) The spectrum of this star is one of great interest in connection with the view that stars of Group II. are similar in constitution to comets. Dunér states that, notwithstanding the small magnitude of the star the bands are very well seen even in the ultra blue, and that they are so wide and dark that the spectrum is totally discontinuous, especially in the blue-green and the blue. Now it seems pretty evident that all the light referred to in the blue in a faint star like this cannot be due simply to continuous spectrum, and it is therefore probably due to the radiation of some substance. This substance is probably carbon, giving a series of bright flutings in the blue-green and blue, and giving rise to apparent dark bands, which are in all probability simply the dark spaces between the bright flutings. The measurements made by Dunér and Vogel of the bands in other stars show close coincidences with the carbon flutings, but the question can only be finally decided by direct comparisons. If the existence of the carbon flutings be confirmed, then we must conclude that stars of Group II. and comets showing the same series of flutings are identical in constitution.

(4 and 5) These are stars of the solar type and of Group IV. respectively, and the usual more detailed observations are required in each case.

(6) The observations of Secchi and Dunér show that the spectrum of this star is a well-marked one of Group VI.; but the only details observed were three "zones" separated by two strong dark bands. Further details and deviations from the regular type should be looked for.

A. FOWLER.

NICE OBSERVATORY.—The third volume of the "Annales de l'Observatoire de Nice" contains a new map of the solar spectrum by the late M. L. Thollon, the whole of the theory of the minor planet Vesta by M. Perrotin, the Director of the Observatory, and numerous observations of comets and planets made by M. Charlois.

The part of the spectrum mapped by M. Thollon extends from A to *b*, and is contained on seventeen beautifully engraved plates, each having two horizons 32 cm. long. The whole length is thus a little over ten metres, and the number of lines contained in it is about 3200, of which 2090 are said to have a solar origin, 866 are purely telluric, and 246 have a mixed origin—that is to say, they result from the superposition of solar and telluric lines.

Each of the 33 horizons is divided into millimetres, from 0 to 320, hence the lines can easily be read off to $\frac{1}{10}$ of a division. Thollon intended at the beginning of his work to express the position of the lines on a scale of wave-lengths, and this would doubtless have facilitated their identification to a considerable extent; but the method of relative measurement which he adopted was more accurate than the absolute measures made by Ångström, and he found that to use a wave-length scale it would be necessary to alter a number of accepted places of lines or to alter his measured intervals. It is rather unfortunate that such should be the case, for ready reference to the lines and comparisons of them with those mapped by other observers are rendered somewhat difficult. Beneath each scale are four horizons on which are respectively represented: (1) the appearance of the lines when the sun is 80° from the zenith and the air is dry; (2) the appearance of the lines when the sun is 60° from the zenith and the air is very moist; (3) the appearance of the lines when the sun is 60° from the zenith and the air is very dry; (4) the lines of solar origin—that is, those that would be observed from outside our atmosphere. The width of the lines was determined for each of the four horizons, and intensities are expressed from 1 to 10, 1 indicating the weakest and 10 the strongest lines. The values for each line are given in the text relating to the maps. Another horizon gives the position of iron lines, but this is incomplete in some of the maps owing to M. Thollon's death.

The theory of Vesta, by M. Perrotin, is in continuation of that published in the first volume of "Annales de l'Observatoire de Toulouse, 1880," and deals with the algebraical expressions of the perturbations produced on its elements by different planets.

ENLARGEMENT OF PHOTOGRAPHS OF STELLAR SPECTRA.—The enlargement of all the photographs of stellar spectra taken under Prof. Pickering's direction at the Henry Draper Memorial observatories is made by means of a cylindrical lens, and the result of the adoption of this method is well known. Dr. Scheiner, of Potsdam Observatory (*Astronomische Nachrichten*, No. 2969), has obtained even better results by fixing the negative lengthways in a frame which has a to-and-fro movement. The motion causes the width of the lines to be increased on the plate being exposed, in a manner similar to the increase that takes place when a cylindrical lens is inserted between it and the negative. The advantage of the arrangement over that of Prof. Pickering lies in the fact that the diminution of the intensity of the lines in the process of enlargement is much less.

The method now described by Dr. Scheiner has been used successfully at South Kensington for some time.

THE SCIENTIFIC PRINCIPLES INVOLVED IN MAKING BIG GUNS.

I.

STEAMSHIPS are now called *boats*, and the largest cannon are called *guns*, according to a process in language which philologists have explained; but while steamships have increased in size and complication, the gun, however big, satisfies the Hibernian definition of a cylindrical hole with metal placed round it; and the most difficult problem of the gun-maker is to dispose the metal in the most efficient manner, hampered as he is by the limitations of the metallurgical art.

The difficulties increase with the size of the gun, according to the well-known law of Mechanical Similitude.

Geometrical Similitude is independent of scale; a geometrical theorem is true, however large the figure may be drawn; but the laws of Mechanical Similitude are complicated, when we notice the differences between a simple girder and the Forth Bridge, or between the anatomy of large and small animals.

As an example of mechanical similitude, consider what sort of a steamship would be required to reduce the voyage to America from six to five days. The present steamers crossing in six days have a speed of 20 knots, and displacement of 10,000 tons, and the indicated horse-power is close on 20,000. To cross in five days the speed would have to be increased 20 per cent., to 24 knots; and now if we apply Froude's law that, at corresponding speeds as the sixth root of the displacements, the resistances are as the displacements, we shall find that the steamer would have to be of 30,000 tons, and of 65,000 horse-power, thus exceeding even the *Great Eastern's* dimensions.

With given material, say steel, the strongest with which we are familiar, a limit of size is soon reached at which the structure falls to pieces almost of its own weight; and recent experience with the heaviest artillery seems to show that we are nearing this limit.

The larger the gun or structure, then, the greater the necessity for careful and scientific design and proportion. It is proposed to give here a sketch of the fundamental principles which guide the gun-maker, and which he applies to secure the safety of the gun under the greatest pressure it can ever be called upon to sustain.

While reaping almost all the glory of success, the gun-maker cannot risk the disgrace of a failure; on the other hand, the carriage-maker can work with a small margin of safety, as ample warning would be given of any failure, and breakage is easily repaired; but the failure of a gun may be so disastrous that it must be avoided at all cost, so that the gun-maker never allows himself to work very close to the limits which his theory allows.

At the present time the design and employment at sea or in forts of such monsters as our 110-ton or Krupp's 135-ton guns is severely criticized and condemned in certain circles; but it is a maxim in artillery that one big gun is worth much more than its equivalent weight in smaller guns; and for naval engagements a few line-of-battle ships armed with the heaviest artillery are invincible, if properly flanked and protected by the light cavalry of frigates.

So, too, with steamships; the largest and fastest always fill with passengers, and by making rapid passages, and therefore more in a given time, are found to be more profitable in spite of their great initial cost and expense of working.

The size of the gun is settled by the thickness of armour it is required to attack; the calibre increasing practically as the thickness to be pierced, but the weight of the gun mounting up as the cube of the calibre. Thus if an 8-inch gun weighing 13 tons can pierce 12 inches of armour, a 16-inch gun is required to pierce 24 inches, and the 16-inch gun will weigh 104 tons.

PART I.—THE STRESSES IN A GUN.

(1) The theory of gun-making begins with the investigation of the stresses set up in a thick metal cylinder, due to steady pressures, applied either at the interior, or exterior, or at both cylindrical surfaces.

So far, the dynamical phenomena which arise from the propagation and reflexion of radial vibrations are beyond our powers of useful analysis; so that we restrict ourselves to the investigation of the elastical problem of the thick cylinder of elastic material, subject to given internal and external pressures, applied steadily, as in the case of a tube tested under hydraulic pressure.

Fig. 1 is drawn representing the stresses set up in a cylinder or tube B, by an internal pressure p_i; we denote by r_i and r_o the inner and outer radii, the suffixes i and o denoting *inside* and *outside*; and then r can be used to denote any intermediate radius.

The stress at any point at a distance r from the axis will consist of a radial pressure, p, and a circumferential tension, t; the radial pressure p decreasing from p_i at the inner radius r_i to zero at the outer radius r_o, the atmospheric pressure not being taken into account; while the circumferential tension t at the same time diminishes from t_i to t_o.

The British units employed in practical measurements with guns are the inch and the ton; so that r being measured in inches, p and t are measured in tons per square inch.

(2) To determine the state of stress at any point of the cylinder, we suppose it divided by a diametral plane $r_o r_i \mathrm{O} r_i r_o$; and the equilibrium of an inch length of either half is considered.

The stresses p and t being represented graphically by the ordinates of the curves $p_i p p_o$, $t_i t t_o$, the equilibrium of either half of the cylinder requires that the area of the circumferential tension-curve $r_i t_i t_o r_o$ and its counterpart should be equal to the area of the rectangle $\mathrm{O}p_i$, and its counterpart, these latter representing the thrust due to the pressure p_i on the half cylinder.

Then, denoting the area $r_i t_i t_o r_o$ by Q, and calling it the resistance of the section $r_i r_o$,

$$Q = p_i r_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

If we divide the resistance Q by the thickness of the cylinder $r_o - r_i$, we obtain the *average* circumferential tension in the material; and when the cylinder is thin, the maximum circumferential tension t_i and the average tension $Q/(r_o - r_i)$ will not be appreciably different; so that a knowledge of the average circumferential tension will be sufficient for practical purposes in such cases as, for instance, of the cylindrical shell of a boiler; and we have thus the elementary formula ordinarily employed in the design of boilers.

But when, as in a gun or hydraulic press, the thickness has to be made considerable, we must have the means of determining the *maximum* tension t_i, and of contriving that t_i shall not exceed a certain proof limit suitable for the material.

(3) Now, just as the equilibrum of either half of the cylinder requires that the area $r_i t_i t_o r_o = p_i r_i$, so the equilibrium of either half of a part of the cylinder bounded internally by the radius r_i, and externally by any radius r, requires that the area $r_i t_i t r$ should equal the rectangle $\mathrm{O}p_i$ minus the rectangle $\mathrm{O}p$; or, in the notation of the Integral Calculus—

$$\int_{r_i}^{r} t dr = p_i r_i - pr \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

The first attempt at a solution of these equations (1) and (2) is due to Peter Barlow, when called upon to calculate the strength of the cylinder of the Bramah hydraulic press, in a paper read before the Society of Civil Engineers in February 1825, and published in the *Edinburgh Journal of Science*, and in the Trans. I.C.E., vol. i. 1836.

(4) Barlow assumed that under an internal pressure the metal is compressed radially as much as it is stretched circumferentially, so that the cubical compression of the metal is zero, and he is justified therefore in putting $p = t$ in the material of the cylinder.

Then equation (2) becomes

$$\int_{r_i}^{r} p dr = p_i r_i - pr;$$

so that, differentiating with respect to r,

$$p = -d(pr)/dr, \text{ or } dp/p + 2dr/r = 0;$$

and integrating again with respect to r,

$$\log p + \log r^2 = \text{constant},$$

or

$$pr^2 = a, \text{ a constant}; \; p = t = ar^{-2} \quad . \quad . \quad . \quad . \quad (3);$$

so that p and t, if equal, vary inversely as the square of the distance from the axis.

Thus, a cylindrical tube under internal and external pressures which are inversely as the squares of the internal and external radii respectively, will, according to Barlow's law, have at any point a radial pressure and an equal circumferential tension, also inversely as the square of the distance from the axis.

When the thickness of the cylinder is considerable, compared with the bore, this solution of Barlow will give a very fair indication of the true result.

(5) But Rankine showed ("Applied Mechanics," § 273) that, by superposing the state of hydrostatic stress produced by equal internal and external pressures, we obtain the algebraical solution of the most general case where the internal and external applied pressures are arbitrary.

For if we suppose the state of stress in the cylinder is a hydrostatic stress, composed of a radial pressure p, and an equal circumferential pressure $-t$, then equation (2) becomes—

$$\int_{r_i}^{r} p\,dr = pr - p_ir_i;$$

and differentiating with respect to r,

$$p = d(pr)/dr, \text{ or } dp/dr = 0;$$

so that

$$p = b, \text{ a constant; and then } t = -b \quad . \quad . \quad . \quad (4).$$

(6) The superposition of this state of stress on Barlow's state of stress gives—

$$p = ar^{-2} + b, \qquad t = ar^{-2} - b \quad . \quad . \quad . \quad . \quad (5),$$

or

$$(p + t)r^2 = 2a, \qquad p - t = 2b;$$

values which will be found to verify equation (2); and now the constants a and b are determined for arbitrarily applied internal and external pressures p_i and p_o by the equations

$$p_i = ar_i^{-2} + b, \qquad p_o = ar_o^{-2} + b;$$

so that

$$a = \frac{p_i - p_o}{r_i^{-2} - r_o^{-2}} = \frac{(p_i - p_o)\,r_i^2r_o^2}{r_o^2 - r_i^2};$$

$$b = \frac{p_or_o^2 - p_ir_i^2}{r_o^2 - r_i^2} = \frac{p_or_i^{-2} - p_ir_o^{-2}}{r_i^{-2} - r_o^{-2}}.$$

These results were first obtained by Lamé and Hart (the late Sir Andrew Searle Hart, of Dublin), but in a much more complicated manner. Lamé's solution was given in his "Leçons sur la théorie mathématique de l'élasticité des corps solides"; while Hart's treatment of the question will be found in Note W to Robert Mallet's "Physical Conditions involved in the Construction of Artillery" (1856). An investigation of the same problem by Maxwell, when about eighteen years old, in the Trans. R. S. Edin., vol. xx. 1850, has been generally overlooked.

Rankine's treatment analyzes the mechanical signification of the separate terms of the solution, and obtains them by simple reasoning from the state of stress, without an appeal to the laws of elasticity and the consequent state of strain.

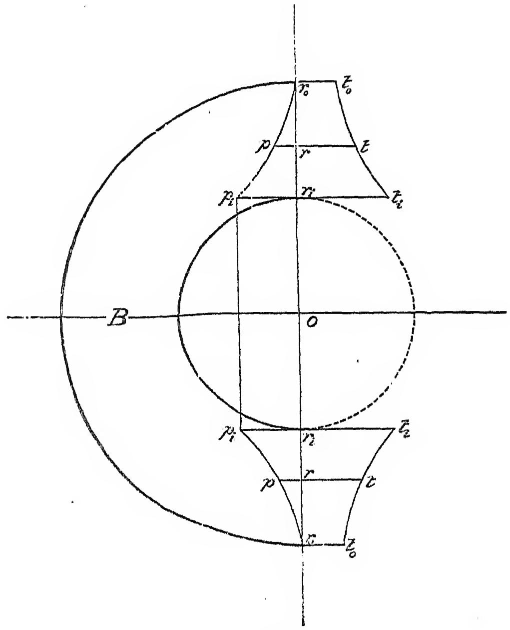

FIG. 1.

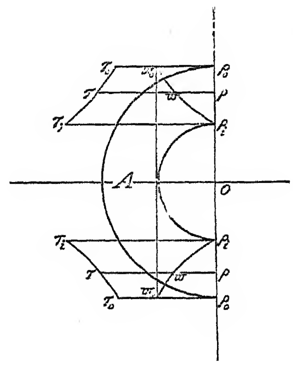

FIG. 2.

(7) Putting $p_ir_i^2 = p_or_o^2$ makes $b = 0$, and gives the particular case considered first by Barlow; and putting $p_i = p_o$ makes $a = 0$, and gives the additional particular case of uniform hydrostatic stress invented by Rankine.

But, in the general case, a and b may have any values, positive or negative, according to the relations between p_i and p_o, r_i and r_o.

Thus, as in Fig. 1, with $p_o = 0$, we find—

$$a = \frac{p_i}{r_i^{-2} - r^{-2}}, \quad b = \frac{-p_ir_o^{-2}}{r_i^{-2} - r_o^{-2}};$$

and then

$$p = ar^{-2} + b = p_i\frac{r^{-2} - r_o^{-2}}{r_i^{-2} - r_o^{-2}}; \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$t = ar^{-2} - b = p_i\frac{r^{-2} + r_o^{-2}}{r_i^{-2} - r_o^{-2}}; \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$t_i = p_i\frac{r_i^{-2} + r_o^{-2}}{r_i^{-2} - r_o^{-2}} = p_i\frac{r_o^2 + r_i^2}{r_o^2 - r_i^2}; \quad . \quad . \quad . \quad . \quad (8)$$

$$t_o = p_i\frac{2r_o^{-2}}{r_i^{-2} - r_o^{-2}} = p_i\frac{2r_i^2}{r_o^2 - r_i^2} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

(8) Now using t to denote the *average* value of the circumferential tension, so that

$$t = p_ir_i/(r_o - r_i),$$

then

$$\frac{t_i}{t} = \frac{r_o^2 + r_i^2}{r_i(r_o + r_i)}, \quad \frac{t_i - t}{t} = \frac{r_o}{r_i}\,\frac{r_o - r_i}{r_o + r_i} \quad . \quad . \quad (10)$$

thus showing that the maximum tension t_i may exceed the average tension t by a considerable amount; and it is this *maximum* tension t_i which must be carefully watched and kept down below a certain working value; so that, with given t_i, the maximum allowable pressure in the tube is given by

$$p_i = t_i\frac{r_o^2 - r_i^2}{r_o^2 + r_i^2}.$$

This is the formula now used in the design of a hydraulic press, or of a thick tube, of bore $2r_i$, to stand an internal pressure p_i; t_i being fixed by the strength of the material, and then r_o being calculated.

We notice that p_i is always less than t_i, so that a tube, how-

-ever thick, cannot stand, if unsupported, an internal pressure greater than the working tenacity of the material.

But, as the pressures in gunnery often exceed the tenacity of any known material, the requisite strength must be provided by an initial compression of the tube due to shrinking on one or more cylindrical jackets.

(9) Fig. 2 is drawn representing graphically the state of stress set up in a tube A by an external applied pressure ϖ_o, as in the tube or flue of a boiler by the external pressure of the water, or in the internal tube of a gun by the shrinkage pressure of the outside jacket.

Denote by ρ_i and ρ_o the inner and outer radii of the tube A, and by ρ any intermediate radius.

The stress at any point of the tube will now consist of a radial pressure ϖ, and of a circumferential *pressure* τ, represented by the ordinates of the curves $\varpi_o\varpi\varpi_i$, $\tau_o\tau\tau_i$; and dividing the tube by a diametral plane $\rho_o\rho_i O\rho_i\rho_o$, and considering the equilibrium of inch length of either half, we shall find as before that the area $\rho_i\tau_i\tau_o\rho_o$ = the rectangle $O\varpi_o = \varpi_o\rho_o$; while considering the equilibrium of any coaxial cylindrical portion, bounded by the radii ρ_o and ρ, then the area $\rho\tau\tau_o\rho_o$ = rectangle $O\varpi_o$ − rectangle $O\varpi$; or, in the notation of the Integral Calculus—

$$\int_{\rho}^{\rho_o} \tau d\rho = \varpi_o\rho_o - \varpi\rho\,; \quad \ldots \ldots (11)$$

leading, by differentiation with respect to ρ, to

$$\tau = -\,d(\varpi\rho)/d\rho\,; \quad \ldots \ldots (11^*)$$

the general solution of which can, as before, be exhibited in the form—

$$\varpi = \beta + \alpha\rho^{-2},\ \tau = \beta - \alpha\rho^{-2}, \quad \ldots (12)$$

or

$$(\varpi - \tau)\rho^2 = 2\alpha,\ \varpi + \tau = 2\beta,$$

where α and β are arbitrary constants, determined from the values of the arbitrary pressures applied to the interior and exterior surfaces.

(10) Now with $\varpi_i = 0$,

$$0 = \beta + \alpha\rho_i^{-2},\ \varpi_o = \beta + \alpha\rho_o^{-2}\,;$$

so that

$$\alpha = \frac{-\varpi_o}{\rho_i^{-2} - \rho_o^{-2}},\quad \beta = \frac{\varpi_o\rho_i^{-2}}{\rho_i^{-2} - \rho_o^{-2}}\,;$$

and then

$$\varpi = \beta + \alpha\rho^{-2} = \varpi_o\frac{\rho_i^{-2} - \rho^{-2}}{\rho_i^{-2} - \rho_o^{-2}}, \quad \ldots (13)$$

$$\tau = \beta - \alpha\rho^{-2} = \varpi_o\frac{\rho_i^{-2} + \rho^{-2}}{\rho_i^{-2} - \rho_o^{-2}}, \quad \ldots (14)$$

$$\tau_o = \varpi_o\frac{\rho_i^{-2} + \rho_o^{-2}}{\rho_i^{-2} - \rho_o^{-2}} = \varpi_o\frac{\rho_o^2 + \rho_i^2}{\rho_o^2 - \rho_i^2}, \quad \ldots (15)$$

$$\tau_i = \varpi_o\frac{2\rho_i^{-2}}{\rho_i^{-2} - \rho_o^{-2}} = \varpi_o\frac{2\rho_o^2}{\rho_o^2 - \rho_i^2}. \quad \ldots (16)$$

Given, then, τ_i the maximum allowable crushing pressure of the material, then

$$\varpi_o = \tau_o(\rho_o^2 - \rho_i^2)/2\rho_o^2 = \tfrac{1}{2}\tau_o(1 - \rho_i^2/\rho_o^2) \quad \ldots (17)$$

is the maximum allowable external pressure on the tube.

(11) If we make $\rho_o = r_i$ and $\varpi_o = p_i$, the tube A of Fig. 2 may be supposed to be gripped by the cylinder B of Fig. 1, of which only the upper halves need now be shown, as in Fig. 3; and now Fig. 3 will represent the cross-section of a tube, A, over which a jacket, B, has been shrunk, as at the breech end of an ordinary field-gun, and will represent graphically the stresses set up when the pressure, $\varpi_o = p_i$, at the common surface, is supposed known; these are called the *initial stresses*, or stresses of repose; the internal pressure at the radius ρ_i and the

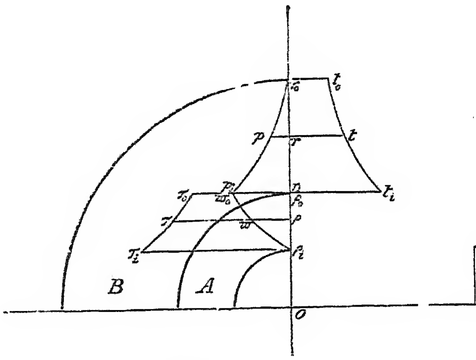

FIG. 3.

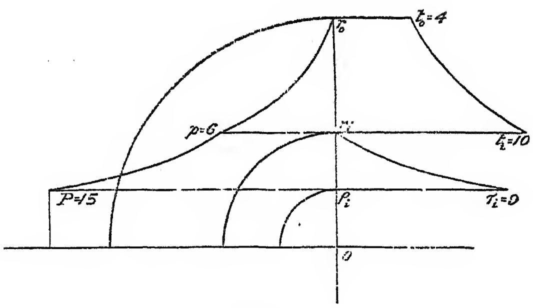

FIG. 4.

external pressure at the radius r_o being zero, as the atmospheric pressure is insensible in our calculations.

In Fig. 3 we notice that the total pull resistance across the section r_or_i, represented by the area $r_ot_ot_ir_i$, is equal to the total thrust resistance of the section $\rho_o\rho_i$, represented by the area $\rho_o\tau_o\tau_i\rho_i$, and each of these is equal to the resultant pressure thrust represented by the area of the rectangle Op_i.

(12) Now, suppose a pressure P (say 15 tons on the square inch) is applied at the interior of the tube, either by the steady pressure of water, as in a hydraulic press, or by the momentary pressure of gunpowder, as in the bore of a gun.

We suppose that the additional stresses due to this pressure, P, which we shall call the *powder stresses*, are the same as those which would be set up in a homogeneous cylinder of internal radius ρ_i, and external radius r_o, by a steady pressure, P; and these powder stresses will therefore, by what precedes, in equations (6), (7), (8) (Fig. 1), at a distance r from the axis, consist of a radial pressure—

$$\mathrm{P}\frac{r^{-2} - r_o^{-2}}{\rho_i^{-2} - r_o^{-2}}, \quad \ldots \ldots (18)$$

and a circumferential tension—

$$\mathrm{P}\frac{r^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}}, \quad \ldots \ldots (19)$$

having a maximum value at the bore of

$$\mathrm{T} = \mathrm{P}\frac{\rho_i^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}}.$$

We must superpose these powder stresses on the initial stresses of the compound cylinder to obtain the stresses when the cylinder is used as a gun (or hydraulic press); these are called the *firing stresses*, and they are exhibited graphically in Fig. 4.

(13) We now see the reason for setting up initial stresses in the gun by shrinking a jacket over the interior tube.

For the maximum circumferential tension at the bore on firing is reduced by the initial stresses from

$$\mathrm{T} = \mathrm{P}\frac{\rho_i^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}} \text{ to } \mathrm{P}\frac{\rho_i^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}} - \varpi_o\frac{2\rho_i^{-2}}{\rho_i^{-2} - \rho_o^{-2}}, \quad (20)$$

while at the interior of the jacket the circumferential tension is altered from

$$\mathrm{P}\frac{r_i^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}} \text{ to } \mathrm{P}\frac{r_i^{-2} + r_o^{-2}}{\rho_i^{-2} - r_o^{-2}} + p_i\frac{r_i^{-2} + r_o^{-2}}{r_i^{-2} - r_o^{-2}}. \quad \ldots (21)$$

The maximum stresses in the gun are thereby equalized to a great extent, and material can be economized.

(14) Thus, with $\rho_i/\rho_o = r_i/r_o = \frac{1}{2}$, and P = 15, the powder stresses are given by circumferential tensions—

$$T = \tau_i = 17, \qquad t_i = 5;$$

so that, with a shrinkage pressure $\varpi_o = p_i = 3$, the principal firing stresses are given by circumferential tensions—

$$\tau_i = 17 - 8 = 9, \text{ a great reduction on } 17,$$
$$t_i = 5 + 5 = 10,$$

while

$$\tau_o = 5 - 5 = 0,$$
$$t_o = 2 + 2 = 4.$$

We need not consider the radial pressures for practical purposes.

To equalize these maximum tensions, $\tau_i = 9$ and $t_i = 10$, the tube might be made slightly thicker and the jacket thinner, or else the shrinkage pressure ϖ_o or p_i slightly diminished, keeping to the same bore and external diameter.

(15) We have thus shown how the initial stress, the powder stress, and the firing stress at any point of a gun composed of a tube and a single jacket is found, and exhibited graphically in Figs. 3 and 4.

The curves in the figure are seen to be all similar to a curve whose equation is of the form $y = ax^{-2}$, now called the Barlow curve.

When the gun is built up of three or more concentric cylinders, the method of procedure is the same; the initial pressure between the cylinders may be supposed known from the amount of shrinkage given in the manufacture; and now, taking any intermediate cylinder of the gun under initial pressures p_i and p_o at the internal and external surfaces, of radii r_i and r_o, we erect ordinates to represent p_i and p_o, and draw the Barlow curve joining their ends.

The Barlow curve representing the circumferential tension or pressure will always appear as an equal reflection of the pressure curve, the position being assigned so as to make the area of the circumferential *tension* curve equal to $p_i r_i - p_o r_o$; and it may happen that this area may vanish or become negative, showing that some or all of the initial circumferential tensions are really pressures.

(16) But practically the gun-maker reverses this procedure; with him it is the maximum circumferential firing tension t_i of a tube or hoop which is limited by the strength of the metal; so that, starting with these t_i's, as given, he calculates the pressures between the successive coils of the gun, proceeding inwards, and finally determines the maximum allowable powder pressure in the interior of the bore.

Afterwards he subtracts the powder stresses from these firing stresses, and thus obtains the initial stresses in the gun; and then from these initial stresses he calculates the amount of shrinkage to be given to the coils or hoops to obtain the requisite state of initial stress. But we shall show subsequently that the requisite amount of shrinkage is given just as simply from the firing stresses as from the initial stresses; so that henceforth we need only determine the firing stresses.

(17) Then Fig. 5 represents the maximum allowable firing stress over the powder-chamber of the American 8-inch gun, shown in cross-section, as composed of an inner tube, A, over which a jacket, B, and two hoops, C and D, have been shrunk on.

In practice, the maximum allowable tension in the jacket and hoops is restricted to 18 tons per square inch, but in the inner tube to 15 tons per square inch, so as to allow for erosion of the bore, the weakening due to the rifling grooves, and the possible failure of the tube.

(18) In the notation of the "Text-book of Gunnery," 1887, by Major Mackinlay, R.A., supposing there are n cylinders in the cross-section of the gun, the successive radii of the cylindrica

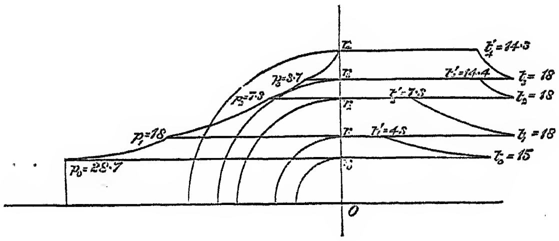

FIG. 5.

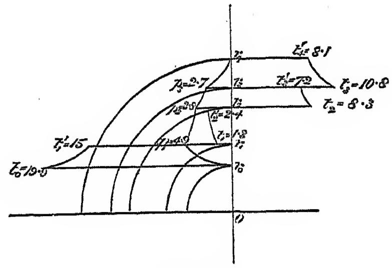

FIG. 6.

surfaces, beginning from the outside, are denoted in inches by $r_n, r_{n-1}, \ldots, r_2, r_1, r_0$; and the firing pressures at the surfaces of separation are denoted by $p_{n-1}, \ldots, p_2, p_1$; and p_0 finally denotes the powder pressure at the radius r_0 of the bore.

We notice that there is no sudden change in the value of the radial pressure; but that the circumferential tension, t, changes suddenly from one cylinder to the next.

As we are concerned principally with the *maximum* tensions, which occur practically at the inner surface of a cylinder, we denote them, proceeding inwards, by $t_{n-1}, t_{n-2}, \ldots t_2, t_1, t_0$; and we shall suppose them to change suddenly to $t'_{n-1}, t'_{n-2}, \ldots t'_2, t'_1$, in proceeding inwards to the next cylinder.

(19) Starting from the outside cylinder, the stress formulas give, since $p_n = 0$,

$$p_{n-1} = ar_{n-1}^{-2} + b, \qquad 0 = ar_n^{-2} + b,$$

while

$$t_{n-1} = ar_{n-1}^{2} - b, \qquad t'_n = ar_n^{-2} - b;$$

so that

$$p_{n-1} = a\left(r_{n-1}^{-2} - r_n^{-2}\right),$$

$$t_{n-1} = a\left(r_{n-1}^{-2} + r_n^{-2}\right),$$

or

$$p_{n-1} = \frac{r_{n-1}^{-2} - r_n^{-2}}{r_{n-1}^{-2} + r_n^{-2}} t_{n-1}, \quad \ldots \ldots (22)$$

giving p_{n-1} when t_{n-1} is assigned by its maximum allowable value.

Also

$$t_{n-1} - t'_n = p_{n-1}, \quad \ldots \ldots (23)$$

giving t'_n, if required for the diagram.

Proceeding inwards to the next cylinder, we have (with different values of a and b)—

$$p_{n-2} = ar_{n-2}^{-2} + b, \qquad p_{n-1} = ar_{n-1}^{-2} + b,$$

$$t_{n-2} = ar_{n-2}^{-2} - b, \qquad t'_{n-1} = ar_{n-1}^{-2} - b;$$

so that, eliminating the a and b, first b and then a,

$$p_{n-2} - p_{n-1} = a\left(r_{n-2}^{-2} - r_{n-1}^{-2}\right),$$

$$t_{n-2} + p_{n-1} = a\left(r_{n-2}^{-2} + r_{n-1}^{-2}\right),$$

and therefore, by division,

$$p_{n-2} - p_{n-1} = \frac{r_{n-2}^{-2} - r_{n-1}^{-2}}{r_{n-2}^{-2} + r_{n-1}^{-2}} (t_{n-2} + p_{n-1}),$$

or

$$p_{n-2} = \frac{r_{n-1}^{2} - r_{n-2}^{2}}{r_{n-1}^{2} + r_{n-2}^{2}} (t_{n-2} + p_{n-1}) + p_{n-1}. \quad (24)$$

giving p_{n-2} when p_{n-1} is known, and when t_{n-2} is assigned by its maximum allowable value in practice.

Also

$$t_{n-2} - t'_{n-1} = p_{n-1} \quad \ldots \ldots (25)$$

thus determining t'_{n-1}; a knowledge of t'_{n-1} is required when we come to the determination of the amount of shrinkage necessary to produce p_{n-1}.

Proceeding successively in this manner, we finally obtain—

. . . .

$$p_2 = \frac{r_3^2 - r_2^2}{r_3^2 + r_2^2}(t_2 + p_3) + p_3 \quad . . . . \text{(iii.)}$$

$$p_1 = \frac{r_2^2 - r_1^2}{r_2^2 + r_1^2}(t_1 + p_2) + p_2 \quad . . . . \text{(ii.)}$$

$$p_0 = \frac{r_1^2 - r_0^2}{r_1^2 + r_0^2}(t_0 + p_1) + p_1 \quad . . . . \text{(i.)}$$

thus determining p_0, the maximum allowable powder pressure in the gun, for maximum working values of $t_0, t_1, t_2 \ldots t_{n-1}$; and these are the fundamental equations employed in gun-making.

(20) With no shrinkage, or a homogeneous gun, the maximum allowable powder pressure would be reduced to

$$t_0 \frac{r_n^2 - r_0^2}{r_n^2 + r_0^2},$$

so that we perceive the advantage of the shrinkage in strengthening the gun.

(21) In Fig. 5 the dimensions are taken from the American "Notes on the Construction of Ordnance," No. 31, by Lieut. Rogers Birnie, slightly altered to round numbers; the diameter of the powder-chamber of the 8-inch gun is supposed to be 10 inches; so that $r_0 = 5$; and we put $r_1 = 7$, $r_2 = 11$, $r_3 = 13$, $r_4 = 16$; instead of 4·75, 7, 11, 13·15, and 15·75, as given in the Note 31.

Now, solving equations (25), (iv.), (iii.), (ii.), (i.) with $p_4 = 0$, $t_3 = t_2 = t_1 = 18$, $t_0 = 15$, we shall find—

$$p_3 = \frac{16^2 - 13^2}{16^2 + 13^2} \times 18 = 3·7, \qquad t'_4 = 14·3;$$

$$p_2 = \frac{13^2 - 11^2}{13^2 + 11^2}(18 + 3·7) + 3·7 = 7·3, \quad t'_3 = 14·4;$$

$$p_1 = 18, \qquad t'_2 = 7·3;$$

$$p_0 = 28·7, \qquad t'_1 = 4·3.$$

Thus, the maximum allowable powder pressure in the chamber of this gun is nearly 29 tons per square inch; so that if the pressure is limited to 17, the gun has a factor of safety $29 \div 17 = 1·7$.

Joining the tops of the ordinates for p_0 and p_1, t_0 and t'_1, &c., by Barlow's curves, we have the graphical representation of the maximum allowable firing stresses of this gun; in which it must be noticed that the area of the rectangle, $p_0 r_0 = 143·5$, is equal to the area of all the circumferential tension curves bounded by the jagged edge $t_0 t'_1 t_1 t'_2 \ldots$

(22) With a powder pressure $p_0 = 28·7$ (tons on the square inch) the powder stresses will be given by

$$t_0 = \frac{16^2 + 5^2}{16^2 - 5^2} \times 28·7 = 34·9,$$

$$p_1 = p_0 \frac{7^{-2} - 16^{-2}}{5^{-2} - 16^{-2}} = 13·1,$$

$$t_1 = t_0 - p_0 + p_1 = 19·3 = t'_1;$$

$$p_2 = 3·5, \quad t_2 = 9·7 = t'_2;$$

$$p_3 = 1·0, \quad t_3 = 7·2 = t'_3;$$

and

$$p_4 = 0, \quad t_4 = 6·2.$$

Subtracting these powder stresses from the firing stresses, we are left with the initial stresses in the gun in a state of repose, represented in Fig. 6, and given by

$p_0 = 0,$	$t_0 = -19·9,$	
$p_1 = 4·9,$	$t'_1 = -15\ ,$	$t_1 = -\ 1·3;$
$p_2 = 3·8,$	$t'_2 = -\ 2·4,$	$t_2 = -\ 8·3;$
$p_3 = 2·7,$	$t'_3 = \ 7·2,$	$t_3 = \ 10·8.$
$p_4 = 0,$	$t'_4 = \ 8·1,$	

(23) The data to which the gun-maker works are, first, the calibre of the gun; and secondly, the maximum powder pressure to be expected at any point of the bore; from these data, and the quality of the steel at his command, and also from the capacity of his machinery in producing and shaping the various pieces, the gun-maker proceeds to calculate the requisite thickness and number of the coils, arranged so that the maximum working tension shall not exceed certain practical limits laid down (18 tons per square inch in the coils, and 15 in the tube).

Thus, suppose he is called upon to design the cross-section of a gun over the powder-chamber, 10 inches in diameter, to stand a pressure of 20 tons per square inch.

He will generally take a factor of safety, say 2, and allow for double the pressure, so that he puts $p_0 = 40$, and then $t_0 = 15$.

He has r_0 given as 5 inches, and now r_1 is settled by the manufacture of the solid steel block or log, which is bored out to form the inner tube A; and now he can calculate p_1 and t'_1.

Practical considerations of manufacture decide the thickness and external radius r_2 to be given to the jacket B; and now, knowing r_1, r_2, p_1, and $t_1 = 18$, he can calculate p_2 and t'_2.

Similar practical metallurgical and manufacturing considerations decide the most suitable thickness for the hoops C, D, . . . ; and when he finds the radial pressure has become zero (or negative) the gun-maker knows that he has given his gun sufficient thickness and strength.

(24) A rule, suggested by Colonel Gadolin, was originally found convenient, by which the radii of the coils were made to increase in geometrical progression; this rule, though useful when guns were formed of a steel tube strengthened with wrought-iron hoops, is obsolete now that steel is used throughout; it was, however, formerly employed as a first approximation in the tentative solution of the problem.

The Longitudinal Stress in the Gun.

(25) So far we have not yet taken into account the distribution of longitudinal tension in the gun; and it must be confessed that no satisfactory rigorous theory exists at present for the determination.

Practically it is usual to take the longitudinal tension as uniform across a cross-section, and as due to the powder-pressure in the bore, treated as a closed vessel, closed at one end by the breech-piece, and at the other by the projectile.

Thus, with r_0 and r_2 as the internal and external radii, and p_0 as the powder-pressure, the longitudinal tension will have its average value

$$\pi r_0^2 p_0 / \pi(r_2^2 - r_0^2) = p_0/(r_2^2/r_0^2 - 1) \quad . . . (26)$$

tons per square inch.

The average circumferential tension being

$$p_0 r_0/(r_2 - r_0),$$

this longitudinal tension will be

$$\frac{r_0^2}{r_2^2 - r_0^2} \cdot \frac{r_2 - r_0}{r_0} = \frac{r_0}{r_2 + r_0}$$

of the average circumferential tension, reducing to one-half in a thin cylinder, in which we may put $r_2 = r_0$.

For this reason it was formerly considered safe to leave the longitudinal strength to take care of itself; but some alarming failures, in which the gun on firing drew out like a telescope, have shown the necessity of carefully hooking the coils together, to provide the requisite longitudinal strength.

The larger the gun, the greater the number of separate parts requisite in its construction, and the greater the difficulty of providing for longitudinal strength.

(26) By taking a simple cylindrical tube under given internal and external pressures, and supposing it closed by hemispherical ends, a certain theory of distribution of longitudinal tension can be constructed.

For while the cylindrical part has the same transverse stresses as previously investigated, the stresses in the hemispherical ends may be considered the same as would be produced if they were joined up into a complete spherical vessel, under the same applied pressures.

A similar procedure to that already given for the cylinder is shown by Rankine ("Applied Mechanics," § 275) to lead to radial pressure $p = ar^{-3} + b$, and tension $t = \frac{1}{2}ar^{-3} - b$, in all directions perpendicular to the radius r.

For equation (2) for the cylinder becomes modified in the sphere to

$$\int_{r_i}^{r} 2\pi r t dr = \pi r_i^2 p_i - \pi r^2 p \; ; \quad . . . . . (27)$$

or, differentiating with respect to r,

$$2rt = -d(r^2p)/dr$$
$$= -2rp - r^2dp/dr,$$

or

$$t = -p - \tfrac{1}{2}rdp/dr. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

(27) The first assumption of Barlow, that there is no cubical compression, gives $t = \frac{1}{2}p$; and therefore

$$dp/p + 3dr/r = 0,$$

or

$$pr^3 = a, \text{ a constant},$$
$$p = 2t = ar^{-3}.$$

Rankine's second assumption of uniform hydrostatic stress gives $t = -p$; and therefore

$$dp/dr = 0, \ p = b, \text{ a constant}.$$

Hence, in the general case,

$$p = ar^{-3} + b, \ t = \tfrac{1}{2}ar^{-3} - b; \quad . \quad . \quad . \quad . \quad (29)$$

where a and b are determined from the given values p_i and p_o of the internal and external applied pressures; so that

$$p_i = ar_i^{-3} + b, \quad p_o = ar_o^{-3} + b,$$

$$a = \frac{p_i - p_o}{r_i^{-3} - r_o^{-3}}, \quad b = \frac{p_o r_o^3 - p_i r_i^3}{r_o^3 - r_i^3} \quad . \quad . \quad (30)$$

(28) We may now take $\frac{1}{2}ar^{-3} - b$ to represent the longitudinal tension at radius r in the cylindrical part of the closed vessel.

Unfortunately for the strict mathematical accuracy of this method, we must suppose the circumferential tension to change suddenly from its value given from the formula $ar^{-3} - b$ to one given by a formula of the form $\frac{1}{2}a'r^{-3} - b'$, in passing from the cylindrical part to the hemispherical end.

A. G. GREENHILL.

(*To be continued.*)

STUDIES IN BIOLOGY FOR NEW ZEALAND STUDENTS.[1]

IT is now generally recognized that of all recent works dealing with elementary natural science, none have more thoroughly revolutionized our methods of teaching than those of Huxley, well known; and the years 1875–77 will be for all time memorable to English-speaking students, as those which marked their publication. The principles therein laid down are now so well known and generally adopted, that explanation of them would be here superfluous. In his work on "Physiography" the author points out (preface, p. viii.) that any intelligent teacher will have no difficulty in making use of the resources of his surroundings, in the manner and to the end laid down by himself; and this, in the long run, is the refrain of the method by which he has effected the revolution alluded to. So far as external evidences go, this wise counsel appears to have been nowhere more readily acted upon than in New Zealand.

Prof. Hutton, now of Christchurch, New Zealand, early took the hint; and, in so doing, produced the first of the series of pamphlets now under consideration. He chose for his purpose the Shepherd's Purse (cf. NATURE, vol. xxiv. p. 188), and Prof. Parker, who succeeded him, has, in turn, prepared notes serial with those of his predecessor—upon "The Bean Plant" (1881), and now upon "The Skeleton of the New Zealand Crayfishes." During the interval between the publication of Prof. Parker's pamphlets there appeared the third of the series, entitled "The Anatomy of the Common Mussels (*Mytilus latus*, *edulis*, and *magellanicus*)." This, the work of Alex. Purdie, and the least didactic of the series, was originally presented as a thesis for the degree of M.A. in the University of New Zealand.

The pamphlets alluded to are illustrated—in the case of that before us, by six clear woodcuts; and those of Parker, with which we need now alone be concerned, chiefly depart from the precedent laid down by Huxley in their less rigid adherence to the single type organism chosen for study. Wherever parts of this are, by adaptive change, so modified as to be non-typical in structure, Parker has introduced supplementary descriptions of corresponding parts of less modified allies. The necessity for this mode of procedure is now generally recognized; and the only danger to be averted in the future is that of unconscious reversion to the old condition of the "*omnium gatherum* of scraps." Let the type organism be always adhered to as closely as possible. Prof. Parker has exercised, in the matter, a wise discretion; and, having availed himself of the researches of Boas, has given to the world of carcinologists a laboratory guide which cannot fail to be of great service to them. The arthropods of the genus *Palinurus* happen to have furnished him, a few years ago, with material for original observation; the results of his inquiry are brought to bear upon the needs of the beginner in the pamphlet before us, and the value of the latter is thereby enhanced.

In dealing with the morphology of the eye-stalk (and of the pre-oral region generally), Prof. Parker states the alternative views, and gives the names of their leading upholders. Although he adopts the belief that the ophthalmic and antennulary regions of the arthropod body do not form the first and second metameres, and introduces, in accordance therewith, a revised nomenclature, his remarks, when dealing with the real point at issue, are so framed as to leave the mind of the student unbiassed. And moreover, he has so arranged his book that consideration of this vexed question in morphology is deferred until the concluding paragraph. This is as it should be. He naïvely summarizes the position in the words:—

"The fact of the eye-stalk bearing a flagellum seems to prove conclusively that it and the antennule are homologous. The question then resolves itself into this: Are the eye-stalks and antennules appendages in the ordinary sense, *i.e.* lateral offshoots of the first two metameres, or are they to be looked upon as prostomial appendages comparable with the tentacles of Chætopods and the antennæ of insects?"

Mindful of comments upon the general question raised in the above, which have already appeared in this journal (NATURE, vol. xxxv. p. 506), we are of opinion that equally good arguments are still to be adduced on both sides. The extraordinary facts of development of the invertebrate nervous system which are now accumulating, render it doubtful if we are justified in regarding the prostomium as something so very different from the rest of the body as we are wont; and we are led to ask whether it may not merely represent a precociously differentiated portion of the common perisoma? If there is any truth in the belief that the symmetry of the bilateralia is a laterally compressed radial one, the probability that the prostomium may represent that which we suggest becomes vastly increased; and it is worthy of remark that that lobe in some Chætopods (*Nemodrilus*, *Phreoryctes*) so far conforms to the characters of a body segment as to become externally subdivided. Nor must it be forgotten that the *Catometopa* bear (especially the *Ocypodidæ*), an optic style which would appear to present us, in its variations, with a series of conditions transitional between that of the eye-stalk of Milne-Edwards's *Palinurus* (to which Parker appeals in seeking to show that that appendage and the antennules are homologous) and that of the ordinary podophthalmatous forms.

We congratulate the students of the University of Otago upon the good use which, in their interests, their Professor has made of the advice of his distinguished master. We cannot, however, allow to pass unnoticed the statement (p. 7) that the seventh abdominal somite (by which term Prof. Parker designates the telson) bears appendages only in Scyllarus. This is not the case, as has been previously pointed out in these pages (NATURE, vol. xxxii. p. 570). The supposed appendages, did they exist, would be at least peri-proctous in position; and, as there is reason to believe the antennules (which Parker, be it remembered, admits to be serially homologous with the ophthalmites) to have been originally peri-stomial, if not meta-stomial, the supposed peri-proctous appendages might, with equal reason, be denied homology with the other abdominal members.

Finally: the altered position of the sterna in the anterior cephalic region and the consequent displacement of their appendages is said to be "a result of the *cephalic flexure*, by which, in the embryo, the anterior cephalic sterna become bent strongly upwards." Allowance has not yet been made, in dealing with this question, for the fact that, in the Decapods, these changes are greatly exaggerated by the general enlargement of the cephalo-thoracic region, consequent upon the aggregation therein of the more important and specialized viscera, and upon specialization of the thoracic appendages for ambulation.

G. B. H.

[1] "Studies in Biology for New Zealand Students." No. 4. "The Skeleton of the New Zealand Crayfishes (*Palinurus* and *Paranephrops*)." By T. J. Parker, B.Sc., F.R.S., of the University of Otago. (Wellington: Colonial Museum and Geological Survey Department. London: Trübner and Co.)

THE MANCHESTER WHITWORTH INSTITUTE.

THE inaugural proceedings in connection with the formal organization and constitution of the Manchester Whitworth Institute took place on Thursday last, July 17. Among those present were Lord Hartington, Sir F. Leighton, Sir Joseph C. Lee, Sir J. J. Harwood, Mr. W. Mather, M.P., Sir Henry Roscoe, M.P., Mr. O. Heywood, and many representatives of educational institutions in the city.

The governors first held their inaugural gathering in the building which is to form part of the museum, and which stands in one corner of the park. Afterwards, a meeting was held in a tent in the park. At this meeting Lord Hartington said that, although he had not been aware that he would be called upon to address them before the evening proceedings, he was pleased to move a resolution which acknowledged the wise benevolence and generosity of the legatees of Sir Joseph Whitworth, and commended the Institute to the support of the public as subscribers and donors of works of art and books, and to the community of Manchester for a contribution from its municipal funds for maintenance. He described the new departure taken that day as of a very important and possibly momentous character—probably the most important and ambitious step which had been taken yet in the direction of the movement of technical and scientific instruction and art education. That undertaking was the embodiment of a great idea, and the charter of the institution appeared to have embodied the ancient idea of a University, under which various colleges independent of one another agreed to co-operate in a common management and government, while retaining a considerable independence for a common end and a common good. In one respect, however, the ancient course seemed to have been reversed, for the University was prepared to support the colleges, which were the technical and art schools, instead of the colleges supporting the University, as of old. In conclusion, he expressed a hope that the example of the Whitworth legatees would lead others, and especially the Corporation, to assist and promote the useful objects of the Institute.

The proceedings connected with the opening of the Institute were continued in the evening, when the Mayor entertained a distinguished company at a banquet in the Town Hall. The loyal toasts having been honoured,

The Mayor proposed the residuary legatees of the late Sir Joseph Whitworth.

Chancellor Christie, in responding, said it was the earnest desire of the late Sir Joseph Whitworth that his fortune should be employed in promoting the cause of education, and especially of science and art education. He desired that there should be a graduated system of schools and colleges, by which a deserving lad might rise from the lowest elementary school to the highest institutions for the teaching of science, literature, or art. How best to accomplish this exercised Sir Joseph Whitworth for many years, but he was never able to perfect a scheme. That work he left to his legatees, and they had already spent over £300,000 in carrying out what they believed to be his ideas, while other liabilities still remained.

Mr. Alderman Thompson proposed "Success to the Whitworth Institute."

The Marquis of Hartington, in responding, said that his connection with the question of technical education was an extremely slight and superficial one. He did not pretend to be be an expert on the matter, and he had only taken it up because he had been struck with the fact that every other country in Europe gave more time and money to the promotion of technical education in some form or another than did the English nation. This state of things was coincident with complaints of the great severity of the commercial and industrial competition to which we were exposed. He could not help asking himself whether there was any connection between our neglect of technical education and the increased severity of the competition to which we were exposed. Then there was another question. Suppose the severity of the commercial competition were due to other causes, were we giving ourselves every chance in neglecting the technical education of our industrial population? He thought it was scarcely possible to exaggerate the importance of this question. To us the maintenance of our place in the race of commercial and industrial competition was not a question of greater or less prosperity at any particular moment; it was not a question of being leader or follower in the world's civilization; it was for many millions of our population a question of actual existence. If, through any circumstances, we ceased to be the greatest producers of the necessaries the world required; if, through any circumstances, we ceased to be the greatest distributors of the wealth of the world, not only would these small islands cease to be the seat of a great empire, but their limited extent would fail to produce the materials of bare existence for millions of people whom our industrial supremacy alone had brought together and enabled to exist here. We had received from our predecessors a great inheritance—the commercial and industrial leadership of the world. Up to the present time that inheritance had not shrunk or dwindled. Our pre-eminence had been largely due to the natural advantages we had enjoyed, but we knew that the conditions of supremacy, such as we had hitherto enjoyed were not always permanent. History taught us that in ancient times Greece and her colonies, and in modern times Italy and Holland, enjoyed that commercial supremacy which had more lately been ours. That supremacy had passed away from those countries under the changing conditions of commercial and industrial enterprise in Europe, and it would be rash to predict that our natural advantages, to which we owed so much, were sure to continue. It would be impossible for human foresight to make adequate protection against what might happen, but it must be a great advantage to any nation when the leaders and captains of its industries and commercial pursuits were able to avail themselves of the most complete scientific education which it was possible to give. It was such considerations as these that had induced him upon more than one occasion to call the attention of his fellow-countrymen to the importance of this question. He could not pretend to do more. How these things were to be attained he left to experts to say. We might have long to wait before, by the action of the State, any measures would be taken which we might hope would place us on a footing as regarded technical and scientific education with other European nations, and it therefore gave him the greatest satisfaction to see that localities where the need was more especially felt had themselves taken the initiative, and had founded institutions for the purpose of making some advance in that which had been considered to be the business of the State in other countries. There was one feature of the present time which was calculated to give cause for just and legitimate satisfaction. He alluded to what he thought he saw in the growth of local public spirit. Such a spirit had never been altogether wanting among us. That it existed formerly among us was abundantly proved by the munificent foundations for religious, educational, and charitable purposes which our forefathers had handed down. There was a time when there was a tendency for even these ancient foundations to lapse into lethargy, and mismanagement began to prevail, but all that had begun to change, and now we had not only been occupied in reforming the abuses of those old foundations and institutions, so as to make them fully available for the new and growing wants of the people, but there had been shown to exist at the present time to as great an extent as formerly a disposition on the part of individuals who had acquired wealth in certain localities to use that wealth not for any selfish or personal purpose, but for the benefit and advantage of that population in the midst of which they had lived. He doubted not that the example which had been set by men like Sir Joseph Whitworth would be largely followed.

Sir Frederick Leighton also responded.

WEIGHTS, MEASURES, AND FORMULÆ USED IN PHOTOGRAPHY.

THE Photographic Convention of the United Kingdom, at a meeting held in the Town Hall, Chester, on June 26, considered the Report of a Committee which had been appointed to consider the weights, measures, and formulæ used in photography. The Committee consisted of W. Bedford, C. H. Bothamley (Secretary), A. Cowan, A. Haddon, A. Levy, A. Pringle, and G. Watmough Webster. The Report was drawn up by C. H. Bothamley. The following recommendations were unanimously adopted by the Convention:—

A. *Weights and Measures.*—(1) If the metric system be used, weights will naturally be expressed in grammes and measures in cubic centimetres.

(2) If the English units be used, the minim and the drachm should not be employed at all. All weights should be expressed either in grains or decimal parts of a grain, or in ounces and fractions of an ounce; all measures in fluid grains, or in fluid ounces and fractions of a fluid ounce.

B. *Formulæ.*—(3) Formulæ should give the number of *parts* of the constituents, by weight or measure, to be contained in some definite number of *parts, by measure*, of the solution. The mixture can then be made up with (*a*) grammes and cubic centimetres, or (*b*) grains and fluid grains, or (*c*) ounces and fluid ounces, according to the unit selected.

(4) The standard temperature for making up solutions should be 15° C. or 62° F. No appreciable error will be introduced by the fact that these two temperatures are not quite identical.

(5) Formulæ should give the quantities of the constituents to be contained in x parts of the finished solution, and not the quantities to be dissolved in x parts of the solvent. When a solid dissolves in a liquid, or when two liquids are mixed, the volume of the solution or mixture is, as a rule, not equal to the sum of the volumes of its constituents. The expansion or contraction varies with the nature of the solids and liquids and the proportions in which they are brought together. In making up a solution, therefore, the constituents should first be dissolved in a quantity of the solvent smaller than the required volume of the finished mixture, and after solution is complete, the liquid, cooled if necessary to the ordinary temperature, is made up to the specified volume by addition of a further quantity of the solvent.

(6) It is very important to specify, in the case of liquids, whether parts by weight or parts by measure are intended. The equivalence between weight and measure only holds good in the case of water and liquids of the same specific gravity: a fluid ounce of ammonia solution or of ether weighs less than an ounce; a fluid ounce of strong sulphuric acid weighs nearly two ounces.

(7) Whenever possible, formulæ should give the quantities of the constituents required to make up 10, 100, or 1000 parts of the solution.

(8) When a mixture (*e.g.* a developer) is to be prepared just before use from two or more separate solutions, it is desirable that the proportions in which the separate solutions have to be mixed should be as simple as possible—*e.g.* 1 to 1, 1 to 2, 1 to 3, 1 to 10.

(9) When metric units are employed, the original French spelling, "gramme," should be used in preference to the contracted spelling, "gram," in order to avoid misreading and misprinting as "grain."

SCIENTIFIC SERIALS.

IN the *Journal of Botany* for June and July we find contributions to systematic and descriptive botany by Mr. E. G. Baker, on new plants from the Andes, and on the genera and species of Malveæ; by Mr. F. N. Williams, a synopsis of the genus *Tunica* of Caryophyllaceæ, and others.—Mr. A. Fryer records what he believes to be an example of hybridity in *Potamogeton.*—Mr. H. T. Soppitt describes a new parasitic fungus, *Puccinia digraphidis*, the teleutospore-form of which occurs on *Phalaris arundinacea*, while the æcidio-form is parasitic on *Convallaria majalis.*

THE original papers in the *Nuovo Giornale Botanico Italiano* for July all refer to the geographical distribution of Italian plants, chiefly Hepaticæ and Fungi. Among the papers read at the meetings of the Italian Botanical Society the following are of special interest:—Signor O. Kruch contributes to our knowledge of the foliar fibrovascular bundles of *Isoetes.*—The exhaustive researches of Prof. Arcangeli on the structure of the various organs in the Nymphæaceæ are represented by an account of the leaves of *Nymphæa* and *Nuphar.*—Signor U. Martelli gives a very interesting account of the dissociation of a lichen (*Lecanora subfusca*) into its constituent algal and fungal elements, the complement of Stahl and Bonnier's observations on the synthesis of lichens.—Prof. Arcangeli describes the carnivorous habits of an Aroid, *Helicodiceros muscivorus.*

American Journal of Science, July, 1890.—The inconsistencies of utilitarianism as the exclusive theory of organic evolution, by Rev. John T. Gulick. The author criticizes various conclusions arrived at by Mr. Wallace in his volume on "Darwinism."—The southern extension of the Appomattox formation, by W. J. McGee. In a paper entitled "Three Formations of the Middle Atlantic Slope," published in this *Journal* in 1888, a distinctive late Tertiary formation well displayed on the Appomattox River in Eastern Virginia was defined and named after that river; and its principal characters, distribution, stratigraphical relations, and probable age were recorded. The present number contains the result of an extension of the research into the Carolinas, Georgia, Alabama, and Mississippi.—An experimental proof of Ohm's law, preceded by a short account of the discovery and subsequent verification of the law, by Alfred M. Mayer. The experiment described is very suitable for lecture demonstration, and all details are given. A low-resistance Thomson galvanometer is joined up to a box containing coils of 1, 2, and 3 ohms resistance, and to a coil of wire wound round a disk of wood which slides on an upright magnet 1·5 cm. in diameter. The quick movement of this coil causes the production of a magneto-electric current, and adopting the conception of the lines of magnetic force it may be said that a ring with one coil cuts a certain number of these lines, this cutting of the lines causes the current, and is the electromotive force. A ring with two, three, or four coils cuts two, three, or four times the number of lines, and increases the electromotive force in the same proportion. The resistance in the circuit can also be changed by means of the resistance coils, and hence it can be proved that the current is directly as the electromotive force and inversely as the resistance by observations of the galvanometer deflections.—Microscopic magnification, by W. Le Conte Stevens. If F be the equivalent focal length of the eye-piece of a microscope, f that of the objective, T the tube length, and D the distance of distinct vision, the magnification, M, is expressed by the formula $M = \frac{(D+F)(T-f)}{Ff}$.—Notes on the minerals occurring near Port Henry, N.Y., by J. F. Kemp.—Occurrence of goniolina in the Comanche series of the Texas Cretaceous, by Robert T. Hill.—A method for the reduction of arsenic acid in analysis, by F. A. Gooch and P. E. Browning.—On the development of the shell in the genus Tornoceras, Hyatt, by Dr. Charles E. Beecher.—Fayalite in the obsidian of Lipari, by Jos. P. Iddings and S. L. Penfield.—On some selenium and tellurium minerals from Honduras, by Edward S. Dana and Horace L. Wells. The locality from which the minerals were obtained is the El Plomo mine, Ojojoma District, Department of Tegucigalpa, Honduras. An analysis of one showed that it contained 29·31 per cent. of selenium and 70·69 per cent. of tellurium, the great proportion of selenium constituting it the nearest approach to native selenium which has yet been found in nature. It is proposed to call this mineral selen-tellurium. Some tellurium-iron minerals are also described.—Some connellite from Cornwall, England, by S. L. Penfield.

American Journal of Mathematics, vol. xii., 4 (Baltimore, July 1890).—This number opens with a short note (pp. 323-336) on confocal bicircular quartics, by Prof. Franklin, and closes with a memoir on the theory of matrices, by H. Taber (pp. 337-396.) The memoir is a full investigation of the subject, touching upon the results already obtained by Cayley ("Theory of Matrices," *Phil. Mag.*, 1858), Hamilton ("Quaternions," 1852), the two Peirces, and Clifford. The writer was not aware of Buchheim's paper, with an identical title, in the London Mathematical Society's Proceedings (vol. xvi.) until after his own paper was written. There is much which is substantially the same in the two memoirs, but Mr. Taber claims to have "treated the whole subject more in detail and more systematically than Mr. Bucheim" (*sic*).

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 15.—M. Hermite in the chair. New studies on the rotation of the sun, by M. H. Faye. An account is given of Dr. Wilsing's observations of faculæ for the purpose of determining the time of rotation, and of the recent work done by M. Dunér, in which Fizeau's method was adopted.—On the photography of the polarization fringes of crystals, by MM. Mascart and Bouasse. A method of obtaining photo-

graphs of these fringes is described.—On the freezing of meat by cold liquids, by M. Th. Schlœsing. A new method for freezing and preserving large quantities of meat is described.—The active elasticity of muscle, and the energy used in its creation, in the case of dynamic contraction, by M. A. Chauveau.—On linear differential equations, by M. Cels.—Method of measuring the difference of phase of the rectangular components of a refracted light-ray, by M. Bouasse.—On the measurement of the vapour-tension of solutions, by M. Georges Charpy. The author uses the condensation hygrometer to determine indirectly the tension of the vapour above the solution employed.—On the laws of Berthollet, by M. Albert Colson.—Researches on the double nitrites of rhodium, by M. E. Leidié. Double nitrites of rhodium and potassium, sodium, ammonium, and barium respectively are described, methods of preparation and properties of each salt being given.—On some combinations of camphor with phenols and their derivatives, by M. E. Léger. Many of the compounds obtained yield crystals of definite form and constant composition, and are hence proved to be true compounds.—On mannite hexachlorhydrin, by M. Louis Mourgues. The method of preparation and properties of this body are given; its analysis indicates that it possesses the formula $C_6H_8Cl_6$. Raoult's method gives its molecular weight as 278; the writer is of opinion that its constitution corresponds to $CH_2Cl(CHCl)_4CH_2Cl$.—On some new derivatives of β-pyrazol; a contribution to the study of the nitric ethers, by M. Maquenne.—Researches on the division of the embryonic cellules among the Vertebrata, by M. L. F. Henneguy.—On the colouring reagents of the fundamental substances of membrane, by M. L. Mangin. The author compares the action of colouring matters of membrane with their chemical composition, and establishes the results furnished by the colouring reagents by chemical analyses of the tissues.—On the expansion of silica, by M. H. Le Chatelier. The experiments show that amorphous silica expands very little between 600° C. and 1000° C. Quartz expands regularly up to nearly 600°, and then reaches a point where increase of temperature causes contraction. Calcined chalcedony expands slowly up to 200°, then the coefficient of expansion is enormously increased for a time, but finally it returns to the original value. Tridymite behaves much like chalcedony, expanding slowly up to about 120°, when an abrupt change takes place; the slow expansion then returns again, and finally contraction takes place with increase of temperature. Thus the change in the coefficient takes place at a higher temperature in the minerals of high density (quartz, chalcedony) than for those of lower density (tridymite and calcined chalcedony).—Analysis of the menilite of Villejuif, by M. Auguste Terreil.—On the prediction of storms by the simultaneous observation of the barometer and the higher atmospheric currents, by M. G. Guilbert.

AMSTERDAM.

Royal Academy of Sciences, June 28.—Prof. van de Sande Bakhuyzen in the chair.—Dr. Beyerinck described experiments relating to the culture of Zoöchlorella, Lichen gonidia, and other lower Algæ in a pure state.—The same speaker treated of the artificial infection of *Vicia Faba* with *Bacillus radicicola*. Twelve pots filled with sterilized river-sand, which was rendered very poor in nitrogen by washing with distilled water, were divided into four sets, each of three. On April 25, a well-sterilized seed of *Vicia Faba* was planted in each pot. The pots were of such a construction that the dust of the air was wholly excluded from the sand, and the watering could also take place under perfect dust-exclusion. The first set was watered with a mixture of 0·1 monopotassium phosphate, 0·03 calcium chlorate, 0·06 magnesium sulphate, pro 1 litre distilled water; the second set with the same mixture; the third set with the same mixture, to which was added 0·2 gr. calcium nitrate; the fourth set with the same mixture, to which was added 0·2 gr. ammonium sulphate. When the plants had developed their second leaf, the three pots of the first set, and one single pot of each of the three latter sets, were infected with a gelatine culture of *Bacillus radicicola* var. *fabæ*, cultivated in 1889 from the tubercles of *Vicia Faba*, and since that time kept in successive cultures. The bacteria wherewith the infection took place were mixed with sterilized common water. On June 20 there was found on one old cotyledon a *Penicillium*, and therefore the experiment was not further continued. All the plants were taken from the pots, and their roots well washed and examined; every single one of the six infected plants bore many tubercles, whilst no single one of the six remaining not infected plants showed the least sign of tubercles. The presence or absence of nitrogen as nitrate or as ammonium is therefore indifferent with regard to the practicability of the infection. By another set of experiments it was shown that gelatine cultures of *Bacillus ornithopi*, cultivated in 1889 from the tubercles of *Ornithopus perpusillus*, had no power to infect *Vicia Faba*. But negative results are not equal to positive in value.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Evolution of Photography: J. Werge (Piper and Carter).—Higher Geometry, W. J. Macdonald (J. Thin).—Zoological Types and Classification: W. E. Fothergill (J. Thin).—Principles of General Organic Chemistry: Prof. E. Hjelt, translated by J. Bishop Tingle (Longmans).—Philosophy of Tumour Disease: C. Pitfield Mitchell (Williams and Norgate).—Diseases of Crops and their Remedies: A. B. Griffiths (G. Bell and Sons).—Principles of Economics, vol. i.: A. Marshall (Macmillan and Co.)—Elementary Text-Book of Heat and Light: R. Wallace Stewart (W. B. Clive and Co.).—Quarterly Review, July (Murray).—The Forum, July (New York).—Electrical Engineer's Pocket-Book: H. R. Kempe (Lockwood).—Monograph of the British Cicadæ, Part III.: G. B. Buckton (Macmillan and Co.).

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THURSDAY, JULY 31, 1890.

LAVOISIER.

La Révolution Chimique : Lavoisier. Par M. Berthelot. (Paris : Félix Alcan, 1890.)

AMONGST the crop of literature which the centenary of the French Revolution has produced, there are probably no works more interesting to the historian of science in general, and certainly none more interesting to the historian of chemistry in particular, than the two biographies of Lavoisier which then appeared, the one due to the patient industry of M. Grimaux, and the other to the patriotic zeal of M. Berthelot. These works have necessarily much in common, but they differ essentially in the standpoint from which their authors regard their subject. M. Grimaux's book was the first to make its appearance. It deals more especially with the public life of Lavoisier, with his work as a *fermier-général* and at the *Régie des Poudres*, and with his labours as an economist and as a social and political reformer. To a reader but little versed in the history of science the general tendency of M. Grimaux's work is to place in high relief the political side of Lavoisier's career; and to magnify the servant of the State at the expense of the chemist. Hence it was but proper and natural that M. Berthelot, the Perpetual Secretary of the Academy, should have felt urged to set forth in a clearer light the nature of the service which his illustrious predecessor, who fought so nobly for the Academy during the dark days of the Great Terror, has rendered to science. M. Berthelot has accordingly occupied himself almost exclusively with the scientific part of Lavoisier's work. If he dwells at all on the details of his career as an administrator, it is only for the purpose of explaining the conditions which directed, controlled, or in any way modified the course of his investigations. For the greater part of these details he is mainly indebted to M. Grimaux. M. Berthelot has, however, enjoyed this advantage over M. Grimaux, that he has been in a position to study the minutes of the Academy, more especially at about the period of the Revolution, and he has had the rare privilege of being able to peruse the laboratory journals of Lavoisier, which had been preserved by the pious care of Madame Lavoisier and her descendants. These documents are of the greatest interest and importance, for they enable us not only to determine the exact time and sequence of his researches, but also to trace the gradual development of his conceptions, and the manner in which he shook himself free from the trammels of phlogistonism. These registers, thirteen in number, are deposited in the Archives of the Institute. They have been most carefully examined and collated by M. Berthelot, and a statement of the results of the analysis forms a considerable and specially valuable section of his work.

It is a remarkable circumstance, as M. Grimaux has already stated, that, in spite of the glory which surrounds the name of Lavoisier, a century should have elapsed before any substantial effort should have been made to do justice to his memory. Beyond the *éloge* by Fourcroy (inspired, there is too much reason to fear, by the extraordinary revulsion of public feeling which immediately followed the death of Robespierre), and the short biographical notices by Lalande and Cuvier, there had been no real attempt to deal with the career of the man whom his countrymen regard as the Newton of chemistry until the appearance of M. Grimaux's book. Dumas—who exercised such a predominant influence on chemical thought in France, and who throughout his life professed the most fervent admiration for Lavoisier, the official republication of whose works he superintended—never did more towards the realization of his oft-repeated intention of producing such a monograph as M. Grimaux has now given us than is to be seen in a few enthusiastic pages, more eloquent, perhaps, than exact, in his "Leçons de Philosophie Chimique." The tardiness of this reparation does not fail to strike M. Berthelot, and he ventures to discuss its cause. We do not propose to follow him in this. *Qui s'excuse s'accuse:* the conclusion is not creditable to the national fame or to its sense of retributive justice. No statue of Lavoisier is to be found in the city of his birth and death. Republican Paris is apparently unwilling to give any outward and visible sign of contrition for the great crime of May 8, 1794.

It is hardly surprising that in a book written at a time when France had invited the world to assist her to commemorate an epoch which had such a tremendous influence on her destiny, M. Berthelot should have sought and found a parallel between the work of Lavoisier and the great upheaval which so completely changed the social and political aspect of his country. The active revolt against phlogistonism no doubt had its origin in France, and Lavoisier was unquestionably the leader in the revolution. That, however, is not saying that he was the actual author of it. Black, who in this as in other matters was far ahead of the scientific thought of his age, had already convinced himself of the inadequacy of Stahl's generalization even as a theory of combustion, and Black's influence still counted for something in this country. Indeed, as Lavoisier admits, it also counted for much with at least one man in France, and that man was Lavoisier himself. He spoke of Black as "le savant illustre qui le premier a réuni et mis en corps de doctrine le phénomène de la fixation de l'air dans les corps." Black's great discovery was, in fact, the real beginning of *la révolution chimique*. M. Berthelot is constrained to admit this.

"La théorie du phlogistique recevait par là une première atteinte: les changements survenus dans les propriétés de la chaux et des alcalis caustiques se trouvant expliqués, non par la présence ou l'absence de cet agent mystérieux, comme on l'avait fait jusque-là; mais par celle d'une matière chimique déterminée, que l'on pouvait recueillir, peser, et transporter d'une combinaison dans une autre. Aussi les partisans de la théorie régnante se hâtèrent-ils de réfuter Black. Il s'engagea à cette occasion une première lutte, qui préluda à la grande discussion de Lavoisier."

Black, however, was not fitted to lead a revolution. A man of philosophic calm, gentle and somewhat retiring in disposition, he had nothing of the fire and energy of Lavoisier; he hated controversy, and was constitutionally so indolent that it was only under pressure that he could be induced to write out the results of his investigations for publication. Black, who wrote French with ease—he was born at Bordeaux, and spent much of his

youth there—frequently corresponded with Lavoisier, and next to his friend Hutton there was probably no one who knew more of his opinions on current scientific topics. We have it on the authority of Thomas Thomson that Black felt hurt at the publication of several of Lavoisier's papers in the *Mémoires de l'Académie*, without any allusion whatever to what he himself had previously done on the same subject. Thomson adds, however, that, "from the posthumous works of Lavoisier, there is some reason for believing that, if he had lived, he would have done justice to all parties; but there is no doubt that Dr. Black, in the meantime, thought himself aggrieved, and that he formed the intention of doing himself justice by publishing an account of his own discoveries; however, this intention was thwarted and prevented by bad health" ("History of Chemistry," vol. i. 330).

We have ventured to say this much in justice to Black, not because we wish in any way to disparage Lavoisier, or to minimize the greatness of his services to the philosophy of chemistry, but because we think that M. Berthelot has allowed his analogy to run away with him. To say that Lavoisier was the actual author of *la révolution chimique* is hardly more true than the statement that Marat was the author of the Revolution of 1789. The learned author of the "Introduction à la Chimie des Anciens et du Moyen Age" stops short of attempting to prove the truth of Wurtz's saying that "chemistry is a French science; its founder was Lavoisier of immortal memory;" but we cannot help thinking that the circumstances under which his book was produced have in some measure warped his critical faculty; and that, seduced by analogy—that fruitful parent of error—he has been led to claim for his hero a pre-eminence in the creation of the new order of things that the unbiassed historian could not possibly grant.

T. E. THORPE.

THE ORGANISMS INFESTING WATER-WORKS.

Die Pflanzen und Thiere in den dunkeln Räumen der Rotterdamer Wasserleitung. Bericht über die Biologischen Untersuchungen der Crenothrix-Commission zu Rotterdam, vom Jahre 1887. Erstattet von Hugo de Vries. (Jena: Gustav Fischer, 1890.)

THE water-works of Rotterdam obtain their supply of water from the River Meuse, and apparently were able to filter and purify it in a satisfactory manner until the spring of 1887, when the Schizomycete *Crenothrix Kühniana* made its appearance in great abundance in the various reservoirs and aqueducts. This gave rise to so much trouble and difficulty in obtaining a pure water-supply, that new and improved filters were made, and finally a Commission of investigation was appointed, which carried on its work chiefly during the winter 1887–88. Some further questions bearing on the matter were investigated in the following year, and now we have before us the chief scientific results of the Report sent in by the Professor of Botany at Amsterdam, Hugo de Vries, and giving a most interesting account not only of this particular pest, the *Crenothrix*, but also of the other plants and animals found living in the dark places of the Rotterdam aqueducts.

A small laboratory was fitted up in the water-tower of one of the reservoirs, so that Prof. de Vries and Dr. F. Dupont, who conducted the microscopical investigations, might have every opportunity of examining the plants and animals in a living condition. In the first part (50 pages) of the paper the attached organisms found in the aqueducts in 1887 are described, beginning with *Crenothrix Kühniana* (one of the "iron-bacteria"), of which a full account, with figures, is given. This organism was found to be undoubtedly the chief cause of the impurity of the Rotterdam water-supply, as it also had been in the case of the "water-calamity" of Berlin in 1878. Its powers of reproduction are so enormous that in a very short space of time it can spread in abundance over a wide area and render a vast amount of water impure. De Vries comes to the conclusion from his observations that *Crenothrix* does not vegetate in the soil, as had been supposed to be the case by Brefeld and Zopf in the Berlin investigations, but is derived merely from the basins and canals of unfiltered water.

The fixed plants and animals—(1) in the Meuse and in the open basins, (2) in the covered-in canals for the unfiltered water, and (3) in the dark chambers containing the purified water—are successively described, and various interesting observations noted. *Spongilla (Meyenia) fluviatilis* was found, in the dark passages, covering the walls in a thin layer, and was of a white colour, in place of being green as it is when exposed to the light. The fresh-water mussel, *Dreyssena polymorpha*, and the hydroid zoophyte *Cordylophora lacustris* were found in great abundance in some parts; lower worms, Rotifers, Infusoria, some Crustacea, and a few Molluscs in others; and a luxuriance of fresh-water Polyzoa of the genera *Plumatella* and *Paludicella* in other parts of the system—over 30 species in all being observed, and these very much the same forms which Kraepelin had found in the Hamburg aqueducts, and Potts in those of Philadelphia—while *Crenothrix* was present everywhere, apparently covering everything in great abundance. In the filters, however, and in the channels containing filtered water, only *Crenothrix* and a few other Algæ were found. The Sponges, Zoophytes, Polyzoa, and Molluscs were entirely absent.

The second part of the work deals with the free-swimming animals—the fresh-water Crustacea. Of these, two species, *Asellus aquaticus* and *Gammarus pulex* unlike the attached animals (Polyzoa, &c.), are able to penetrate into the filtered waters along with the *Crenothrix;* and in 1887 these Crustaceans developed in the purified water to such an extent as to be a perfect plague, thus giving an excellent example of the rate of increase of a species unchecked (for a time, at least) by competition. The *Gammari* and *Aselli* which penetrated to the filtered waters had the field to themselves, they had found a niche of nature previously unoccupied by any animals, they had food and other conditions necessary for life, and plenty of room, so they increased with astonishing rapidity.

In regard to their nourishment, the *Gammari* were found by de Vries to subsist upon the *Crenothrix*, which they thus to some small extent helped to keep down; while the *Asellus*, as was proved by an

examination of the fæcal pellets and of the alimentary canal, eats the wood-work used in the construction of the filters, and also the hyphæ, conidia, &c., of the Fungus *Melanomma pulvis-pyrius*. The *Aselli* are found to eat away the softer spring wood of the beams, and leave the harder autumn wood of the annual rings standing out as ridges. Consequently one important practical conclusion at which the Rotterdam Commission arrived was that the wooden beams in the filters should be removed, and their place be taken by cement, which would not afford shelter and nourishment to the Crustacea, Fungi, and other organisms. The Report is illustrated by woodcuts, and a plate giving a plan of the Rotterdam water-works, so as to show the connection between the various reservoirs and aqueducts and the course taken by the water in passing through the system.

W. A. HERDMAN.

AMERICAN GEMS.

Gems and Precious Stones of North America: a Popular Description of their Occurrence, Value, History, Archæology, and of the Collections in which they exist; also a Chapter on Pearls, and on Remarkable Foreign Gems owned in the United States. Illustrated with Eight Coloured Plates and numerous minor Engravings. By George Frederick Kunz. (New York: The Scientific Publishing Company, 1890.)

THE general dissociation in Nature of useful and ornamental materials, which has often been commented upon, finds nowhere a more striking illustration than in the North American continent. Rich as this part of the globe is in coal, the ores of iron, and of almost all the metals employed in the arts, as well as in all kinds of building materials, yet the value of gem-stones found within its limits is practically insignificant. As the author of the work before us admits,

"the daily yield from the iron and coal mines, or from the South African diamond mines, or a week's yield of the granite quarries, would exceed in value the entire output of precious stones found in the United States during a year."

Small though their aggregate value may be, however, there are many facts concerning the variations in character and the mode of occurrence of these interesting and beautiful objects, the gem-stones, which can better be studied in North America than in any other part of the world. Nor could we possibly wish for a more fully informed guide than Mr. Kunz: his skill as a mineralogist is well known, and he has frequently, in his capacity of gem-expert to Messrs. Tiffany and Co., been able not only to reject the spurious but to recognize for the first time the latent capabilities of mineral varieties not previously employed as gems. Since the year 1883, Major J. W. Powell, the Director of the U.S. Geological Survey, has published a valuable series of annual volumes on "The Mineral Resources of the United States"; and the chapters on precious stones in these reports have been written by Mr. Kunz.

The book aims at combining the exact information required by the mineralogist with the curious and sometimes trivial, but by no means unimportant, lore dear to the collector and the archæologist. As is fitting in such a work, the typography and illustrations are of remarkable excellence, and reflect the highest credit upon the printers and engravers of the United States; indeed, it would be hard to find anywhere a volume which combines so many excellences, alike in the paper, printing, plates, and binding.

Every care has evidently been given to making the scientific part of the work trustworthy; and we may especially refer to the chapters which deal with the corundums, the beryls, and the felspars of the United States, as containing much new and valuable information. The details concerning the silicified ("jasperized") woods of Arizona given in this book are more complete and satisfactory than any that have before appeared, while the accounts of the pearls and pearl-fisheries of North America are full of interest. In order to make the work more complete, the twelve chapters on the gems of the United States are followed by two others on the precious stones of Canada and Mexico respectively. Little more than an enumeration of the gem-stones of the Dominion can be given in the space at the command of the author, but more justice is done to the jades, opals, and obsidians of Mexico.

The two last chapters deal respectively with the lapidaries' work performed by the aborigines of North America, and the work of the same kind now being done in the country; and both chapters abound with curious and interesting facts. The publication of this book cannot fail to call attention to the importance of systematic searches being carried on with a view to the discovery of some of the more valuable gem-stones, in districts where no authentic account of their occurrence at present exists. Collectors, archæologists, jewellers, and dealers will all find their respective wants anticipated by Mr. Kunz; and by attention to the methods of discrimination and the detection of fraud which he indicates, will be saved frequent disappointment and much pecuniary loss.

J. W. J.

OUR BOOK SHELF.

Timbers, and how to Know Them. By Dr. R. Hartig, Professor of Botany in Munich University. Translated by W. Somerville. (Edinburgh: Douglas, 1890.)

THE original of this little book is the third edition of a small pamphlet entitled "Die Anatomischen Unterscheidungsmerkmale der wichtigeren in Deutschland wachsender Hölzer," and why the translator should have altered the significance of the title is not explained. In any case it would not be easy to justify the more ambitious title of the English translation, seeing that no additions have been made to the original, and that the original title claims too much. For the book does little more than give in bare outline the more conspicuous features observed on the transverse sections of our common woods; and although this is done fairly well, the treatment is neither exhaustive nor free from defects.

The only other alterations made by the translator are the additions of an index and a glossary. The former appears adequate and useful, the latter has shortcomings, especially under the headings "bordered pit," "parenchymatous cells," &c. Definitions such as "*Vertical resin-duct*, one which runs longitudinally, *i.e.* parallel to the outside of a stem," are, to say the least, not improved by the additional remark.

With regard to the actual translation, it is good and

accurate on the whole: so faithful is it that Mr. Somerville has omitted to correct Hartig's own mistake as to the generic name of the teak, which reappears in the English edition as *Tectonia*—surely the translator knows it should be *Tectona!*

The chief defects in the original pamphlet may be summed up in that characters are relied on for distinguishing closely allied woods which do not serve the purpose. For instance, the broad medullary rays, so called, of the alder are a very treacherous guide; and the admission that the wood of *Æsculus* "occupies a position midway between" the hard and soft woods, itself shows how useless the property of hardness is, as a class character, unless defined in a rigid manner.

Both the selection and the description of the seven exotic timbers mentioned in the appendix are faulty, and we are driven to the conclusion that there is room for a much better book on the subject than the little pioneer under review. As a pioneer, however, it is to be welcomed, with its useful, compact information, as well as its failings.

Advanced Physiography (Physiographic Astronomy). By John Mills. (London: Chapman and Hall, 1890.)

THE introductory part of this book is a reprint of the elementary lessons in the subject by the same author (NATURE, vol. xlii. p. 76), and the remainder is intended to meet the requirements of advanced students in connection with the Science and Art examinations. The new material constitutes a fair general outline of the subject, but some of the descriptions suffer from want of detail. There are also indications of the author's unfamiliarity with some parts of the subject. On p. 248, for example, it is evident that the author is not well acquainted with stellar nomenclature, as he does not seem to be aware that Roman capitals are reserved for recently discovered variable stars. Again, on p. 253, he gives some figures relating to variable stars, which he evidently does not understand; he forgets to point out that Dunér's observations of stars were all of one spectroscopic group, and that the numbers given show that the maximum of variability occurs in that particular group. It should be an author's duty to use no term which he has not explained, but on page 114 he refers to the moon's mean horizontal parallax, although the meaning is not even hinted at.

The excellent plan of writing a head-line over each important paragraph has been adopted, but has not been employed consistently throughout. Thus, under the heading "To weigh a planet having a satellite," we find also a reference to the determination of the masses of the moon and the satellites of Jupiter and Saturn; and again, the chapter headed "Celestial Photography" consists largely of terrestrial magnetism.

The illustrations are numerous, but of varying quality; it is difficult to imagine what kind of telescope would give such a view of the moon as that represented in Fig. 93.

Travels in Africa. By Dr. Wilhelm Junker. Translated from the German by A. H. Keane, F.R.G.S. (London: Chapman and Hall, 1890.)

THE work of which this is a translation records Dr. Junker's experiences as a geographical explorer from the year 1875 to 1878. Besides an excursion to the Siwa Oasis and Natron Valley, it includes "a careful survey of the Báraka watercourse, wanderings through Upper Nubia, an expedition to the Sobat River, and numerous journeys throughout Makaraka Land and surrounding regions." It is to his later work that Dr. Junker chiefly owes his fame as an explorer; but in the present volume he gives an account of many notable achievements, and, as the translator points out, his descriptions of Makaraka Land and neighbouring districts will supply cartographers with plentiful material for filling up their blank spaces in an extensive region. Dr. Junker is a good writer as well as a bold and scientific traveller, and no one who begins to read his narrative will find it hard to go on to the end. The translator has done his work carefully, and the interest of the story is much increased by a valuable map and many good illustrations.

Selected Subjects in connection with the Surgery of Infancy and Childhood. By Edmund Owen, M.B., F.R.C.S. (London: Baillière, Tindall, and Cox, 1890.)

IN this volume Dr. Owen has published (by request) the Lettsomian Lectures delivered by him at the Medical Society of London in the present year. The position of Lettsomian Lecturer has been held by so many illustrious members of the profession that he seems to have undertaken with diffidence the task entrusted to him. The subjects with which he decided to deal have of late, as he says, been attracting considerable attention; and no one can doubt that they are of great practical importance. Dr. Owen discourses on them not only with learning, but with the directness, clearness, and force that spring from careful and long-continued observation.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Correspondence on Russian Transliteration.

As absence from England prevented our replying at the time to the last letters on the system of Russian transliteration proposed in NATURE (vol. xli. p. 397), we thought it best to delay reply till any further communications from foreign correspondents had arrived. Since our last note (NATURE, vol. xli. p. 535) four letters have been received:—

(1) Mr. Wilkins (NATURE, May 22, p. 77) writes from Tashkend to point out that the system fails to distinguish between the few Russian words which differ only in their final semi-vowel. This is quite correct, but could only be avoided by the adoption of a separate symbol for each of these two characters and their retention at the ends of words. The addition to the trouble of printing that this would involve would be far more serious than the chance of error: *krob*, a roof, is hardly more likely to be confused with *krob*, blood, than, in a quite analogous case, is the German *band*, a volume, with *band*, a ribbon.

We do not accept Mr. Wilkins's criticism that *ui* does not "even remotely" represent the sound of ы. In the use of the letter in such a word as Полный we fully admit that this is so; but in other cases, as after a labial, it seems to us to represent the sound fairly well. Phonetically, Prībuilov (to take Mr. Wilkins's own case) is not so exact as Prĭbūəlǫff, as the word would probably be rendered by the elaborate refinements of the "Historical English Dictionary"; but even this is inadequate. We despair of any correct phonetic rendering of Russian words in English characters till a system is arranged on the lines of Dr. Murray's; and then the word would appear in some such guise as ꟼɹĭbūəlǫf!f. *Ui* seems to us on the average, and certainly in the case chosen by Mr. Wilkins, a better phonetic equivalent than *y*—a letter which is unfairly overworked in nearly all systems of transliterations, and which we have reserved exclusively for the double symbols *ya, ye, yu*.

(2) Mr. Wilkins, and our second critic, Baron Osten-Sacken (NATURE, May 22, p. 77), agree in condemning the adoption of *zh* for ж. A strong case can no doubt be made out for the claim of *j* to represent that letter, and *zh* was accepted (largely on phonetic grounds) as one of those mutual concessions which Baron Osten-Sacken commends. *Zh* has been largely used—almost universally in America—and it represents the sound better than the English *j*. No doubt the French *j* of *jour* is as near to it as the *zh* sound in the word *az(h)ure*. As the system proposed was intended for English-speaking countries we thought it inadvisable to adopt a French sound for one letter. The system is not so ambitious as Baron Osten-Sacken suggests it

should be. The methods of transliteration used in Germany and France differ so much from one another, and both from the English, that it seemed hardly possible, however desirable, to get one system adopted for the three languages.

In regard to Baron Osten-Sacken's other points, we regard *tch* and *stch* as inadvisable for ч and щ respectively, as, without the use of brackets, they would be mistaken for other sequences; and it was generally agreed that brackets should only be used as a very last resource. Finally, *æ* for the rare and practically extinct ѵ is as good a phonetic equivalent as anything else, and more convenient than the already overburdened *y*.

(3) We cordially agree with the main point in Mr. Chisholm's argument (NATURE, May 1, p. 6), viz. that different systems are required for different purposes. The phonetic method adopted by the Geographical Society is unquestionably the best suited for their maps, but is quite inadequate for bibliographic use. We are glad to see that the system proposed for the latter purpose has Mr. Chisholm's approval.

(4) Mr. Groves (NATURE, May 1, p. 6) quotes a case that strikes him as very cumbrous; but we fail to see that the alteration to SKRJIPSKY is a sufficient improvement to be worth the inconveniences that the changes would involve in other cases. Under no system could a page of transliterated Russian hope to read like Addisonian English.

We do not see that, in the cases we quoted in our reply to Mr. Groves's former letter, we really misunderstood him. We do not know whence or how the three Gazetteers derived their renderings of Nizhniĭ; we only quoted them to show how many different spellings were current, and that the word, as transliterated by the new system, is neither unintelligible nor materially different from forms already in use. In the cases to which we attached most weight, viz. the titles of journals quoted from Scudder, Bolton, and the *Geological Record* the transliterations were certainly derived direct from the Russian, and we thought it probable that the Chemical Society's Journal would also have quoted the papers from the original rather than from second-hand German sources.

In conclusion, it is advisable again to repeat that the system was proposed solely for bibliographic use in the English language: the bibliographers who laid down the requirements of the system insisted that it should be based on two principles and should satisfy four rules. Had these been published in the original note, some subsequent criticisms would probably not have been made. Considering that the criticisms that really apply to this non-phonetic, unæsthetic system do not require any changes to be made in it, we hope that it will be adopted by other journals and catalogues. A supplementary list of those that do so will be published in the fuller account and explanation of the system that will be issued shortly.

H. A. MIERS.
J. W. GREGORY.

Discovery of a New Comet.

WHILE sweeping the northern sky at 11h. 35m. on the night of July 23, with a 10-inch reflector, power 40, I found a nebulous object near θ and ζ Ursæ Minoris, which I could not identify. It was faint, round, about 1′ in diameter, and with a very slight central condensation. I noted its position relatively to the stars near, but clouds then came over and prevented further observations for nearly an hour. On reobserving the object I found it to be a comet, a considerable displacement having occurred in its position.

On July 24 I obtained another view of it, and found its diurnal motion to be about 55′ to the south. At 11h. the comet was close to a star of about the ninth magnitude. At 11h. 40m. the comet was centrally projected upon the star, and the latter appeared to be involved in an extensive atmosphere. At about 12h. 30m. the comet reappeared on the other side of the star. I could not resist the impression that the star was decidedly fainter when the comet was passing over it.

The rough estimated positions of the comet were:—

	h.	° °
July 23,	12	228 + 78
,, 24,	12	228 + 77

On July 25 and 26 the sky was cloudy and the comet not seen. It will be invisible during moonlight, but on about August 5 or 6 it ought to be picked up before moonrise in the region between γ Ursæ Minoris and ι Draconis, or at about 226° + 66°.

It is probable that this comet is approaching its perihelion and becoming brighter, in which case it may be readily seen when the sky is clear and free from moonlight.

Bristol, July 27. W. F. DENNING.

P.S.—The comet was observed at Nice on July 25, 10h., when its R.A. was 15h. 14m. (228½°), Decl. 76° 37′ N.

The Rotation of Mercury.

IN the February number of *Himmel und Erde*, and elsewhere, I have seen "that the otherwise meritorious, but in his observations and their discussion not always cautious and strict Schroeter," took the rotation period of Mercury to be twenty-four hours, but that Schiaparelli has now found that Mercury behaves to the sun as the moon to the earth, always showing the same side. The reporter also explained, by way of compliment to Prof. G. Darwin, why the planet next to the sun should differ in this respect from its companions.

As all astronomers like fair play, I went through Schroeter's papers, and read Schiaparelli's letter, No. 2944, *Astronomische Nachrichten*, which shows the usual industry, lucidity of style, and good faith of the Professor.

Schroeter and his companion Harding found the southern hemisphere of the planet rounded (*rundlich*) like the northern, but believed that they saw every twenty-four hours a certain change of form of the southern end of the lighted crescent, not perceptible in the northern. This was the leading observation, of which Schiaparelli remarks: "Of all facts known with regard to Mercury's rotation, this reappearance about every twenty-four hours of a truncation of the southern horn is the most manifest and anciently known" ("un apparente troncatura del corno australe è il più manifesto e anticamente cognosciuto").

Schroeter and Harding had for some time tried in vain to trace on the face of the planet some spot confirming their conclusion, when one day Harding first and Schroeter afterwards perceived a dark streak appearing in the east and moving west over the face of the planet. Both observed the phenomenon, with varying distinctness and under different combinations, during many days, and held that it confirmed their original hypothesis. Schroeter found that considerable increase of the magnifying power of his instrument lessened the distinctness of the shading.

Schiaparelli commenced his investigation because he considered Schroeter's result doubtful, and had instruments far superior to those used ninety years ago.

He has observed Mercury since 1881 more than 500 times, and has made on the most favourable days about 150 drawings, "to say the truth of very unequal value, but nevertheless so far agreeing as to furnish a result." That is, drawings on which the admittedly indistinct and varying feeble shadings united into one dissolving view did not always appear to be the same. The author also tells us that he made "one of his best observations when the planet was only at 3° 2′ from the limb of the sun," and "the disk of the planet then appeared perfectly round and uniformly bright;" and finally confesses, "Of these forms and streaks I have endeavoured to give an idea on the annexed drawing without concealing from myself the futility of such an attempt."

The Professor first made use of his "eight-inch instrument," then of "the eighteen," which showed the shading less distinctly, so as to make him write, "I have the impression that if one looked with a still stronger instrument all would appear dissolved in still more minute formations."

He lastly formulates three hypotheses: (1) the period is 24 hours; (2) the period divides the 24 hours without remainders; (3) there is no rotation properly speaking. He adopts No. 3, and concludes that the different appearances are caused by the great libration consequent upon the large eccentricity of its orbit.

Should there be no farther hypotheses possible, when the results of research are so conflicting, indistinct, and variable?

R.

Birds and Flowers.

IN reference to Mr. Wallace's letter (p. 295) with regard to a note in NATURE (July 17, p. 279), I correctly quoted Mr. Scott-Elliot's remark, who says:—

"I am led to entirely disagree with Mr. Wallace's opinion that the colour of flower-seeking birds is quite unconnected with their habits. As a matter of fact a peculiar shade of red found

on the breast of *Cinnyris chalybea*, *C. afra*, *C. famosa*, *C. sonimanga*, and *C. bicollaris* is exactly the same as that which I found in the majority of ornithophilous flowers in South Africa. It is, moreover, not a common colour in flowers; and since Labiatæ, Aloes, Irids, and Leguminosæ all assume it when they become ornithophilous, some reason must be shown why the simple explanation given by Darwin should be set aside while no other is offered."

The Writer of the Note.

CHELSEA BOTANIC GARDEN.

THE physic garden at Chelsea covers an area of between three and four acres. It stands by the side of the Thames at the east end of Cheyne Walk, opposite Battersea Park, a short distance west of Chelsea Hospital. On three sides it is inclosed by a high brick wall, and on the fourth you look through iron railings on to the Thames Embankment and the river. Within this area there are a dwelling-house, rooms for the gardeners, a large lecture-room, and four conservatories, and the rest is laid out in walks, flower-beds, and grassy interspaces. It is now too much surrounded by houses for trees to prosper, but one of the cedars of Lebanon planted in 1683 still survives. Amongst the others may be seen, or were until lately, well-grown examples of Oriental plane, Salisburia, Wistaria, hawthorn, black walnut, black mulberry, and many others. One of the most striking features of the garden is a large bed of yuccas on the north. It contains one of the finest collections of the different species and hybrids of rhubarb to be found anywhere in the country. The most valuable portion of its contents is a collection of between 300 and 400 hardy plants and shrubs, which are or have been used in medicine. These are arranged, shrubs and herbaceous plants intermixed, according to the system of Jussieu and De Candolle. There is a smaller collection arranged after the system of Lindley, who for many years directed the garden and gave the lectures. From these are sent up the plants which are required for the examinations which are held in the old hall of the Company near Blackfriars Bridge. Against the wall that flanks the garden on the east are nailed the fig and other tender shrubs, and beneath there is a narrow border containing Ferula, Verbascum, Acanthus, the fibre-yielding Chinese and Indian *Boehmeria nivea*, and a crowd of other herbaceous plants. In the centre of the garden there is a statue of Sir Hans Sloane, and a tank full of buckbean and water violet, surrounded by rockwork on which grow saxifrages, Hieracia, and spiny Astragali. South of the main walk that cuts the garden into two halves are beds full of non-medicinal plants, arranged in natural orders, another tank full of water lilies, bur-reeds, and bulrushes, and south of all have lately been laid out a couple of beds containing types of the twenty natural orders a knowledge of which is required for the elementary examination of the Science and Art Department. The present rainy season has suited the garden capitally, and during many years' acquaintance with it the writer of this article has never seen the herbaceous plants look more luxuriant than they do at the present time.

It would take up more space than we can spare to say even a few words about each of the distinguished botanists who have been connected with the garden. Here was laid the foundation of the classical "Gardener's Dictionary" of Philip Miller, which was first published in 1731, ran through eight editions in his life-time, has been translated into German, French, and Dutch, and formed the foundation and model of the many gardeners' dictionaries that have since been written. Amongst the well-known botanists of older date who were more or less connected with the garden, were Doody, Petiver, Hudson, Rand, and Alchorne, and in later times Lindley, Fortune, Thomas Moore, Curtis, Anderson, and David Don. Full particulars about all these will be found in Field's history of the garden, published in 1820, and a second edition, considerably enlarged, published by Dr. Semple in 1878.

The ground was originally taken by the Apothecaries' Company in 1673, as a spot on which to build a convenient house for their ornamental barge. In 1674 a wall was built round the open space, and the cultivation of medicinal plants commenced. At first the ground was rented, at a nominal sum, from Lord Cheyne, who was then lord of the manor of Chelsea. In 1712 the property was purchased by Dr. (afterwards Sir Hans) Sloane. In 1722, Sir Hans Sloane granted the use of the ground in perpetuity to the Apothecaries' Company at a yearly rent of £5, to the end, says the deed, "that the said garden may at all times hereafter be continued as a physic garden, and for the better encouraging and enabling the said Society to support the charge thereof, for the manifestation of the power, wisdom, and glory of God in the works of the creation, and that their apprentices and others may better distinguish good and useful plants from those that bear resemblance to them that are hurtful." If these conditions are not fulfilled by the Apothecaries, the garden reverts to the Royal Society on the same terms, and if they fail to fulfil them it falls to the College of Physicians. Under this deed the Society of Apothecaries has now held the garden for 170 years, during which time, of course, the land has greatly increased in value.

At the present time the garden is used for botanical purposes by four classes of students:—

Firstly, those who are going up for the preliminary examination of the Apothecaries' Company, in which materia medica is one of the principal subjects. This examination, we understand, is often taken by those who seek places as chemists and druggists, and who do not intend to proceed to the L.S.A. Secondly, the ladies who compete for the silver medal which has lately been offered annually by the Apothecaries' Company. Thirdly, pharmaceutical students. One of the largest private pharmaceutical schools is situated in the neighbourhood. Fourthly, students who are intending to go up for the botanical examinations of the Science and Art Department. For this there have been about 3000 entries per annum for many years, and 25 per cent. of the marks (30 per cent. being a second class pass) are allotted for a description of a plant and a diagnosis of its natural order. Probably we should be justified in estimating that a quarter of these 3000 candidates live in London, and cannot get living specimens to study without undertaking a railway journey, and of course it is only fair to assume that those who have passed their examination will continue to take an interest in the science, particularly as many of them teach botany in elementary schools. It is only the first and second of these four classes of students who have any direct claim on the Apothecaries' Company, but they have always construed liberally the "others" mentioned in Sir Hans Sloane's deed. Last year the number of admissions by students' tickets, as registered in the visitors' book, was 3000. A course of twelve lectures and demonstrations have been given for many years in summer by Mr. J. G. Baker, and at these the annual attendance ranges from 550 to 700, or an average of 50 or 60 students to each lecture.

The Society of Apothecaries have given no public intimation that they are dissatisfied with the present condition of things, but they bear the whole expense of keeping up the garden, and reap only a share of the benefit. A Committee has been appointed by the Royal Society to consider their position in the matter; and last week a meeting was held in the Town Hall at Chelsea, at which Lord Meath presided and Prof. Flower was one of the

speakers, at which the following resolution was passed: "That this meeting of the inhabitants of Chelsea, having heard that there is a probability of the old physic garden on the Chelsea Embankment being no longer kept up by the Apothecaries' Company, considers that every effort should be made to preserve it for the public as an open space." Under these circumstances we wish to put in a plea that the claims of the London students of systematic botany and materia medica should not be overlooked, or the scantiness of their opportunities for the study of living plants forgotten.

THE SEARCH FOR COAL IN THE SOUTH OF ENGLAND.[1]

(1) THE bare facts of the recent discovery of coal-measures at Shakespeare Cliff, near Dover, have been published in the press, and the full account cannot be written till the completion of the inquiry which is now going on. It is, however, not unfitting that the bearing of the discovery on the general question of the existence of workable coal-fields in Southern England should be discussed within these walls, not merely on account of its general interest, but because it naturally follows the paper read by Mr. Godwin-Austen before the Royal Institution, in 1858, "On the Probability of Coal beneath the South-Eastern parts of England." In 1855 he had placed before the Geological Society of London the possibility of the existence of coal in South-Eastern England at a workable depth. In the two years which had elapsed, "the possibility" had grown in his mind into the "probability," and in the thirty-two years which have passed between the date of the paper before this Institution and the present time, "the probability" has been converted into a certainty by the recent discovery at Dover. In this communication, the lines of the inquiry laid down by Godwin-Austen will be strictly followed. We must first examine the conditions under which the coal-measures were accumulated.

(2) The seams of coal are proved, by the surface-soil traversed by roots and rootlets, to which in some cases the trunks are still attached, to have been formed *in situ* by the growth and decay of innumerable generations of plants (*Lepidodendra*, *Sigillaria*, *Calamites*), pines (*Trigonocarpa*, *Dadoxylon*, *Sternbergia*) allied to *Salisburia*, and a vast undergrowth of ferns, all of which contributed to form a peat-like morass. Each seam represents an accumulation on a land-surface, just as the sandstones and shales above it point to a period of depression during which sand-banks and mud-banks were deposited by water. The fact also that the coal-seams in a given sinking are parallel, or nearly parallel, implies that they were formed on horizontal tracts of alluvium, while the marine and fresh-water shells in the associated sandstones and shales prove that they were near the level of the sea, or within reach of a mighty river. This tract of forest-clad marsh-lands, as Godwin-Austen and Prestwich have pointed out, occupied the greater part of the British Isles, from the Highlands of Scotland southwards as far as Brittany, and eastwards far away into the valley of the Rhine, and westwards over the greater part of Ireland. It swept round the hills of South Scotland and the Lake district and the region of Cornwall. It occupied a delta like that of the Mississippi, in which the forest-growths were from time to time depressed beneath the water-line, until the whole thickness of the coal-measures (7200 feet thick in Lancashire, 7600 in South Wales, and 8400 in Somersetshire) was built up. After each depression the forest spread again over the sand and mud of the submerged parts, and another peat-layer of vegetable matter was slowly accumulated above that buried beneath the sand and mud. The great extent of this delta implies the existence of a large river draining a large continent, of which the Highlands of Scotland and the Scandinavian peninsula formed parts, and which I have described before the Royal Institution under the name of Archaia.

(3) At the close of the Carboniferous age, this vast tract of alluvium was thrown into a series of folds by earth-movements. These have left their mark in the south of England and the adjacent parts of France, in the anticline of the English Channel, the syncline of Devonshire, the anticline of the Mendip Hills and of the lower Severn, and the syncline of the South Wales coal-fields. These great east and west folds have been traced from the south of Ireland on the west, through 35 degrees of latitude, through North France and Belgium, as far as the region of Westphalia. Next, the upper portions of the folds were attacked by the subaërial and marine agents of denudation over the whole of the Carboniferous area, leaving the lower parts to form the existing coal-fields which lie scattered over the surface of the British Isles, and are isolated from each other by exposures of older rocks; and a broad east and west ridge was carved out of the folded and broken Carboniferous and older rocks, extending from the anticline of the Mendip Hills eastward through Artois into Germany, and constituting the ridge or axis of Artois of Godwin-Austen.

The next stage in the history of the folded Carboniferous and older rocks is marked by the deposition of the Permian and Secondary rocks on their eroded and water-worn edges, by which they were partially concealed or wholly buried, and these newer strata thin off as they approach the ridge of Artois. This barrier, also, of folded Carboniferous and older rocks sank gradually beneath the sea in the Triassic, Liassic, Oolitic, and Cretaceous ages, and against it the strata of the first three named ages thin off, while in France and Belgium the Cretaceous deposits rest immediately upon the water-worn older rocks.

From these general considerations it is clear that the coal-measures which formerly extended over nearly the whole of Southern England can now only be met with in isolated basins under the newer rocks, and that these are thinnest along the line of the above-mentioned barrier.

(4) The exposed coal-fields in Britain, and on the Continent also, Godwin-Austen pointed out, along this line, are of the same mineral character, and the pre-Carboniferous rocks are the same. This ridge or barrier also, where it is concealed by the newer rocks, is marked by the arch-like fold (anticlinal) of the chalk of Wiltshire, and by the line of the North Downs in Surrey and Kent. Godwin-Austen finally concluded that there are coal-fields beneath the Oolitic and Cretaceous rocks in the south of England, and that they are near enough to the surface along the line of the ridge to be capable of being worked. He mentioned the Thames Valley and the Weald of Kent and Sussex as possible places where they might be discovered.

These strikingly original views gradually made their way, and in the next eleven years became part of the general body of geological theory. They were, however, not accepted by Sir Roderick Murchison, the then head of the Geological Survey, who maintained to the last that there were no valuable coal-fields in Southern England.

(5) The next important step in the direction of their verification was that taken by the Coal Commission of 1866-67, by whom Mr. Godwin-Austen was examined at length, and the results of the inquiry embodied in the Report by Mr. Prestwich. In the Report Mr. Godwin-Austen's views are accepted, and fortified by a vast number of details relating both to the coal-fields of Somersetshire and of France and Belgium. Mr. Prestwich also calls special attention to the physical identity of the coals of these two regions, and to the fact that the Carboniferous and older rocks in both are similarly dis-

[1] Friday Evening Lecture delivered at the Royal Institution on June 6, by Prof. W. Boyd Dawkins, F.R.S.

turbed. He concludes, further, that the coal-fields which now lie buried beneath the newer rocks are probably equal in value and in extent to those which are exposed in Somerset and South Wales on the west, and in Belgium and France on the east.

We will now proceed to test these theoretical conclusions by the light of recent observations.

(6) The coal-fields of Somerset and Gloucester were proved by the labours of Prof. Prestwich and the Coal Commission of 1866–67 to be small fractions of the great coal-basin which lies buried beneath the Triassic, Liassic, and Oolitic rocks, from the Mendip Hills northwards past Bristol to Wickwar. On the west also three small isolated coal-basins occur—those of Nailsea and Portishead, which are partially, and that of Aust, which is wholly, concealed by the newer rocks. The coal-measures are folded and broken, and traversed by great "overthrust" faults, which at Kingswood give the same series of coals twice over in the sinkings of one colliery. Their southern boundary is the line of the Mendip Hills. They also probably occur at a depth which remains to be proved, still further to the south, in the valley of the Axe and the district of Glastonbury, the most southern boundary being the mountain limestone of Cannington, near Bridgwater. The great Somerset and Gloucester field may extend to the east under the newer rocks, between Freshford and Beckington, in the district south of Bath.

The value of the evidence of the coal-fields of the west of England on the general question consists in the fact that they may be taken as fair samples of those which lie concealed along the line of the buried ridge through South-Eastern England in the direction of France, Belgium, and Germany.

(7) One of these concealed coal-fields has been struck in a deep boring at Burford, near Witney, in Oxfordshire, at a depth of 1184 feet, under the following rocks:—

	Feet.
Oolites	148
Lias	598
Rhætic	10
Triassic rocks	428

The sandstones and shales of the coal-measures were penetrated to a depth of 225 feet (De Rance, Manch. Geol. Soc., March 26, 1878).

These coal-measure rocks form, as suggested by Hull, one of the same series of coal-basins as those of South Wales and the Forest of Dean, and probably mark the line of the continuation of the South Wales syncline in the direction of Harwich, where Carboniferous shale has been struck at a depth of 1052 feet from the surface.

This boring proves not merely the presence of coal-measures at a workable depth in Oxfordshire, but also the important fact that the Triassic rocks, which are of great thickness further north, have dwindled down to an unimportant thickness in their range southwards and eastwards. Further, that south, in the London area, these rocks are wholly absent; and farther to the east, at Harwich, the Liassic and Oolitic strata and Lower Greensand are absent, and the Gault rests on the eroded Lower Carboniferous rocks, inclined at a high angle.

(8) The water-worn surface of the folded rocks, which are older than the Carboniferous, has been repeatedly struck in deep borlngs for water in the neighbourhood of London, at depths ranging from 839 feet at Ware to 1239 feet at Richmond. They consist of Silurian strata in the north at Ware, and of Old Red Sandstone or Devonian rocks in the other localities. From their high angle of dip, as in the case of similar rocks underlying the coal-fields of Somerset and Northern France and Belgium, it may be inferred that coal-fields lie in the synclinal folds in the neighbouring areas.

From the fact of the Silurian rocks being in the north, while all the rest of the borings to the south terminate in the Devonian or Old Red rocks, it may be inferred that the chalk of the North Downs probably conceals the coal-measures. It must also be noted that there are no Wealden rocks in the London area, and no Lower Greensands, and that the Lower Oolites at their thickest are only 87 feet. The secondary rocks, which are of great thickness in the midland and northern counties, thin off as they pass southwards towards London, against the ridge of older rocks, as both Austen and Prestwich have pointed out.

It is therefore in the area south of London, rather than in that immediately to the north, that the coal-measures are to be looked for at a workable depth beneath the surface, and underneath the chalk of the North Downs. It must, however, be noted that the line of the South Wales syncline through Burford passes to the north of Ware, and that there may be coal-measures in the northern parts of Essex and of Hertfordshire at a workable depth.

(9) The Report of the Coal Commission was published in 1871, and in the following year the Sub-Wealden Exploration Committee was organized by Mr. Henry Willett, to test the question of the existence of the Carboniferous and pre-Carboniferous rocks in the Wealden area by an experimental boring. The site chosen was Netherfield, about 3 miles south of Battle, in Sussex, where the lowest rocks of the Wealden formation constitute the bottom of the valley. The rocks penetrated were as follows:—

Section of Netherfield.

	Feet.
Purbeck strata...	200
Portland strata	57
Kimmeridge clay	1073
Corallian strata	515
Oxford clay	60
	1905

This boring showed that the coal-measures and older rocks are, in that region, more than 1900 feet from the surface of the ground. We may also infer, from the fact of the bottom of the bore-hole being in the Oxford clay, and from the known thickness of the Bath Oolitic strata in the nearest places, that it lies buried beneath considerably more than 2000 feet of newer rocks. With this valuable, though negative result, the Sub-Wealden exploration came to an end. It was a purely scientific inquiry, paid for by subscription, and largely supported by those who had no pecuniary interest in the result.

The experience of the boring at Netherfield showed that the search for the coal-measures and older rocks of Godwin-Austen's ridge would have to be carried out at some spot further to the north, in the direction of the North Downs. In the district of Battle the Oolitic rocks were proved to be more than 1700 feet thick, and the great and increasing thickness of the successive rocks of the Wealden formation above them, which form the surface of the ground between Netherfield and the North Downs, rendered it undesirable to repeat the experiment within the Wealden area proper, where the Wealden rocks presented a total thickness of more than 1000 feet, in addition to that of the Oolites. My attention, therefore, was directed to the line along the North Downs, where Godwin-Austen believed that the Wealden beds abruptly terminated against the ridge of coal-measures and older rocks, and where, therefore, there would be a greater chance of success.

(10) The evidence, also, of the French, Belgian, and Westphalian coal-fields pointed in the direction of the North Downs.

The Carboniferous and older rocks, which we have hitherto traced only as far as the area of London from their western outcrops in Somerset, Gloucestershire, and South Wales, reappear at the surface in Northern France,

Belgium, and Westphalia, and contain most valuable coal-fields, which are long, narrow, and deep. These extend from the district of the Ruhr on the east, through Aachen, Liége, Namur, Charleroi, Mons, and Valenciennes. The enormous value of the last field led, during the last hundred years, to numerous borings through the newer rocks, which have extended the western range of the coal-measures upwards of 95 miles away from its disappearance under the Oolites and chalk, as far as Flechinelle, south of Aire, or to within 30 miles of Calais. It occupies throughout this distance a narrow trough or syncline, 11 miles across at Douchy, and about half a mile at its western termination. It is represented still further to the west by the faulted and folded coal-fields of Hardinghen and Marquise, which are within about 12 miles of Calais. The coal-measure shales and sandstones found in a boring at Calais, at a depth of 1104 feet from the surface, in 1850,[1] reveal the existence of another coal-field in the same general line of strike, and making for Dover and the North Downs.

(11) We have seen that the range of the coal-measures has been pushed farther and farther to the west by experimental borings, until they have been proved to exist underneath Calais. The opposite shores of the Straits of Dover, therefore, presented the best locality for a trial still further to the west. In choosing a site, the Channel Tunnel works, close to Shakespeare Cliff, Dover, appeared to me to present great advantages, which I embodied in a report to Sir Edward W. Watkin, in 1886. The site is within view of Calais, and not more than 6 miles to the south of a spot where about 4 cwt. of bituminous material was found embedded in the chalk in making a tunnel, which, according to Godwin-Austen, had been probably derived from the coal-measures below.

Prestwich also had pointed out, in 1873, in dealing with the question of a tunnel between England and France, that the older rocks were within such easy reach at Dover, that they could be utilized for the making of a submarine tunnel. Sir Edward Watkin acted with his usual energy, and the work was begun in 1886, and has been carried on down to the present time, under my advice, and at the expense of the Channel Tunnel Company. The boring operations have been under the direction of Mr. F. Brady, the Chief Engineer of the South-Eastern Railway, to whose ability we owe the completion of the work to its present point, under circumstances of great difficulty. A

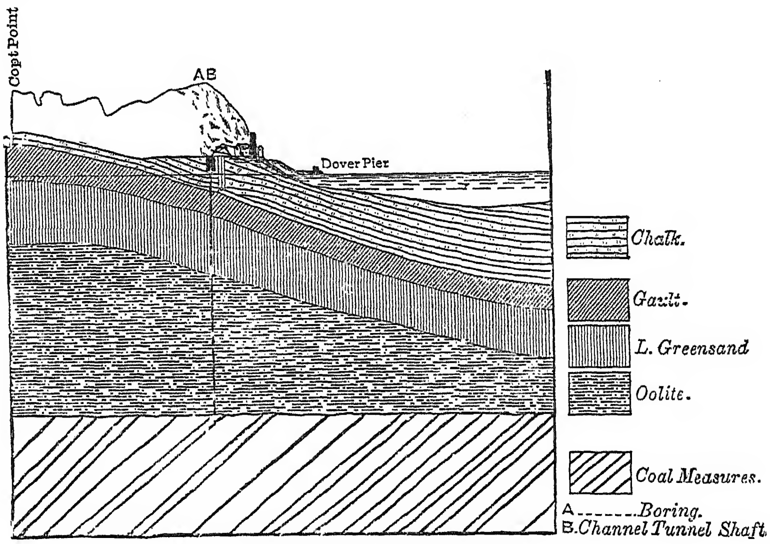


Fig. 1.—Boring at Shakespeare Cliff.

shaft has been sunk (A, Fig. 1) on the west side of the Shakespeare Cliff, close to the shaft of the Channel Tunnel (B) to a depth of 44 feet, and from this a bore-hole has been made to a depth of 1180 feet.

Section at Shakespeare Cliff, Dover.

	Feet.
Lower grey chalk, and chalk marl	
Glauconite marl	
Gault	500
Neocomian	
Portlandian	
Kimmeridgian	
Corallian	660
Oxfordian...	
Callovian	
Bathonian	
Coal-measures, sandstones, and shales and clays, with one seam of coal	70

The coal-measures were struck at a depth of 1204 feet from the surface, or 1160 feet from the top of the bore-hole, and a seam of good blazing coal was met with 20 feet lower.

(12) This discovery proves up to the hilt the truth of Godwin-Austen's views as to the range of the coal-measures along the line of the North Downs, and as to the thinning off of the Oolitic and Wealden strata against the buried ridge. The former are less than one-third of their thickness at Netherfield, and the latter are wholly unrepresented. It establishes the existence of a coal-field in South-Eastern England, at a depth well within the limits of working at a profit. The principal coal-pits in this country are worked at depths ranging from over 1000 to 2800 feet, and one at Charleroi, in Belgium, is worked to a depth of 3412 feet.

The Dover coal-field probably forms part of the same narrow trough as the Calais measures, prolonged westward under the Channel further to the south than Godwin-Austen drew it in 1858. Whether it is a trough similar to that which extends through Northern France for more than 100 miles from east to west, as Godwin-Austen has drawn it in the diagram on the wall, reaching as far to the west as Reading, or whether it is a small, faulted, insignificant fragment of a field, such as that of Marquise and Hardinghen, remains to be proved. It is,

[1] This fact is doubted by Gosselet. I am, however, informed by Prestwich that both he and Elie de Beaumont identified them as coal-measures at the time, and I see no reason for doubting the accuracy of those two eminent observers. The cores were, unfortunately, lost in the first Paris Exhibition.

however, one of a chain of coal-fields which will, in my opinion, ultimately be proved to extend under the newer rocks between Dover and Somerset, along the line of the North Downs, in long narrow east and west troughs. It is probably a continuation beneath the Straits of Dover of the coal-measures struck at Calais (see Fig. 2).

The further question as to the value of these fields may be answered by the amount of coal in the fields which are now being worked in Westphalia, Belgium, France, and Somersetshire. The Westphalian coal-field contains 294 feet of workable coal, distributed in 117 seams; that of Mons, 250 feet, in 110 seams; and that of Somerset, 98 feet, in 55 seams. The North French coal-field in 1887 yielded 7,119,633 tons, and gave employment at the pits to 29,000 men, and is rapidly increasing its output.

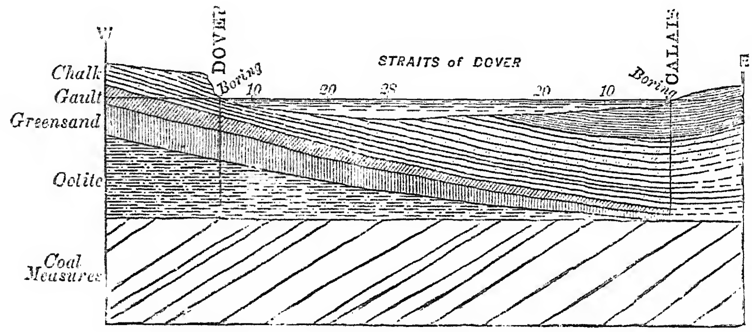


FIG. 2.—Probable Range of Coal-measures between Dover and Calais.

It may be inferred that the buried coal-fields which await the explorer in the North Downs are in all probability not inferior to these. Godwin-Austen, in his memorable paper before the Geological Society, in 1855, said that if one of these buried fields were once struck in South-Eastern England, their exploration would be an easy matter. It has been struck at Dover, and the necessary base is laid down for further discoveries, which in all probability will restore to South-Eastern England the manufactures which have long since fled away to the coal districts of the west and north, and which will put off by many years the evil day when the energy stored up in the shape of coal in these islands shall have been spent.

RECENT ADDITIONS TO THE LITERATURE OF INSULAR FLORAS.

THE LACCADIVES.

THESE small islands, fourteen in number, are situated between 10° and 14° N. lat., and at 120 to 180 miles from the Malabar coast of India. They are of coral formation, almost without exception portions of atoll rings, and nowhere elevated more than twenty feet above the sea, so that storm-waves sometimes sweep completely over them. In 1847 such a wave destroyed 1000 of the small population, and there have been equally disastrous cyclones in much more recent times. Indeed, according to Hunter's Imperial Gazetteer of India, the islands, which have an area of two to three square miles, are nowhere more than ten or fifteen feet above the level of the sea. In 1871 the population was estimated at about 13,500, and the almost sole cultivation is the coco-nut palm. It is supposed that the abundance of this palm may have attracted the first settlers, but as that event occurred more than 350 years ago—how much more it is impossible to say—this point must remain uncertain. The total annual value of the exports, consisting almost entirely of the products of the coco-nut palm, is said to be about £17,000. From the physical character of the group, it was not expected that the flora contained any endemic element, but until quite recently there was no published account of the vegetation, beyond broad generalizations. Dr. D. Prain, Curator of the Calcutta Herbarium, has supplied the want in the "Memoirs by Medical Officers of the Army of India," Part V., where he gives an enumeration and analysis of all the plants hitherto known by him to have been collected in the islands, and he has since communicated to the writer a list of some twenty additional species. Briefly, the vegetation consists, apart from cultivation, of very widely dispersed plants—whose wide area is due to ocean currents, birds, or winds—plus a number of weeds of tropical cultivation. Dr. Prain has not visited any of the islands himself, and collectors have not concerned themselves with the question of colonization of plants from drift-seeds or from seeds conveyed to the islands by carpophagous birds; hence his deductions are mainly based on probabilities, which he discusses in considerable detail, followed by a table giving the full distribution of all the plants then known to him from the islands. These number eighty, including seventeen purely cultivated plants. It is interesting to know what is cultivated, of course; but it is undesirable to encumber the distributional tables with plants of this category. Dr. Prain estimates that the presence of eleven species is certainly due to the sea, seventeen probably so, and twenty-two possibly so; whilst birds are regarded as the agents in two, three, and five instances respectively. The two ferns collected in the island of Anderut are set down with certainty to the wind, and two or three other plants probably to the same agency. The rarity of ferns seems to be accounted for, in part at least, by the extreme flatness of the islands rather than by unfavourable conditions, for Dr. Treub found eleven species of ferns on the elevated part of Krakataõ only three years after the great eruption, which absolutely destroyed all the vegetation previously existing, and covered the island with a volcanic deposit of intense heat from one to sixty yards in thickness.

One common tree in the vegetation of many islands of the Indian Ocean we miss in Dr. Prain's list, and that is *Cordia subcordata*, the iron-wood of the Keeling Islands.

THE KURILES.

Mr. Kingo Miyabe, lately appointed Professor of Botany at the Agricultural College, Sapporo, Japan, and formerly a student at Harvard, U.S., and for a short time in this country, is the author of a "Flora of the Kurile Islands," which is published in the Memoirs of the Boston [U.S.] Society of Natural History, vol. iv., No. 7.

This is perhaps the most finished piece of systematic and geographical botany yet published in English by a Japanese botanist, and it will give the author a reputation for completeness and conciseness that might be envied by many western botanists. The enumeration is based partly on personal observation and partly on scattered records and herbarium specimens, for which full references are given; and all authorities are cited, so that the sources of information are not uncertain, as is too often the case.

The Kurile Islands form a chain nearly 800 miles long, extending from the southern point of Kamtschatka to Yezo; and, by treaty with Russia in 1875, they are now all under Japanese rule. The principal islands are about twenty-four in number, but they are only partly inhabited, on account of their barrenness and lack of good water. The whole chain is described as of volcanic origin, and fifty-two cones have been observed, seventeen of which were active. The coasts generally are precipitous and unapproachable, and the few bays and coves they possess are insufficiently sheltered to be safe for ships in bad weather. Indeed, some of the islands can only be visited in the perfectly calm weather of the summer-time. In consequence of the sea-currents from the north, the climate is very cold for the latitude (about 43° to 51°), and dense fogs prevail during easterly or southerly winds. There is, however, a marked difference in the climate of two or three of the southern islands, which come under the influence of a warm current running to the north-east. North of Etorofu the islands are locked in ice from November till April or May, and the mountains are snow-capped throughout the summer; hence it is not surprising to learn that the vegetation is of a sub-arctic character.

Mr. Miyabe's enumeration comprises 299 species of flowering plants, and 18 vascular cryptogams; but it is not supposed that these numbers exhaust the flora. These 317 species belong to 187 genera and 53 natural orders, and 21 of the latter are represented by a single genus, and 9 by a single species each. The natural orders comparatively rich in genera are: Compositæ, 15; Rosaceæ and Liliaceæ, 12; Gramineæ, 11; Ranunculaceæ and Ericaceæ, 8; Cruciferæ and Umbelliferæ, 7; and most of these orders are the richest in species, though the Caryophyllaceæ and Scrophulariaceæ come in before the Cruciferæ and Umbelliferæ. The Compositæ number 30 species; the Rosaceæ, 23; the Gramineæ, 17; and the Ericaceæ, 16. It is noteworthy that in this small flora, or, rather, portion of a flora, the Compositæ form a relatively high percentage, as they do in the Arctic flora and in the various regions of Central and Eastern Asia, from the Caspian to Japan, whose floras have been analyzed by Maximowicz. So far as at present known, the Kurile flora contains no endemic element, unless we except two imperfectly-known plants, which, however, as Mr. Miyabe observes, are much more likely to be forms of more widely-spread species. North of the islands mentioned as under the influence of a warm sea-current, the flora is largely composed of species having a wider range, many of them all round the northern hemisphere, and species having a more or less wide area in North-East Asia. The facts that upwards of 25 per cent. of the species are British, and that 84 per cent. of the genera are spread over Europe, Northern Asia, and North America, will assist us in forming an idea of the general composition of the vegetation. Only three of the genera are restricted to the mountains of tropical Asia and North-Eastern Asia—namely, *Skimmia*, *Crawfurdia*, and *Acanthopanax*. Mr. Miyabe finds that 26 per cent. of the species are American-Asiatic; and 10 per cent. of these reach Eastern North America. Only six genera occur which do not reach Japan—namely, *Parrya*, *Tetrapoma*, *Claytonia*, *Lupinus*, *Armeria*, and *Dodecatheon*.

The existence in the southern islands, Kunashiri and Etorofu, of such plants as the following is strong evidence of a warmer climate: *Dianthus superbus*, *Hypericum erectum*, *Skimmia japonica*, *Ilex crenata*, *Rhus Toxicodendron*, *Hydrangea scandens*, *Aralia racemosa*, *Acanthopanax ricinifolia*, *Crawfurdia japonica*, and *Bambusa Kurilensis*. The bamboo is said to grow so thick and so tall in the neighbourhood of Shana, in Etorofu, as to form almost impassable thickets.

Mr. Miyabe concludes his discussion of the flora of the Kuriles in the following words:—"From these observations I agree with Prof. Milne in the opinion that, at the time of the last great southerly migration of the rich polar flora, Japan received her portion mostly through the island of Saghalin, and but little, if any, through the then uncompleted chain of the Kurile Islands."

THE BAHAMAS.

A provisional list of the plants of this chain of islands, by John Gardiner and L. J. K. Brace, edited by Prof. C. Dolley, appears in the Proceedings of the Academy of Natural Sciences, Philadelphia, 1889, pp. 131–426.

This is not intended as a critical review, and perhaps an avowed provisional list is, in a sense, exempt from such an ordeal; yet it seems no more than right to call attention to the extraordinary notes and remarks under some of the species, genera, and orders, so that the writer who is responsible for them may have the opportunity of claiming all the credit due to him. Taking the first of the dicotyledons, *Clematis Vitalba*, it is said to be "indigenous and nearly cosmopolitan"; and *Delphinium* sp. is recorded as "indigenous from old world," whatever that may mean. The Bixineæ, "as a whole, have fully bitter and astringent properties, and some of the members are poisonous." This is indefinite, but the Compositæ are described as "plants mostly possessing a bitter principle which renders them tonic"; and *Eupatorium* (a genus of about 500 species) "is extensively used as a remedy for malaria." A more definite statement, "grasses are valuable as food for cattle and men," is true, although the instances on record of men having eaten grass itself are exceedingly rare. Some of the remarks on the distribution of the plants enumerated, and really restricted to the West Indian region, or the West Indian and Mexican regions, are equally incomprehensible. Thus *Alvaradoa amorphoides*, a shrub inhabiting the Bahamas, Cuba, and Mexico, including the interior province of Chihuahua in the north, is said to be found on "all tropical coasts." The work abounds in indefinite, and often unintelligible, remarks on the medicinal properties of plants. Under *Clethra tinifolia* we find the note: "This plant does not appear to be of use for anything. The order [Ericaceæ] has astringent properties. Its leaves and flowers are used as a diaphoretic; they are saponaceous and detergent."

The list itself is largely compiled from Grisebach's "Flora of the British West Indies," and from names communicated from Kew to Mr. Brace, based on specimens supplied by him from time to time; and is so far approximately correct. On the other hand, some of the additional names are strangely inaccurate and far-fetched Thus, *Sinapis Brassicata*, Linn., a Chinese plant, now believed to be the same as *S. juncea*, is put down as mustard, and as native of the West Indies. It is true that Grisebach uses this name in his "Flora of the British West Indian Islands," therefore it is, to that extent, excusable. That an "M.D." and a Professor of Biology should be so careless of his reputation as to publish such undigested matter is inconceivable. Apart from its faults, the list is imperfect so far as our present knowledge goes, and it may be better to await an emended edition before attempting to give any particulars of the flora here.

FERNANDO NORONHA.

Darwin landed on this island on the outward voyage of the *Beagle*, and collected a few plants, and Moseley succeeded in obtaining specimens of a few plants from the main island and the islet of St. Michael's Mount, but was prevented from making a complete collection in consequence of the *Challenger* being unprovided with the necessary authorization. These plants were described by the writer, and some of them figured in the "Botany of the *Challenger* Expedition." Provided with funds by the Royal Society, Mr. H. N. Ridley, formerly of the British Museum, and now Government Botanist for the Straits Settlements, visited the island in the summer of 1887, accompanied by Mr. G. A. Ramage and the Rev. T. S. Lea. The party remained on the island, or rather group, for there are several islets besides the main island, forming a chain, which may have formerly been continuous. Thus they had time to explore thoroughly the natural history; and an account of the botany, by Ridley, has just appeared in the current volume of the Journal of the Linnean Society. The singularly unconnected form of the introductory matter is doubtless due to the hurried manner in which it had to be completed before the author's departure for Singapore.

Fernando Noronha is in about 3° 50′ S. lat., and nearly 200 miles from the nearest point of the Brazilian coast. The whole chain is about eight miles in length, and the main island five miles long and nearly two miles across in one part, though very much narrower generally.

The fragment of the flora published in the "Botany of the *Challenger*" was considered sufficient to enable us to form an opinion of its general character, and state that there was no peculiarly insular element in the vegetation. This is fully borne out by the subsequent discoveries.

Mr. Ridley gives no analysis of the composition of the flora beyond classifying the plants as weeds, such plants as might be introduced by sea-currents, and such as have berries and eatable seeds, with examples; but he does not tabulate the whole. His very brief "summary" follows:—

"The whole group of islands possesses certain characteristics common to all truly oceanic islands and some of those which are merely the relics of vanished continents. In the first place there is the absence of indigenous mammals, and more noticeably of bats, of fresh-water fish, and amphibians. Again, the number of indigenous species, both of plants and animals, is very small, while the number of individuals is very large. The insects are small and dull in colour, and but few of the plants have showy flowers, white and yellow being prevailing colours. A considerable proportion of the indigenous plants are shrubby or arboreous, as in many other oceanic islands; but arboreous or even shrubby Compositæ do not exist, indigenous species of the order being rare in the group."

There will be differences of opinion, of course, as to the teachings of the data collected by Mr. Ridley and his companions, especially as to whether the present vegetation be a remnant of a former continental flora or a purely derived insular flora of comparatively recent origin.

Mr. Ridley himself states "that there is no evidence whatever to show a former connection with the mainland of Brazil at any time, in spite of what has been asserted by Dr. Rattray to the contrary." On the other hand, in a sketch of the geology of the island, based on petrological notes by Thomas Davies, which follows the enumeration, it is merely doubted "that the evidence is sufficient to prove a connection."

It appears, too, that "some American petrologists, who have found similar rocks to those of Fernando Noronha in the neighbourhood of Cape San Roque, seem to consider that the group may have been connected at one time with the mainland at this point."

Roughly counting the plants in the enumeration, we find there are nearly two hundred species of phanerogams, including weeds and a few others undoubtedly introduced, intentionally or unintentionally, by man. Out of this total, about thirty-two are described as new, or, in about half-a-dozen instances, more fully described than was possible from the imperfect material previously known. So far as present evidence goes, these are all endemic in Fernando Noronha; but while so much remains to be done in the investigation of the Brazilian flora, it should not be assumed that they are really so. Some of them, indeed, are admittedly very closely allied to previously-described species, and botanists might differ as to the propriety or expediency of treating the majority of them as independent species. And as to the whole, they present no peculiar characteristic suggesting the improbability of their occurring on the mainland.

The poverty of the flora in species may be largely due to climatal and other conditions. The climate is so dry generally, or the periods of drought are so protracted, that marsh plants, epiphytes, and ferns are almost wholly wanting. Mr. Ridley discovered one fern, *Pellæa geraniæfolia*, but it was rare and local, and this very widely-spread fern will grow in comparatively dry situations.

A large number of the plants, including several of the supposed endemic species, bear edible fruits; yet "there is only one fruit-eating bird on the island, and that is the endemic dove, *Zenaida noronhæ*." This fact tempts Mr. Ridley "to wonder whether the number of endemic species with edible fruit could possibly have all been introduced by this single species of dove, or whether other frugivorous birds may not at times have wandered to the shores." This sentence can hardly convey what Mr. Ridley had in his mind when he wrote; and being so distant from home he probably had no opportunity of revising it in print. Moreover, it is hardly correct to designate this group of islands as "oceanic."

Prominent in the vegetation among the assumed new or endemic plants are: *Erythrina aurantiaca*, *Cereus insularis*, *Bignonia roseo-alba*, *Pisonia Darwinii*, *Sapium sceleratum*, and *Ficus noronhæ*. There are also described two species of *Oxalis*, three of *Ceratosanthes*, a genus of Cucurbitaceæ, a *Sesuvium*, a *Cuscuta*, a *Physalis*, a *Solanum*, a *Lantana*, and three of *Cyperus*, besides a few others of less familiar genera. Of greater botanical interest is an apparently diœcious Combretacea, provisionally placed in *Combretum* as the type of a new section, *Terminaliopsis*. Taken as a whole, the vegetation is quite that of the mainland deprived of the moisture-loving element.

In conclusion, it may be stated that the woods mentioned by the earlier writers have almost disappeared since the main island has been made a convict settlement.

W. BOTTING HEMSLEY.

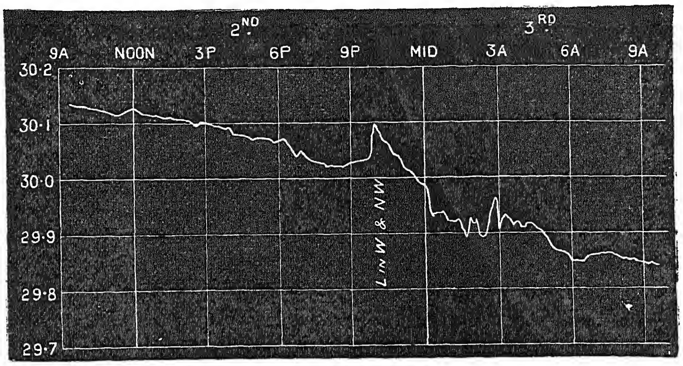
THE BRONTOMETER.

FOR more than a century meteorologists have been puzzled by the exceptional action of the barometer during some (not all) thunderstorms, and during some (but not all) heavy rains. As a general rule, one expects the barometer to fall for rain and bad weather, but in 1784 Rosenthal pointed out that "when a thunderstorm approaches the place where a barometer is situated, the mercury in the tube begins to rise; the nearer the thundercloud comes to the zenith of the observer, the higher does the mercury rise, and reaches its highest point when the storm is at the least distance from the observer. As soon, however, as the cloud has passed the zenith, or has become more distant from the observer, the weight of the atmosphere begins to decrease and the mercury to fall."

The recent rapid increase in the number of self-recording barometers in use has led to much interest being taken in these fluctuations, which are sometimes very

numerous, and far from the simple rise and fall noted by Rosenthal, Planer, Toaldo, and other early writers. A very good illustration was afforded by my Redier barograph on August 2-3, 1879, of which the curve is reproduced below.

It might for a moment be supposed that the zig-zag about 2 a.m. was due to what is known as "pumping" in the barometer, but that is not the case. "Pumping" rarely takes a minute from its lowest to its highest point, *i.e.* two minutes from one maximum to another,[1]



Camden Square, London, August 1879.

whereas these maxima are at intervals of about half an hour.

Several explanations of these oscillations have been offered, and in my paper in the Proc. Roy. Soc. I have endeavoured to give a summary of them, but I have long felt that as a preliminary to a theory we ought to be sure of the facts.

Years ago, when Sir Francis Ronalds, F.R.S., had his collection of electrometers in the dome of Kew Observatory, he devised what he called a storm clock, which was really a paper going at a regular rate, so that the observer needed only to record the phenomena, and the position of his writing showed the time.

In 1890 we ought to do better than half a century ago, and, thanks to the great skill of MM. Richard Frères, of Paris, the new machine, if it does not absolutely justify its name "brontometer" (Βροντή μέτρον, thunderstorm measurer), is a very near approach to it.

And first as to the object, we want to find out (1) the nature of the oscillations already mentioned; and (2) to what they are due.

The only way to do this is to get them on so large a scale that they can be critically examined, and to find out with what phenomena they are synchronous, or in definite relation as to sequence and time. Irrespective altogether of these barometric oscillations there are several features in thunderstorms not at all understood, such, for instance, as whether the rush of rain which sometimes accompanies an exceptionally fine flash of lightning is the cause or the result of that flash. For this and other points absolutely accurate time is of the highest importance, and evidently all phenomena must be recorded on one sheet of paper.

A method adopted for some of his instruments by Mr. H. C. Russell, F.R.S., of Sydney, might with advantage be copied in some of the European Observatories. As a general rule, and for ordinary phenomena, half an inch of paper for an hour of time, *i.e.* 12 inches per diem, gives a sufficiently open scale; but when special phenomena occur it is very handy to be able to accelerate the speed five or ten times, and this Mr. Russell does with ease. But even ten times times the ordinary speed, or 5 inches an hour, would not enable one to read closer than to quarter minutes, which would be useless for ascertaining the details of a thunderstorm and the correlation of the various phenomena with the peculiar oscillations already mentioned.

These, then, are some of the reasons which led me, more than three years since, to consult MM. Richard as to the construction of the brontometer, now at length completed.

It is provided with endless paper, 12 inches wide, travelling under the various recording pens at the rate of 1·2 inch per minute, or 6 feet per hour. This is about 150 times faster than is usual in meteorological instruments, and enables the time of any phenomenon to be read off with certainty to a single second of time.

The traces are made in aniline ink by a series of seven Richard pens.

The first pen is driven by the clock which feeds the paper, so that the time scale and the paper must go together. The pen usually produces a straight line, which serves as the base line for all measurements, but at 55 seconds after each minute the pen begins to go, at an angle of about 45°, one-tenth of an inch to the left, and at the sixtieth second it flies back to its original position.

The second pen is driven by one of Richard's anemo-cinemographs—a name which they have given to a pattern of anemometer not yet known in England. The external portion has some resemblance to the ordinary windmill governor, but it differs from it in that the plates are curved, not flat; they are made of aluminium, and are so light that they have little momentum, and have thus a great advantage over cups, which run on for many seconds after the wind-force has decreased or ceased. The fans make one revolution for each metre of wind that passes, and send an electric current to the brontometer, where it acts on an electro-magnet, and tends to draw this (second) pen towards the left; but a train of clock-work is constantly tending to draw the pen to the right, the joint result being that the pen continuously shows, not the total motion (as is the case with most anemometers), but the actual velocity almost second by second. It does this certainly with an error of less than five seconds, for the fans will stop dead in less than that time, and the clock-work train will bring the pen from indicating a velocity of 70 miles an hour to 20 miles an hour in three seconds, and down to a dead calm in seven seconds. The trace will thus resemble that of a pressure anemometer, but with a much more open scale than was ever before available.

The third pen is actuated by a handle, and can be set at zero or at 1, 2, 3, or 4 spaces from it. The author's

[1] I believe that half a minute would be nearer, but until th brontometer has been worked during a heavy gale no one knows.

original idea was, partly by watching a storm-rain-gauge, and partly by estimation, to decide on the intensity of the rain, and to indicate that intensity by moving the pen further and further from zero as the fall becomes heavier. Experience alone will show whether that is, or is not, superior to moving it one step for each $\frac{1}{100}$th of an inch of fallen rain, which can be done by making a Crosley rain-gauge send a current into the room where the brontometer is placed, and strike a bell there. In a heavy storm there will, however, be so much for the observer to do, that very probably count would be lost. It may, therefore, be necessary to make it act automatically.

The fourth pen is actuated somewhat like a piano. On the occurrence of a flash of lightning, the observer presses a key, the pen travels slightly to the right, and flies back to zero. Referred to the automatic time-scale, this gives, to a second, the time at which the key was depressed.

The fifth pen is similar, but, being intended to record the thunder, the observer will continue to hold down the key until the roll is inaudible. The time of the departure of this pen from zero will evidently be later than that for the lightning by the time-interval due to the distance of the flash, and possibly something may be learned from the accurate record of the duration of the thunder.

The sixth pen is similar to the third, and is intended to record the time, duration, and intensity of hail.

The seventh and last pen is devoted to an automatic record of atmospheric pressure. As the rapid motion of the paper, which is indispensable for studying the details of a thunderstorm, has enlarged the time-scale more than a hundredfold, it was imperative that the barometric scale should itself be greatly enlarged. But the range of the barometer in London is more than 2½ inches, and no enlargement less than ten times the natural (mercurial) scale would be of any use; hence a breadth of 25 inches of paper would be necessary, unless some mode of shifting the indication could be devised.

Several plans were tried, but finally a modification of Richard's statoscope has been adopted, which is so sensitive that it will indicate the opening or shutting of a door in any part of the house, gives a scale of 30 inches for each mercurial inch (*i.e.* about three times that of a glycerine barometer), and yet only requires 4 inches breadth of the brontometer paper. Without entering into all the details of construction, it is desirable to explain the general principle, and its application. As it was essential that the apparatus should record accurately to 0·001 inch of mercurial barometric pressure, it was evident that friction had to be reduced to a minimum, and considerable motive power provided. This is done by placing in the base of the brontometer a galvanized iron chamber, which contains about 3½ cubic feet of air; on the upper part are a series of elastic chambers, similar to the vacuum boxes of aneroid barometers, but much larger. When the instrument is to be put in action, these chambers are connected with the large air-chamber, and a tap is closed which shuts off communication with the external air. Any subsequent increase, or decrease, of atmospheric pressure will compress, or allow to dilate, the air in these chambers, and the motion of the elastic ones produces that of the recording pen.

Obviously, any large change in the temperature of the confined air would vitiate the readings; but (1) the instrument is not required to give absolute, but merely differential, values, and (2) the influence of the changes of temperature is greatly reduced by the chamber being surrounded with 4 inches thick of non-conducting material, besides nearly 1 inch of wood outside of it. The change of temperature in a room, and during the short time that the statoscope will be worked without resetting to zero (*i.e.* without opening the tap) has not hitherto produced any measurable effect.

G. J. Symons.

NOTES.

The Société de Physique et d'Histoire naturelle de Genève has decided to celebrate its hundredth anniversary. It was founded in 1790, having originated in informal meetings of eminent men of science who lived at that time in Geneva. On October 23 a special meeting will be held, at which papers will be read relating to the history of the Society and to the labours of its members. It is hoped also that some honorary members may be disposed to contribute to the success of the meeting by sending scientific communications. In the evening there will be a banquet. Members who intend to be present are asked to write to M. de la Rive, the President, some days before the celebration.

The International Exhibition of Mining and Metallurgy, at the Crystal Palace, was officially opened on Monday by Lord Thurlow, F.R.S., one of the honorary vice-presidents of the Exhibition. We shall give some account of the Exhibition when the arrangement of the specimens is in a more forward state.

The British Medical Association is holding its fifty-eighth annual meeting at Birmingham. The President, Dr. Willoughby F. Wade, delivered the opening address on Tuesday. On Wednesday Sir Walter Foster delivered an address in medicine; to-day Mr. Lawson Tait delivers an address in surgery; and to-morrow, at the concluding general meeting, Dr. W. H. Broadbent will speak on therapeutics. At the meeting to-day the Association's gold medal "for distinguished service" will be presented to Surgeon Parke, of the Emin Relief Expedition.

The summer meeting of the Institution of Mechanical Engineers is being held at Sheffield. It began on Tuesday, July 29, and will not be concluded until to-morrow. The following is a list of the papers: on steel rails, considered chemically and mechanically, by Mr. Christer P. Sandberg, of London; on recent improvements in the mechanical engineering of coal-mines, by Mr. Emerson Bainbridge, of Sheffield; description of the Parkgate iron and steel works, by Mr. Charles J. Stoddart, managing director; description of the Sheffield water works, by Mr. Edward M. Eaton, engineer; description of the Loomis process of making gas for fuel, by Mr. R. N. Oakman, Jun., of London; on milling cutters, by Mr. George Addy, of Sheffield; on some different forms of gas furnaces, by Mr. Bernard Dawson, of Malvern; on the Elihu Thomson electric welding process, by Mr. W. C. Fish, of London (communicated through Prof. Alexander B. W. Kennedy, F.R.S., Vice-President).

The Leeds Executive Committee, appointed for the purpose of making arrangements for the visit of the British Association, met on Monday. The Mayor, Alderman Elmsley, in opening the proceedings, said that some of the most eminent men of science in Europe and America had announced their intention of being present. Many of the principal manufacturers of Leeds had most generously consented to open their works for inspection by members of the Association. Arrangements had also been made for excursions to places of interest, historical or otherwise. He had no doubt that the inhabitants of Leeds would show all the hospitality and enthusiasm that was required. In the course of the proceedings it was stated that it was the original intention to have a guarantee fund of not less than £500, but that fund now amounted to not less than £6540. A report of the Executive Committee recommended that a call of 50 per cent. should be made on the guarantors, but Mr. Benson Jowitt, in moving the adoption of the report, expressed his belief that eventually it would turn out that the call had been more than sufficient to meet the demands which had been made upon it. The Vicar of Leeds, Dr. Talbot, having seconded the adoption of the report, it was carried, and the proceedings terminated.

THE National Association for the Promotion of Technical and Secondary Education has issued its third annual report. It speaks of the past year as the most eventful one of its existence, so far as the actual realization of the objects of the Association are concerned. The report will be of great interest to anyone who may wish to obtain a general view of the progress which is being made towards the establishment of a proper national system of technical and secondary education.

ATTEMPTS have been made in Parliament this week to secure that the money to be raised from the new tax on spirits shall be applied in Scotland to the establishment of a perfectly free system of elementary education. The Government declines to accept the proposal, which has, therefore, for the present been rejected. On Tuesday evening Mr. Goschen said the matter had been spoken of as a small one, but he thought the decision whether or not the standards above the compulsory standards as well as the compulsory standards themselves should be freed was by no means a very small question. The argument had been put forward that the Government would be justified in freeing parents from the duty (which hon. members now entirely discarded) of educating or contributing to the education of their children. They had relieved parents from that duty where the State had enacted compulsion, but the Government were not prepared to sanction the principle that beyond the compulsory standards education must necessarily be free. Mr. Mundella strongly supported the scheme. He pointed out that the compulsory standard varied in different districts from the third to the sixth. The compulsory standard had been fixed as a minimum, but the Chancellor of the Exchequer would tend to stereotype it, and in many places make it the maximum. Children were passing out of school at an earlier age year by year. In other countries the standard was not one of class, but of age. In Scotland children were passing the fifth standard as early as 10 or 11 years old. The payment of fees had been a great hindrance to the attendance of children at schools. That was why they were dealing with the question now. No doubt the child's wages were a great temptation, but the fee might just make the difference to a poor parent in deciding whether to keep his child at school or not. The present system was a great hardship on precocious children who passed the fifth standard at an early age.

THE Board of Agriculture announce that they have received information reporting the presence of the Hessian fly in the counties of Lincoln, Suffolk, and Herts slightly, and badly near Errol, in Perthshire. Owing to the twisted condition of much of the corn, it is more than usually difficult to detect the presence of the insect. Information is being prepared, and will at once be circulated by the Board.

THE returns presented to the Middlesex County Council by the various inspectors under the muzzling order show that during the quarter ending June last five dogs were seized with rabies in the county, as against seven in the previous quarter. But for the number of cases of rabies the Board of Agriculture would have been asked to withdraw the order. During the same period 526 dogs were seized, 87 of which were claimed and the remainder slaughtered. These figures compare with 1039, 108, and 946 respectively for the March quarter. The total number of dogs seized in the year was 3250, of which 488 were claimed and 2634 slaughtered. In the same period there were 49 cases of rabies, as against 22 in the previous year.

THE trustees of the South African Museum, in their Report for the year ended December 31, 1889, say that in the course of this period valuable assistance was rendered in the palæontological section by Prof. H. G. Seeley, F.R.S., who, during his brief visit to the colony, examined the South African fossils in the Museum, and determined and labelled a considerable number of them. The trustees were glad to learn that Prof. Seeley discovered in the Museum series an apparently new genus, and they had much pleasure in intrusting to his care some of the most interesting specimens for further investigation in England.

WE learn from the *Bulletin* of the Torrey Botanical Club that, through the cordial co-operation of the officers of the New York State Fish Commission, and the great personal interest of its President, Mr. Eugene Blackford, the Brooklyn Institute has been enabled to open a sea-side laboratory for teaching and research in zoology and botany, under the direction of Dr. Bashford Dean. The laboratory is located at Cold Spring Harbour, Long Island, 32 miles from New York, reached by the Long Island Railway. The session opened on July 7, and was to extend over eight weeks; the fee is 24 dollars. The location is described as a capital one, and an extensive corps of lecturers on special subjects has been secured, those on the botanical side being Dr. W. G. Farlow, Dr. N. L. Britton, and Prof. Byron D. Halsted.

THE death is announced of Dr. Alexander von Bunge, the veteran Professor of Botany at the University of Dorpat, at the age of 87. Dr. von Bunge was engaged, in the year 1830, in a scientific expedition in China, and subsequently in Khorassan and Afghanistan. His speciality of recent years was the flora of Russia and of Northern Asia. He was a foreign member of the Linnean Society of London.

SHOCKS of earthquake have lately been felt in different parts of Austria-Hungary. On July 23 two violent though short shocks took place in the Muehl district, in Upper Austria, and on July 25 a violent shock occurred in the valley of the Tscherna, in Moravia. A telegram received at Budapest from Mehadja on July 25 announced that two violent shocks of earthquake had been felt at the Hercules Baths, near that place, at half-past 11 on the previous night. The direction of the disturbance was from east to west.

THE Paris Museum of Natural History received recently from M. J. Bretonnière an interesting sample of limestone (from the suburbs of the town of Constantine in Algeria) in which there are a number of excavations, due apparently to *Helicidæ*. M. Stanislas Meunier thinks that land-snails are enabled to penetrate the rock through the agency of the siliceous particles which were shown by Hancock in 1848 to be the instruments used for similar work by some marine mollusks.

IN his recent thesis on the influence of the sea-shore on leaves M. Pierre Lesage shows by conclusive evidence that a marine, habitat leads to a thickening of the leaves. The palissade-cells are more numerous and larger than in the leaves of the same plants grown inland. Apparently the sea-salt is the cause of this alteration, as plants cultivated in artificially salted soil yield thicker leaves. The observations of M. Lesage bear on some ninety species of plants which are in their natural state found near the sea (in Brittany) as well as inland.

AN excellent paper on the Peabody Museum of American Archæology and Ethnology in Cambridge, U.S., by Frederick W. Putnam, has been reprinted from the Proceedings of the American Antiquarian Society, October 23, 1889. Mr. Putnam, dealing with the problems suggested by the collections of the Museum, thinks that the following are the elements to be taken into consideration in any endeavour to trace the present North American tribes and nations back to their origin. First, small oval-headed Palæolithic man. Second, the long-headed Eskimo. Third, the long-headed people south of the Eskimo. Fourth, the short-headed race of the south-west. Fifth, the Carib element of the south-east. All these elements, Mr. Putnam

holds, must be studied with their differences in physical characteristics, in arts, and in languages. "From a commingling of all," he says, "with greater or less predominance of one over the other, uniting here and subdividing there, through many thousand years, there has finally resulted an American people having many characteristics in common, notwithstanding their great diversity in physical characteristics, in arts, in customs, and in languages. To this heterogeneous people the name Indian was given, in misconception, nearly four hundred years ago, and now stands as a stumbling-block in the way of anthropological research; for under the name resemblances are looked for and found, while differences of as great importance in the investigation are counted as mere variations from the type."

The Royal Society of Victoria has issued the second part of the first volume of its Transactions. Baron von Mueller begins this collection of papers with important "records of observations on Sir William Macgregor's highland plants from New Guinea." Mr. Arthur Dendy writes on the anatomy of an Australian land planarian; Prof. W. Baldwin Spencer on the anatomy of *Amphiptyches urna* (Grube and Wagner). A paper on the preparation of alkyl-sulphine, selenine, and phosphonium salts is by Prof. Orme Masson. Mr. A. W. Howitt, in a well-arranged and instructive paper, deals with the organization of Australian tribes. The following are among Mr. Howitt's conclusions:—(1) The group is the sole unit. The individual is subordinate in the more primitive form of society, but becomes more and more predominant in the advancing social stages. Thus group marriage becomes at length completely subordinate to individual marriage, or even practically extinct and forgotten where descent has been changed from the female to the male line. (2) An Australian tribe is not a number of individuals associated together by reason of relationship and propinquity merely. It is an organized society governed by strict customary laws, which are administered by the elder men, who in very many, if not in all, tribes exercise their inherited authority after secret consultation. (3) There are probably in all tribes men who are recognized as the headmen of class divisions, totems, or of local divisions, and to whom more or less of obedience is freely given. There are more than traces of the inheritance by sons (own or tribal) of the authority of these headmen, and there is thus more than a mere foreshadowing of a chieftainship of the tribe in a hereditary form. (4) Relationship is of group to group, and the individual takes the relationship of his group, and shares with it the collective and individual rights and liabilities. The general result arrived at is that the Australian savages have a social organization which has been developed from a state when two groups of people were living together with almost all things in common, and when within the group there was a regulated sexual promiscuity. The existence of two exogamous intermarrying groups seems to Mr. Howitt to almost require the previous existence of an undivided commune from the segmentation of which they arose.

At the meeting of the Royal Society of Tasmania on May 20, Mr. Morton drew attention to a recent dredging trip in the harbour. The result of the dredging trip was of important interest, as the forms obtained resembled the marine fauna of Port Jackson. Among the specimens dredged were a large number of mussels, and each contained a small crab, which on examination appeared to belong to the genus Fabia. It was rather curious to learn from some of the old residents that many years back, when mussels were numerous as at present, in the majority of cases every mussel contained a crab similar to those exhibited, and that the oysters, while mussels were in large quantities, were few. Some time afterwards the mussel became nearly extinct, while the oyster multiplied. Whether that was due to this parasitical crab or not he was unable to say, but the fact was singular that while the crab was now noticeable in the mussel the oyster was increasing in numbers. Whether history would repeat itself it would be difficult to say, but it would be interesting to observe the result.

The trustees of the State Museum of Natural History, New York, have issued their forty-first Annual Report. It is accompanied by the reports of the director, the State botanist, the State entomologist, and the State geologist. The directors call attention especially to the important and beautiful collection of minerals and gems bought for the Museum from Mr. George F. Kunz. They describe this collection as "one of the most perfect to be found in any American museum."

In the seventh volume of the "Bulletin of the U.S. Fish Commission," lately issued, Dr. W. R. Hamilton has an interesting note on the croaking or grunting noise made by the "Perch" (*Haploidonotus grunniens*). This fish is furnished with a masticatory apparatus in the gullet, and the lower division of this has its upper surface flat and triangular in outline, and studded all over with spheroidal "teeth," if they may be called genuine teeth. The upper division is composed of two parts united by a ligament; their lower surfaces are also supplied with similar teeth. The divisions of this apparatus have powerful muscles attached to them by which they can be pressed together and moved laterally on each other. By this process the fish masticates the crustaceans on which it feeds. When this action takes place, the croaking is produced by the teeth coming in contact and gliding over each other. About twenty years ago, being interested in this subject, Dr. Hamilton procured from an Ohio River fisherman a perch weighing 18½ pounds, which he declared was the largest perch he had ever caught. Dr. Hamilton divided the head on one side, and thus exposed its masticatory apparatus; and while he moved its grinders as he supposed the fish had done during life when crushing a crawfish, an exact imitation of the croaking sound was produced.

The Committee of the Felsted School Natural History Society, in issuing their eighth annual report, are able to congratulate the Society on a large increase of members during the past year. The members seem to give a good deal of attention to scientific study, but the Committee "continue to lament the very serious diminution of the old collecting spirit once so rife in the school, and to hope for its return." They attribute this defect to "compulsory games."

Messrs. Dean and Son announce for publication "Berge's Complete Natural History of the Animal, Vegetable, and Mineral Kingdoms." It will be edited by R. F. Crawfurd, and illustrated with about 400 coloured plates and woodcuts.

Part 22 of Cassell's valuable "New Popular Educator" has been issued. The number is accompanied by a map of Africa, and there are, as usual, many illustrations.

The new number of the Journal of the Royal Agricultural Society of England (third series, vol. i. part 2) begins with an article, by Mr. D. Pidgeon, on the development of agricultural machinery. This is followed by articles on the agricultural lessons of "the eighties," by Prof. Wrightson; the Report of the Royal Commission on Horse-breeding, by Lord Ribblesdale; tuberculosis in animals, and its relation to consumption in man, by Mr. W. Duguid; fifty years of hop-farming, by Mr. Charles Whitehead; the best means of increasing the home-production of beef, by Mr. G. Murray; and the herbage of pastures, by Dr. W. Fream.

The Meteorological Office of Calcutta has just issued Part II. of "Cyclone Memoirs," containing a full description of a very violent cyclonic storm which passed through Bengal from August

21 to 28, 1888, written by Mr. A. Pedler. The text is accompanied by eighteen plates, giving the general meteorological conditions, and showing the track of the storm-centre day by day. Mr. Pedler states that this storm fully bears out the condensation theory of the formation of cyclones. It was formed over an area where comparatively low pressure had for some time been persistent, and there is abundant evidence of heavy rain falling over the district in which the storm was developed, and to the south of it. Several points of interest are referred to in the discussion, viz. the irregular cyclonic circulation of light winds near the centre of the storm, with a violent circulation of the clouds above these light winds, and these conditions appeared to shift their position, like an eddy; secondly, the sudden change from light winds to winds of hurricane force, extending chiefly in the southerly direction. Also the entire absence of a calm centre, and the fact that the lowest barometric pressure was recorded from ten to fourteen hours after the storm centre (as judged by the winds) had passed. The storm was remarkable for the slight barometric depression which accompanied it, considering the excessive force of the winds.

DR. R. J. SÜRING, of the Meteorological Office, Berlin, has submitted to the Friedrich-Wilhelms Universität, on the occasion of his taking his diploma, a useful paper on "the vertical decrease of temperature with height in mountainous districts, and its dependence upon the amount of cloud." In most works upon this subject, the special effect of cloud upon temperature has been limited to very moderate heights; in this paper the author has carefully investigated the observations at mountain stations up to about 4100 feet. The results arrived at are:—(1) In the morning, when the weather is clear, there is a constant tendency to an inversion of temperature. In summer this tendency extends to some 1650 feet, and in winter considerably higher. This condition recurs in the evening in a smaller degree. (2) If the sky is overcast, neither a daily nor yearly period of the vertical gradients is strongly marked. (3) A departure from the law of direct proportional decrease of temperature with height occurs chiefly during the morning hours of clear days—the change of temperature then takes place more slowly in the lower strata of air than in the upper—and on cloudy days, during the warm season, when, in the lower strata, the vertical decrease of temperature appears to be accelerated.

THE Allahabad *Pioneer* reports the result of a recent expedition to investigate the upper course of the Irawadi, the source of which, as is well known, is one of the still unsolved problems of geography. It has long been known from native report that two rivers, the Mali Kha and the Meh Kha, the former from the north, the latter from the east, unite a little below lat. 26° to form the Irawadi. The sources of the Mali Kha are known to be in the mountains to the east of the Brahmakund, which form the south-eastern water-parting of the Lohit Brahmaputra; but the Meh Kha, which is stated to be the larger stream, and which Colonel Walker supposes to be identical with the Lu River of Tibet, has never before been seen by any European. The junction of these two rivers has now for the first time been reached by an expeditionary party ascending from Bhamo. On May 27, Captain Barwick, of the Indian marine, accompanied by Mr. Shaw, the Deputy Commissioner of Bhamo, and Major Fenton, of the Intelligence Department, left Bhamo in the *Pathfinder*, a paddle-steamer of about 35 tons, with a view to reaching the point of confluence. From Bhamo as far as Maingna the stream is well known. Above Maingna the river runs between mountains from 1200 to 2000 feet high, and a succession of rapids has to be passed through, which by dint of hard struggling and after many attempts the *Pathfinder* successfully ascended, not, however, without several hairbreadth escapes from foundering, the whirlpools simply taking charge of the vessel. After six days' steaming, the party reached the confluence of the streams, distant about 150 miles from Bhamo. Here the river was found to be 500 yards wide, one branch, the Mali Kha, trending to the north-eastward, the other, the Nmaika (Meh Kha of the map), to the eastward. Up the former the explorers proceeded some six miles, and then came upon a series of rapids. It was decided not to go further, as the small quantity of fuel remaining was reserved for steaming up the other branch. A halt of a day was made, and the position fixed in lat. 25° 56′ N., and long. 97° 38′ E. Returning to the confluence, Captain Barwick proceeded three miles up the Nmaika, when a rapid prevented further progress. The Kachins are said to have been very friendly, though they had never seen or been in communication with Europeans before.

THE recent expedition to the Bellenden Ker Range (says the *Colonies and India*) has added a long and interesting list of new specimens of Australian flora to Queensland. Since the publication of the official report the Queensland Government Botanist (Mr. F. M. Bailey) has discovered ten more new plants, making a total of forty-one species entirely new to botanical science, and the collection is not yet exhausted. There are also several specimens of mosses and lichens, which so far have remained untouched, Mr. Bailey having had no time up to the present to devote to their examination. The total number of new species will probably extend to fifty—a result far exceeding that of any previous Australian botanical expedition. In the 1889 report of the proceedings of the Linnean Society of New South Wales there is an account of four new specimens to be added to the list of those discovered by the Bellenden Ker expedition. One of these belongs to the genus *Coccincillidæ*, and has been named *Chilocorus Baileyi*, after the Queensland botanist. A large and remarkable dark blue earthworm, over seven inches long, has been named *Perichæta terræ-reginæ*—the latter a rather pedantically inflated version of *Queenslandia*. The worm was found by Mr. Meston on the top of the Herberton Range, at 2700 feet, and given to the Brisbane Museum. Two Bellenden Ker lizards of a genus new to Australian herpetology have been named by Mr. C. W. de Vis, Curator of the Queensland Museum. They belong to the order *Geckonidæ*, and are called respectively *Tropidophorus Queenslandiæ* and *Perochirus Mestoni*, the latter after the discoverer.

AN experimental study of the transpiration of plants has been recently made (we learn from *Humboldt*), by Herr Eberdt, of Marburg. The general method was to periodically weigh an air-tight vessel containing the roots of a plant (chiefly *Asclepias incarnata*, *A. Cornuti*, or *Mercurialis perennis*) in water, while the organs of transpiration projected. Absorption was also measured by means of a graduated capillary tube. We may note the following points:—The absorption and emission-values did not generally differ much. Increase of transpiration by sunlight occurred though the latter had parted with its heat-rays by passage through an alum solution; but when, after action of diffuse daylight, the dark heat-rays of sunlight (passed through a solution of iodine in carbon-sulphide) were thrown on the plants, transpiration was also increased. Direct sunlight causes more emission than absorption (shown by a relaxed appearance of the plant); and on passage into duller light, the emission falls off more quickly than the absorption, and the plant freshens. In plants with strong cuticle or few stomata, there was but little increase of transpiration from drying the air in a bell-jar over the plant by means of a dry air-current. The stomata of *Trianea bogotensis*, being watched through a microscope while light- and heat-rays were thrown on the plant from above, they were seen to open more slowly if the heat-rays were cut off; but with heat-rays alone they remained closed. If opened in light, they remained open when the heat-rays acted alone, and closed when these too

were stopped. A sooty metallic plate at 30° to 25° held 3 to 5 seconds over the leaf opened the stomata, while dark heat-rays of sunlight failed to do so. The stomata opened also on passing a stream of warm, nearly saturated, air over the leaves. A shaking of plants acts not by way of shock, but by changing the atmosphere about the leaves, and therefore like wind. Strong shaking stimulates transpiration; while weak vibration has no perceptible effect. The effect of wind was studied by directing air-currents of measured strength on the plants. The action of the weaker currents proved proportionally greater than that of stronger. The transpiration is greater if the leaves are free to be moved than if they are fixed.

The additions to the Zoological Society's Gardens during the past week include two Ravens (*Corvus corax*), British, presented by Mr. Walter Chamberlain, F.Z.S.; two Wheatears (*Saxicola œnanthe*), two Whinchats (*Pratincola rubetra*), two Great Tits (*Parus major*), British, presented by Mr. J. Young, F.Z.S.; a Cuckoo (*Cuculus canorus*), British, presented by Mr. Valentine Marks; a Black Tortoise (*Testudo carbonaria*) from Jamaica, presented by Master Morris Blake; a Dwarf Chameleon (*Chamæleon pumilis*) from South Africa, presented by Mr. H. Tholen; a Brazilian Hangnest (*Icterus jamaicai*), two Bluish Finches (*Spermophila cærulescens*), a Tropical Seed-Finch (*Oryzoborus torridus*), a Thick-billed Seed-Finch (*Oryzoborus crassirostris*) from Brazil, a Black-faced Kangaroo (*Macropus melanops* ♂) from South Australia, deposited; a Thar (*Capra jemlaica* ♀), two Mule Deer (*Cariacus macrotis* ♀ ♀), five Cuming's Octodons (*Octodon cumingi*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on July 31 = 18h. 37m. 53s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4447	—	Bluish-green.	18 49 27	+32 53
(2) D.M. + 31° 3199 ...	5	Yellowish-red.	18 7 46	+31 23
(3) 70 Ophiuchi	4	Yellow.	17 59 54	+ 2 32
(4) o Herculis	4	Bluish-white.	18 3 18	+28 45
(5) 219 Schj.	8	Red.	18 43 57	− 8 1
(6) V Coronæ	Var.	Yellowish-red.	15 45 35	+39 54

Remarks.

(1) The well-known Ring Nebula in Lyra, which has been described in great detail by various observers. The spectrum consists of bright lines, but the line near λ 500 is the only conspicuous one. When the image of the nebula is sharply focussed on the slit, the chief line is seen as two bright dots connected by a faint line, indicating that nebulous matter fills the interior of the ring. (This is also confirmed by the telescopic appearance.) The line F and the one near λ 495 are exceedingly faint, but they are undoubtedly present. In my own observations, with a 10-inch refractor, I have also glimpsed a less refrangible line, but have not been able to determine its position with any degree of accuracy. Further investigation of this line should be made with as large an aperture as possible. It is not far from *b*.

(2) D'Arrest and Dunér agree in describing the spectrum of this star as one of Group II. with well-developed bands. The bands at the red end are the strongest, indicating that the star is well advanced in condensation. The bands in the red are the last to disappear in passing to stars like α Tauri, and hence this conclusion. As in similar stars, the line absorptions at this stage afford an interesting study. We do not know yet, for instance, the stage at which the hydrogen lines first appear, although we now certainly know that they are present in α Orionis. The same also applies to D and *b*.

(3) The integrated spectra of the components of this double star present an appearance similar to that of a well-developed star of the solar type. No attempt has apparently yet been made to separate the two spectra. This should be done, if possible, and the usual more detailed observations as to whether the temperature is increasing or decreasing should be made.

(4) A star of Group IV. (Gothard).

(5) According to the observations of Dunér, this star has a fine spectrum of Group VI., the principal bands being very wide and dark. In addition to these, the secondary bands 4 and 5 (λ 589 and 576 respectively) were easily seen, and band 2 (λ 621) was also feebly visible. What is most required in this group of stars is a very detailed examination with the largest possible apertures. If such be undertaken, particular attention should be given to the presence or absence of line absorptions.

(6) This variable is chiefly of interest because its spectrum is one of Group VI. We have as yet no knowledge, as we have in the case of variables of Group II., of the variations of spectrum which accompany the increase of light at maximum. The range of variation in V Coronæ is very considerable—7·5 to 12 in a period of about 357 days; and it is not unlikely that well-marked changes may take place in the spectrum. Dunér states that the carbon band near λ 564 is weaker than that near λ 517, and that the secondary bands are not visible; but he gives no indication of the magnitude of the star at the time of his observation. Prof. Lockyer's investigations appear to indicate that the dark carbon bands should be proportionately less strongly marked at maximum than at minimum. There will be a maximum about August 5.

A. Fowler.

Distribution of the Perihelia of Comets.—In 1880, Dr. Henry Muirhead directed attention to the arrangements which the perihelia of comets exhibit in relation to the sun's line of flight, and pointed out that, taking the twenty-two comets given in the "Encyclopædia Britannica" along with thirteen others whose elements were given in Nature up to the date of his communication, and arranging them according to their heliocentric longitudes, the perihelia were seen to be crowded into the quadrants which the sun's line of flight bisects, as compared with those taking place in the quadrants flanking the said line (Proceedings of the Philosophical Society of Glasgow, vol. xiii.). By examining the succeeding volumes of Nature, Dr. Muirhead has obtained the heliocentric longitudes of the perihelia of forty-one more comets, and in a communication to the Philosophical Society of Glasgow, on February 5, 1890, he showed that they also exhibit the same tendency to cluster near heliocentric longitudes 263° and 83°—that is, the longitude of the "apex" and "quit" of the sun's way adopted by him.

It will be remembered that Mr. H. S. Monck, in a letter to the *Observatory*, in December 1888, remarked that, in examining catalogues of comets, he found 177 comets with perihelia north of the ecliptic, against 115 with southern perihelia. With respect to this circumstance, Mr. Monck wrote:—"Our observing stations are chiefly situated in northern latitudes. Comets are rarely visible when very remote from their perihelia; therefore, comets which pass their perihelia north of the ecliptic are more likely to be detected and observed than comets which pass their perihelia to the south of it. . . . As the point towards which the sun is moving lies to the north of the ecliptic, it might be expected that more comets would, on the whole, come to us from the north than from the south. But a comet coming from the north will usually have its aphelion north and its perihelion south. The fact that three-fifths of the comets have their perihelia to the north and their aphelia to the south thus becomes more significant, and I can hardly regard it as *wholly* the result of the position of our observing stations."

Later, however (August 1889), Dr. Holetschek drew attention to a pamphlet "Ueber die Richtungen der grossen Axen der Cometenbahnen," in which he shows that "the tendency of comet perihelia or aphelia to accumulate rather in small latitudes, and about the longitudes 90° and 270° than in other places, can be explained by purely terrestrial considerations, and, consequently, this accumulation offers no proof of the motion of the solar system or of the ultra-solar origin of comets." In fact, it appears that the distribution in latitude of the perihelia of comets is nearly uniform, and has not a marked maximum in the latitudes of the sun's line of flight, although, as Dr. Muirhead indicates, a clustering of aphelia and perihelia occurs near the heliocentric longitude of the line.

THE ROCKS OF THE MOON.—M. Landerer, in continuation of his memoir last year on the polarizing angle of the lunar surface, has just communicated to the Paris Academy the results of some determinations of the angle of polarization of igneous rocks. He finds that specimens from different localities give practically identical results, the probable errors never being greater than $\pm 5'$. The polarizing angle increases from 30° 51′ for ophite, through syenite, basalt (31° 43′), serpentine, trachyte, granite (32° 20′), diorite, diabase, andesite (32° 50′), to obsidian (33° 46′). Vitrophyre, a black rock from the Rhodope chain, which contains large crystals of sanidine, magnetite, and hornblende, in a fluidal, non-perlitic matrix, has a polarizing angle of 33° 18′, which approximates very closely to that of the lunar surface. Without presuming too much on this result, the author regards it as at any rate an additional proof of the similarity, and therefore common origin, of our earth and its satellite. The fact that the polarizing angle of ice is more than 37°, is another objection to M. Hirn's hypothesis of lunar glaciation.

BROOKS'S COMET (*a* 1890).—Dr. Bidschof gives the following ephemeris in *Astronomische Nachrichten*, No. 2979:—

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl.	Log *r*.	Log Δ.	Brightness.
Aug. 1	13 11 1	+45 37·9	0·3121	0·3714	1·38
5	13 7 39	43 57·9	0·3160	0·3849	1·27
9	13 5 4	42 24·2	0·3200	0·3978	1·18
13	13 3 9	40 56·5	0·3242	0·4100	1·09
17	13 1 45	39 34·5	0·3285	0·4215	1·02
21	13 0 47	38 17·7	0·3330	0·4323	0·95
25	13 0 12	37 5·8	0·3376	0·4423	0·89
29	12 59 58	35 58·6	0·3422	0·4517	0·83
Sept. 2	12 59 54	34 56·1	0·3470	0·4605	0·78

The brightness on March 21 has been taken as unity.

BRORSEN'S COMET.—Mr. E. Barnard, of Lick Observatory, notes, in the above number of *Astronomische Nachrichten*, that he has made many searches for this comet from December of last year to the end of April, but with only a negative result. He notes that during the search he has found several unrecorded nebulæ.

TWO NEW COMETS (*b* and *c* 1890).—M. Coggia, at Marseilles, has discovered a pretty bright comet having the following positions (*Astronomische Nachrichten*, 2980).

Marseilles Mean Time. h. m.	R.A. h. m. s.	Decl. ° ′ ″
July 18 ... 10 31·0	8 48 51·0	+44 42 48
19 ... 9 38·8	8 55 58	44 2 48

Mr. Denning discovered a faint comet at Bristol on July 23; its position at 13 hours Greenwich mean time being R.A. 15h. 12m., and Decl. +78°. It was moving towards the east (*Edinburgh Circular*, No. 8).

A NEW ASTEROID (294).—M. Charlois, of Nice Observatory, discovered an asteroid of the twelfth magnitude on the 15th inst.

THE SCIENTIFIC PRINCIPLES INVOLVED IN MAKING BIG GUNS.

II.

PART II.—THE STRAINS IN THE GUN.

(29) SO far we have dealt only with the stresses in the metal, and we have determined these stresses in the manner given by Rankine in his "Applied Mechanics," § 273, p. 290, in which the only assumption made is that the metal of each cylinder is homogeneous. But when the gun-maker wishes to set up a given pressure of shrinkage between two cylinders, he has to determine, by calculation or experiment, the slight amount by which, when cold, the external radius of one cylinder must exceed the internal radius of the next cylinder which is shrunk on it; the outer cylinder being expanded by heat and slipped on, in order that the given initial pressure may be set up on the cooling of the outer cylinder; and this, too, when other cylinders are shrunk on afterwards.

We must therefore determine the strains and deformations set up in a given cylinder due to given applied pressures, and thus we require the equations giving the strains due to given applied stresses when the coefficients of elasticity of the metal are known.

[1] Continued from p. 309.

(30) Now, it is proved in Thomson and Tait's "Natural Philosophy," §§ 682, 683, for a substance of which k is the elasticity of volume or bulk-modulus, and n is the elasticity of figure or rigidity, that when the stress is a simple longitudinal tension P, the principal strains in the substance are an extension $P\left(\frac{1}{3n}+\frac{1}{9k}\right)$ in the direction of the tension, and a compression $P\left(\frac{1}{6n}-\frac{1}{9k}\right)$ in all directions perpendicular to the tension.

We use the words *tension* and *pressure*, as before, to denote *stresses* measured in terms of pull or thrust per unit area, with our practical units, measured in tons per square inch; while the words *extension* and *compression* are used (in accordance with the terminology of Maxwell, Everett, and Unwin) to mean the *strains*, measured by the ratio of linear elongation or contraction to the original length.

Thus, the *tension* or *pressure* being the *stress*, the *extension* or *compression* is the corresponding *strain*; and Hooke's law of elasticity (*ut tensio sic vis*), translated into a formula, gives $\frac{\text{stress}}{\text{strain}} = \frac{\text{tension}}{\text{extension}} = \frac{\text{pressure}}{\text{compression}} =$ the modulus of elasticity.

(31) Then, by superposition, if e, f, g are the extensions produced in three rectangular directions by tensions P, Q, R in these directions—

$$e = \left(\frac{1}{3n}+\frac{1}{9k}\right)P - \left(\frac{1}{6n}-\frac{1}{9k}\right)(Q+R), \ldots (31)$$

with two similar expressions for f and g; or, in Thomson and Tait's notation, § 694—

$$Me = P - \sigma(Q+R), \quad \ldots\ldots (32)$$
$$Mf = Q - \sigma(R+P), \quad \ldots\ldots (33)$$
$$Mg = R - \sigma(P+Q), \quad \ldots\ldots (34)$$

where

$$\frac{1}{M} = \frac{1}{3n}+\frac{1}{9k}, \text{ or } M = \frac{9nk}{3k+n},$$

so that M is Young's modulus of elasticity, the modulus which is directly observable when a test-piece of the substance (steel) is placed in a testing-machine, and the ratio $M = P/e$ is observed of P, the tension, to e, the extension, no lateral tension being applied, or

$$Q = 0, \ R = 0;$$

also,

$$\sigma = \frac{3k-2n}{6k+2n},$$

called Poisson's ratio, is the ratio of the lateral compression to the linear extension of the substance when the stress is a simple tension.

(32) Again, by independent investigation, as in § 692, or by solution of the preceding equations (32, 33, 34), we find—

$$P = \left(k+\frac{4}{3}n\right)e + \left(k-\frac{2}{3}n\right)(f+g), \ldots (35)$$
$$Q = \left(k+\frac{4}{3}n\right)f + \left(k-\frac{2}{3}n\right)(g+e), \ldots (36)$$
$$R = \left(k+\frac{4}{3}n\right)g + \left(k-\frac{2}{3}n\right)(e+f); \ldots (37)$$

or, in Lamé's notation ("Théorie de l'Élasticité," § 19)—

$$P = \lambda\theta + 2\mu e,$$
$$Q = \lambda\theta + 2\mu f,$$
$$R = \lambda\theta + 2\mu g,$$

with

$$\theta = a + b + c,$$

the cubical expansion; and

$$\lambda = k - \frac{2}{3}n, \ \mu = n.$$

The above equations show that when the strain is given as a simple uniform longitudinal extension e, the stresses consist of a uniform longitudinal tension, $(k+\frac{4}{3}n)e = (\lambda+2\mu)e$, in the direc-

tion of the strain, and of uniform lateral tension, $(k - \frac{2}{3}n)e = \lambda e$, in every direction perpendicular to the strain.

(33) These equations, and the previous equations, which show that, when the stress is a simple uniform longitudinal tension P, the strains consist of a uniform extension P/M in the direction of the tension, and of uniform lateral compression σP/M perpendicular to the tension, are so fundamental in the theory of the elasticity of isotropic bodies, that we are almost tempted to make a digression here on their proof, in the manner given in Thomson and Tait's "Natural Philosophy," §§ 682, 683, and 692.

It is necessary to describe and compare the notations carefully, for subsequent purposes, as the variety of notation in the subject of elasticity is very confusing.

(34) Applying these principles to the gun, we take the three principal directions of stress and strain, as (i.) circumferentially to the gun, (ii.) radially, (iii.) longitudinally; and now, estimating *tensions* and *extensions* as positive, we have—

$$\mathrm{P} = t = ar^{-2} - b,$$
$$\mathrm{Q} = -p = -ar^{-2} - b;$$

while the value of R is still indeterminate.

For the determination of the strains, we denote by u the *increase* of radius, r, of a circumferential fibre; and then $2\pi u$ being the elongation of the fibre of original length $2\pi r$, the circumferential extension

$$e = 2\pi u/2\pi r = u/r;$$

while the radial extension $f = du/dr$; the longitudinal extension g being as yet undetermined.

(35) Expressing the strains e and f in terms of the longitudinal tension R,

$$\begin{aligned} \mathrm{M}e = \mathrm{M}u/r &= \mathrm{P} - \sigma(\mathrm{Q} + \mathrm{R}) \\ &= ar^{-2} - b + \sigma(ar^{-2} + b) - \sigma\mathrm{R} \\ &= (1 + \sigma)ar^{-2} - (1 - \sigma)b - \sigma\mathrm{R}; \end{aligned}$$

or

$$\mathrm{M}u = (1 + \sigma)ar^{-1} - (1 - \sigma)br - \sigma\mathrm{R}r; \quad . \quad . \quad . \quad (38)$$

so that, differentiating with respect to r,

$$\begin{aligned} \mathrm{M}f &= \mathrm{M}du/dr \\ &= -(1 + \sigma)ar^{-2} - (1 - \sigma)b - \sigma d(\mathrm{R}r)/dr \\ &= \mathrm{Q} - \sigma\mathrm{P} - \sigma d(\mathrm{R}r)/dr. \end{aligned}$$

But with

$$\mathrm{M}f = \mathrm{Q} - \sigma(\mathrm{R} + \mathrm{P}),$$

Barlow, Lamé, and Hart's expressions for the stresses are verified, provided that

$$d(\mathrm{R}r)/dr = \mathrm{R};$$

or

$$d\mathrm{R}/dr = 0, \qquad \mathrm{R} = \text{constant}.$$

(36) On the other hand, expressing the strains e and f in terms of the longitudinal strain or extension g, since

$$\begin{aligned} \mathrm{R} &= \sigma(\mathrm{P} + \mathrm{Q}) + \mathrm{M}g, \\ \mathrm{M}e = \mathrm{M}u/r &= \mathrm{P} - \sigma(\mathrm{Q} + \mathrm{R}) \\ &= (1 - \sigma^2)\mathrm{P} - \sigma(1 + \sigma)\mathrm{Q} - \sigma\mathrm{M}g \\ &= (1 - \sigma^2)(\mathrm{P} - \sigma'\mathrm{Q}) - \sigma\mathrm{M}g, \end{aligned}$$

putting

$$\sigma' = \frac{\sigma}{1 - \sigma};$$

so that

$$\mathrm{M}u = (1 - \sigma^2)\{(1 + \sigma')ar^{-1} - (1 - \sigma')br\} - \sigma\mathrm{M}gr; \; . \; (38^*)$$

and differentiating with respect to r,

$$\begin{aligned} \mathrm{M}f &= \mathrm{M}du/dr \\ &= (1 - \sigma^2)(\mathrm{Q} - \sigma'\mathrm{P}) - \sigma\mathrm{M}d(gr)/dr, \end{aligned}$$

agreeing again in giving

$$\mathrm{M}f = \mathrm{Q} - \sigma(\mathrm{R} + \mathrm{P}),$$

provided that

$$d(gr)/dr = g;$$

or

$$dg/dr = 0, \qquad g = \text{constant}.$$

We have proved, then, that either the longitudinal tension R or the longitudinal extension g of the gun must be uniform, for the values of the stresses given by the formulas of Barlow, Lamé, Hunt, and Rankine, to be strictly accurate; we shall follow the ordinary practice in assuming that R is uniform, but the work will be almost precisely the same if we assume that g is uniform (Prof. P. G. Tait, "On the Accurate Measurement of High Pressures," Proc. R.S. Edinburgh, 1879–80).

(37) Now, let us determine, for the simplest case of the tube A and the jacket B, the requisite *shrinkage* for producing a given initial pressure $\varpi_o = p_i$ at their common surface; the shrinkage, denoted by S, being defined as the excess of the outside diameter $2\rho_o$ of the tube A over the inside diameter $2r_i$ of the jacket B, when both are finished cold in the lathe; so that

$$\tfrac{1}{2}\mathrm{S} = \rho_o - r_i.$$

The jacket B is now expanded by heat till its inside diameter is greater than $2\rho_o$, and then slipped over the tube A; on cooling, the jacket B shrinks and grips the tube A with the requisite pressure, $p_i = \varpi_o$.

Taking the practical rule that the expansion of steel is one-ten-thousandth for every 15° F., the jacket must be raised in temperature something over 150,000 $\mathrm{S}/2r_i$ degrees Fahr.

Denoting by u and v the outward displacement of any circumferential fibre of the jacket or tube, of radius r in the jacket and ρ in the tube; then, since the tube and jacket fit closely at their common surface,

$$\rho_o + v_o = r_i + u_i,$$

or

$$u_i - v_o = \rho_o - r_i = \tfrac{1}{2}\mathrm{S}.$$

(38) Supposing the tube and jacket to be both of steel of the same quality, so that M, the modulus of elasticity, is the same for both; and assuming that R is uniform, then in the jacket B, from (38),

$$\mathrm{M}u_i = (t_i + \sigma p_i)r_i - \sigma\mathrm{R}r_i,$$

and in the tube A,

$$\mathrm{M}v_o = (-\tau_o + \sigma\varpi_o)\rho_o - \sigma\mathrm{R}\rho_o;$$

and now, since $p_i = \varpi_o$, and we may put $r_i = \rho_o$, subtraction gives—

$$\mathrm{M}(u_i - v_o) = (t_i + \tau_o)r_i,$$

or

$$\mathrm{S} = (t_i + \tau_o)2r_i/\mathrm{M}; \; . \; . \; . \; . \; . \; . \; (39)$$

and t_i and τ_o having been determined either from the formulas (6) to (16), or graphically from Fig. 3, from the given value of $p_i = \varpi_o$, the requisite value is determined of the shrinkage S, or of $\mathrm{S}/2r_i = (t_i + \tau_o)/\mathrm{M}$, which is the shrinkage, estimated as a fraction of the diameter.

This formula shows us that the shrinkage S is the elongation or contraction which would be produced in a bar of steel, one square inch in section, and equal in length to the diameter $2r_i$, by a pull or thrust of $t_i + \tau_o$ tons.

If we had taken g as uniform, we should find in a similar manner—

$$\mathrm{S} = (1 - \sigma^2)(t_i + \tau_o)2r_i/\mathrm{M}. \; . \; . \; . \; . \; (40)$$

With steel, $\sigma = \frac{1}{4}$ about, so that $\sigma' = \frac{1}{3}$; and the values of the shrinkage calculated on the two assumptions of uniform R and uniform g, would be in the ratio of 1 to $1 - \sigma^2$, or as 16 to 15; thus differing by about 6 per cent., a difference which is practically insensible.

(39) In the numerical example we have given of the initial stresses of the tube and jacket, $t_i = 5$, $\tau_o = 5$; so that $\mathrm{S}/2r_i = 10/\mathrm{M}$.

For gun steel M = 12,600 about (Unwin, "Testing of Materials of Construction," p. 249); and supposing the tube and jacket to represent a 3-inch field gun, $2r_i = 6$; and then

$$\mathrm{S} = 1/210 = 0{\cdot}00476,$$

4·76 thousands of an inch.

(40) In heavy guns, one or more hoops are shrunk on over the jacket; for instance, in the 110-ton gun, three such series are superposed. Diagrams in section of modern guns will be found in recent numbers of the *Engineer* and of *Engineering*.

The addition of each hoop that is shrunk on modifies the initial stresses previously existing. The annexed diagram (Fig. 7), taken from the American "Notes on the Construction of Ordnance," Nos. 31, 33, 35, by Lieutenant Rogers Birnie, shows the shrinkage (enlarged 50 times) of the different parts, and the intermediate and final arrangements when a jacket, B, an inner hoop, C, and an outer hoop, D, are successively shrunk on the tube A of the American 8-inch gun, shown in longitudinal section in Fig. 8.

But knowing the initial stresses in the gun, as determined in the manner already explained in Part I., we can determine the requisite shrinkage at each common surface, for any number of layers, by a formula as simple as that just found for the tube A and jacket B, if only we assume that M, the modulus of elasticity, is the same throughout.

(41) Denote, as before, by p_m, the radial pressure at the radius, r_m, of the common surface of the mth and $m + 1$th hoops, as reckoned from the interior; and by t'_m, t_m the circumferential tensions at the exterior radius, r_m, of the mth hoop, and at the interior radius, r_m, of the $m + 1$th hoop.

Denote also by u'_m, u_m, the outward radial displacement from the unstrained position of the outer surface of the mth hoop, and of the inner surface of the $m + 1$th hoop.

Then, with uniform R, from (38),

$$Mu_m = (t_m + \sigma p_m)r_m - \sigma R r_m,$$

$$Mu'_m = (t'_m + \sigma p_m)r_m - \sigma R r_m;$$

so that

$$M(u_m - u'_m) = (t_m - t'_m)r_m.$$

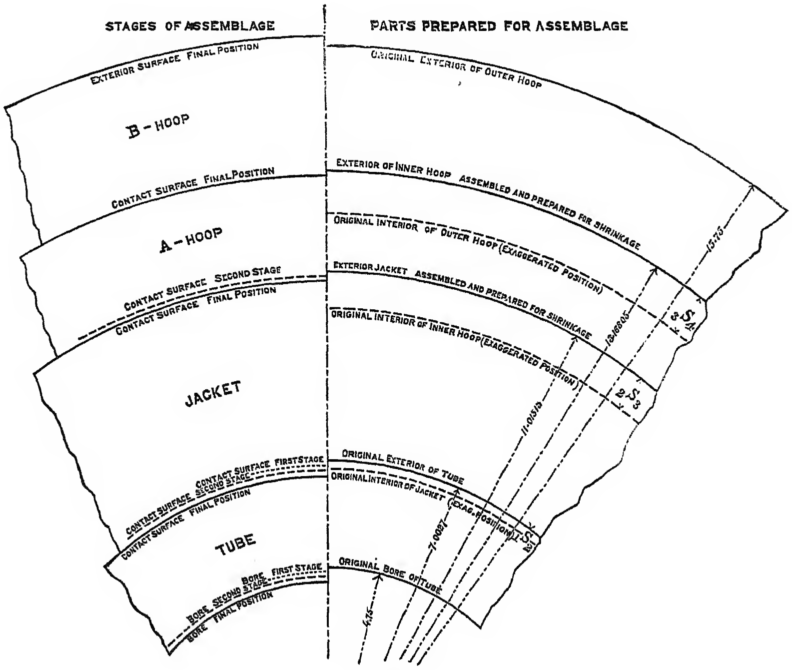


FIG. 7.

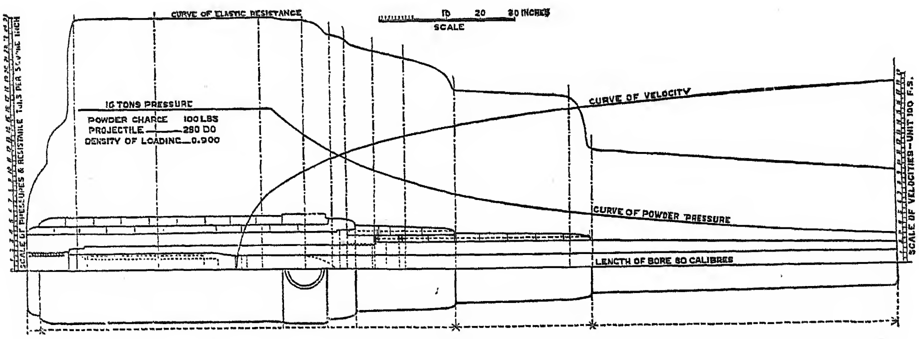


FIG. 8.

Now, using the notation ${}_mS_{m+1}$ to denote the shrinkage when unstrained between the mth and $m + 1$th hoops,

$${}_mS_{m+1} = 2(u_m - u'_m)$$

$$= (t_m - t'_m)2r_m/M, \quad . \quad . \quad . \quad . \quad (41)$$

the same as for a single tube, A, and jacket, B; and showing that the shrinkage, ${}_mS_{m+1}$, is the elongation or contraction which would be produced in a bar of steel, of modulus of elasticity M, one square inch in section, and of length equal to the diameter $2r_m$, by a pull or thrust of $t_m - t'_m$ tons. On the assumption of uniform g, we should find—

$${}_mS_{m+1} = (1 - \sigma^2)(t_m - t'_m)2r_m/M, \quad . \quad . \quad . \quad (42)$$

practically the same as for uniform R.

(42) The stress formulas in the mth hoop give—

$$2a_m = (p_m + t'_m)r_m^2 = (p_{m-1} + t_{m-1})r_{m-1}^2.$$

$$2b_m = p_m - t'_m = p_{m-1} - t_{m-1};$$

so that

$$t_m - t'_m = (t_m - t_{m-1}) - (p_m - p_{m-1}).$$

These values of t_m and t'_m are the initial stresses or circumferential tensions; and as the powder pressure p_0 increases them by equal amounts, their difference is unaltered; so that $t_m - t'_m$ is the same for the initial stresses or the firing stresses; and we may calculate the shrinkage, S, by the above formula from the values of the firing stresses, or of the initial stresses; the former being chosen, as given more directly when the maximum allowable tensions, represented by t_m, are given.

(43) As a numerical illustration, let us calculate the shrinkages in the American 8-inch gun, taking the previous results of § 22, and M = 12,600 (tons per square inch) for all the coils.

Then the final contraction of the bore

$$_0S_1 = t_0 \times 2r_0 \div M = 19{\cdot}9 \times 10 \div 12{,}600 = 0{\cdot}016,$$

or 16 thousandths of an inch; and similarly,

$$_1S_2 = 12{\cdot}7 \times 14 \div 12{,}600 = 0{\cdot}014;$$
$$_2S_3 = 10{\cdot}7 \times 22 \div 12{,}600 = 0{\cdot}019;$$
$$_3S_4 = 3{\cdot}6 \times 26 \div M = 0{\cdot}007;$$
$$_4S_5 = 8{\cdot}1 \times 32 \div M = 0{\cdot}021,$$

the elongation of the external diameter of the last coil.

Lieutenant Rogers Birnie, following Clavarino ("Note on the Construction of Ordnance," No. 6), calls the *extension* or *compression* the *relative elongation* or *relative contraction*; so that the above values of $_0S_1$, $_1S_2$, $_2S_3$, $_3S_4$, $_4S_5$, must be divided by 10, 14, 22, 26, 32, to obtain his values of the relative elongation or contraction; and then, by § 37, 150 thousand times the relative elongation or contraction is the number of degrees Fahrenheit a jacket or coil must be raised in temperature to be expanded sufficiently so as to slip over the inner cylinders.

A. G. GREENHILL.

(*To be continued.*)

THE TOKIO TECHNICAL SCHOOL.

THE *Japan Weekly Mail* in a recent article describes the Tokio Technological School, situated at Asakusa, a suburb of that city. The inclosure in which the school buildings stand formerly belonged to the Shôgun's Government, and was used for the storage of rice. Several of its storehouses, which were ranged round a creek or blind canal leading off the river, still remain, and are utilized by the institution. A frame building of two stories, the chief modern portion, faces the roadway and runs at right angles to the creek. Here are the offices, show-rooms, and lecture-rooms; the workshops are to be found between this building and the river. There are two great departments in the school, the Technological and the Mechanical. Of these the former is the more varied and interesting. To it are attached a dyeing shop, porcelain and glass furnaces, and technological laboratories; to the mechanical department are attached a drawing office, a pattern shop, and a foundry.

The history of the school begins with its foundation in 1882, for the purpose of training foremen and managers for manufactories, and instructors for industrial schools. It was intended that the course of instruction should include all branches of industrial education concerned with arts and manufactures. The course was to extend over three and a half years, of which the first year should be devoted to general preparatory instruction and the others to special training in some particular branch. Next year certain alterations were made, making the course one of four years, and raising the standard. In August of that year the first batch of students, numbering sixty in all, were admitted. The school was shortly afterwards brought into connection with the Imperial University, and placed under the control of that institution—a step which led to a complete change in its curriculum. The preparatory course was abolished, and a short complete course, extending over two years, was instituted. Again, in 1888, a new Imperial decree severed its connection with the University, and placed it under the direct control of the Education Department. The school set itself anew to remodel its course of instruction, abolishing the short general course and resuming the course of three years; and elective courses were established with the view of making the school more popular and generally useful to mechanics and craftsmen. The laboratories and workshops are each provided with responsible superintendents, foremen, and assistants. The general direction is in the hands of a Committee, consisting of the manager of the school, two officials of the Education Department, and two officials of the Department of Agriculture and Commerce. Candidates for entrance to the regular courses must be not under seventeen nor over twenty-five years of age, and unless they have passed satisfactorily through a normal or middle school, must undergo an examination in Japanese, arithmetic, algebra and geometry, physics and chemistry, and English translation. Students sent up by local governments need not undergo this examination. The elective courses have been instituted for the benefit of artisans and mechanics, who, having no general scientific training, are anxious to study some part of the regular course. These candidates receive this privilege only when the convenience of the school admits of it, and are allowed to study for two years, taking one or more of the subjects immediately connected with their special crafts. An elective student must be at least seventeen years of age, and must have followed, for more than one year, some trade having special relation to the subjects of instruction which he has chosen. The fee paid by these students is about 3*s.* monthly.

In the mechanical engineering section-boilers, steam-engines, force-pumps—these last happen now to be in great demand in Japan as an improvement on the clumsy well-bucket—and sawing-machines are manufactured. The shop is also prepared to execute orders for steam and hot-water heating apparatus, and has already fitted up the new Engineering College in the University grounds with a complete set of hot-water pipes and fittings. All the casting and founding required by the College are carried out at the Asakusa School. An improved pattern of perforating machine, now in use at the Imperial Printing Office, is also turned out. It is claimed for this pattern that it possesses a superiority over the one in common use in Europe for perforating stamps and other paper. Experiments are likewise in process on printing-presses, with the view of perfecting a machine for native use. The dyeing department is chiefly concerned with practical instruction in the best methods of fixing colours, rather than in any more original researches. Of late years the importation into Japan of aniline dyes has increased to such an extent that the total annual value of these imports now exceeds £35,000. Unfortunately, although these colours are very attractive to buyers, their proper use is still little understood. Silk, cotton, and other fabrics which have been coloured by native dyers do not wash well, and half the imported dye-stuffs run to waste. It is one of the chief aims of the instructors in this department to teach artisans how to fix these colours. Just now the school dye-shop is busying itself with this particular branch, and also with a series of experiments on the dyeing of mountain silk. This silk, which is soft in texture and durable in wear, refuses the ordinary dye, a circumstance attributable to the presence in it of a large amount of calcium carbonate. The pottery and glass department is associated with the name of Dr. Wagner, who has for a long series of years enjoyed the confidence of the Japanese Government. Dr. Wagner is admitted to be the best authority on all matters connected with Japanese technology, and has directed his particular attention to the fabrication of a ware known in Japan as *asahi-yaki*, and elsewhere as Dr. Wagner's faience. Unlike the Satsuma, which is also faience, but of a much harder kind, this ware receives its decoration when in its unglazed state, which is a manifest advantage. It is made chiefly from a clay found in the Enya district of the Tochigi prefecture, with slight admixture of clays from other localities. The colour of the faience when baked varies from white, having a warm brown tinge, to lightish pink. Much of the *asahi-yaki* is exported to Germany and to the United States, and a certain amount to France, but little or none finds its way to Great Britain. Artists are at work on the spot decorating the plates and other articles preparatory to the receiving of the glaze. The object which Dr. Wagner and his colleagues have in view is technological and not artistic, and consists in perfecting native potters in the manipulation of the material.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for June 1890 contains:—On the embryology of a scorpion (*Euscorpius italicus*), by Malcolm Laurie (Plates xiii.-xviii.). The develop-

ment of this scorpion, of which very elaborate details are given, would appear not to agree closely with any other Arachnid type as yet described; the development of the central and lateral eyes entirely confirms the descriptions of Lankester and Bourne, as well as those of Parker, but Patten's conclusions are shown to be without foundation. The mode of formation of the ventral nervous system is exceptional among Invertebrates, resembling rather that of Chordata.—On the morphology of the compound eyes of Arthropods, by S. Watase (Plate xix.). Reprinted, with a short introduction by the editor, from a recent number of the "Studies from the Biological Laboratory, Johns Hopkins University."—On the structure of a species of earthworm belonging to the genus Diachæta, by Frank Beddard (Plate xx.). This new species, *D. windlei*, is from the Bermudas.—On *Hekaterobranchus shrubsoli*, a new genus and species of the family Spionidæ, by Florence Buchanan (Plates xxi. and xxii.). This worm was found at Sheppey in soft mud, usually covered by an inch or so of brackish water; in addition to the figures of the anatomical details there are coloured portraits of this Annelid.—An attempt to classify earthworms, by Dr. W. B. Benham. Some idea may be formed of the progress made within the last twenty years in our knowledge of this group when we state that the author enumerates and gives analyses of nine families of Lumbricomorpha, containing thirty-two genera and over 200 species. The author wishes the following correction made:—In Fig. 39, which illustrates the anatomy of Lumbricus, the œsophageal pouch (CP) is placed in somite xi.; followed by a pair of calciferous glands in the same somite and a second pair in somite xii. The pouch (CP) should be in somite x.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 19.—"Contributions to the Molecular Theory of Induced Magnetism." By J. A. Ewing, F.R.S., Professor of Engineering in University College, Dundee.

After referring to the discussion by Maxwell of Weber's theory, which ascribes the magnetization of iron and other magnetic metals to the turning towards one direction of molecules which are already permanent magnets, and to suggestions by Profs. Wiedemann and Hughes, and lately by Mr. A. E. Kennelly (the *Electrician*, June 6 and 13, 1890), the writer describes experiments which he has made bearing directly on the molecular theory. The experiments have been made by grouping near to one another a large number of small pivotted magnets each free to turn about a fixed centre, and studying the configuration which the group assumes and the manner in which it yields when an external magnetic force is imposed. The results do not support the idea that the molecular magnets form closed chains in unmagnetized iron. They lead, however, to the important conclusion that no arbitrary conditions of directional constraint need be postulated to make the behaviour of the molecular magnets agree with what is known about magnetic quality.

In the writer's view the molecular magnets are perfectly free to turn in response to external magnetic forces, except in so far as they are constrained by the magnetic forces which they mutually exert on one another. This theory is briefly discussed in the paper in relation to the form of the magnetization curve, to the character of cyclical processes, and to the known effects of temperature, vibration, stress and so forth, and the following conclusions are stated:—

(1) That in considering the magnetization of iron and other magnetic metals to be caused by the turning of permanent molecular magnets, we may look simply to the magnetic forces which the molecular magnets exert on one another as the cause of their directional stability. There is no need to suppose the existence of any quasi-elastic directing force or of any quasi-frictional resistance to rotation.

(2) That the intermolecular magnetic forces are sufficient to account for all the general characteristics of the process of magnetization, including the variations of susceptibility which occur as the magnetizing force is increased.

(3) That the intermolecular magnetic forces are equally competent to account for the known facts of retentiveness and coercive force, and the characteristics of cyclic magnetic processes.

(4) That magnetic hysteresis and the dissipation of energy which hysteresis involves are due to molecular instability resulting from intermolecular magnetic actions, and are not due to anything in the nature of frictional resistance to the rotation of the molecular magnets.

(5) That this theory is wide enough to admit explanation of the differences in magnetic quality which are shown by different substances, or by the same substance in different states.

(6) That it accounts in a general way for the known effects of vibration, of temperature and of stress, upon magnetic quality.

(7) That, in particular, it accounts for the known fact that there is hysteresis in the relation of magnetism to stress.

(8) That it further explains why there is, in magnetic metals, hysteresis in physical quality generally with respect to stress, apart from the existence of magnetization.

(9) That, in consequence, any not very small cycle of stress occurring in a magnetic metal involves dissipation of energy.

Anthropological Institute, June 24.—E. W. Brabrook, Vice-President, in the chair.—Mr. J. E. Price exhibited parts of a skeleton found at West Thurrock, Essex.—Mr. H. H. Risley read a paper on the study of ethnology in India. This paper states the results of certain inquiries into the customs and measurements of the features, stature, &c., of some of the chief tribes and castes in India, conducted during the last five years under the authority of the Government of Bengal. Owing to the influence of the caste system, which forbids intermarriage between members of different castes, India offers a peculiarly favourable field for anthropological researches. The measurements disclose the existence of two extreme types—the Aryan and Dravidian. The Aryan type—as represented by the Brahmans, the Rajputs, and the Sikhs—is tall and fair, with a finely cut nose, and features on the whole superior to those of the average European. The Dravidians, as seen in the Kol tribes, who recently revolted against the oppression of their Hindu landlords, are short and very black, with a broad flat nose, closely approaching in its dimensions to that of the Negro. The proportions of the nose are regarded by European anthropologists—by Prof. Flower, F.R.S., of the British Museum, and Prof. Topinard, of Paris—as the best test of race distinctions. The Indian statistics bear out this opinion. They show that in Bengal caste is so closely connected with race that the social standing of a caste is in inverse ratio to the average width of the noses of its members. The lower the caste the broader and more Negro-like is its nose; and conversely, in ascending the social scale, we meet with continually finer noses, till in the higher castes European proportions are reached. The proportions of the head are of interest in connection with the theory propounded by Herr Karl Penka, of Vienna, and favoured by Prof. Sayce, that the Aryans were a dolichocephalic (long-headed) race who came originally from Scandinavia. The long-headed type is very numerous in the Punjab and North-West Provinces at the present day, and its distribution is such as to give considerable support to Herr Penka's opinions. The inquiry has also brought to light the existence in Bengal of totems such as are found among the North American Indians. Large tribes, like the Kols, are subdivided into two or three hundred groups, each of which is called after an animal, a tree, or a plant; and the rule is that a member of a particular animal group, such as the snakes, the tortoises, the eels, or the mangooses, may not marry within that group. Thus a snake man may not marry a snake woman, but must select his bride from among the frogs, the tortoises, the mango-trees, or a host of groups which include the whole fauna and flora of the district. The paper attempts to account for this custom, which the late Mr. J. F. McLennan called *exogamy*, by connecting it with the theory of natural selection. Among other interesting facts the Bengal inquiry shows that the practice of infant marriage, and the custom forbidding widows to marry a second time, are greatly on the increase, and are being adopted by the lower castes as marks of social distinction. It is feared that the spread of infant marriage will have a weakening effect on the race, and will multiply and aggravate those special diseases of women which Lady Dufferin's Fund was instituted to deal with. The increase in the number of widows is in itself a great evil. It lowers the position of women in India, and tends to lower the standard of social morality.

PARIS.

Academy of Sciences, July 24.—M. Hermite in the chair.—M. Boussinesq presented the second and last volume of his "Course of Infinitesimal Analysis," and commented upon the

application therein given of the integral calculus to physics and mechanics.—New researches on the relative stability of salts in the solid and dissolved state: aniline salts, by M. Berthelot. The author compares the heat of formation and the properties of the more stable aniline salts, such as the sulphate, nitrate, and chloride with the unstable ones, *e.g.* the acetate and benzoate. The observations furnish a new confirmation of thermo-chemical theories.—Heat of formation of certain amides, by MM. Berthelot and Fogh. The amides investigated are acetamide, propionamide, benzamide, and succinimide; and the experiments show that the heat of formation of anilides, *e.g.* acetanilide and benzanilide, is greater than that of the corresponding amides.—The share of the end-plates of motor nerves in the expenditure of the energy which produces contraction; influence exercised on the heating of a muscle by the number and nature of the changes of state which the end-plates excite in the contractile bundle, by M. A. Chauveau.—Discovery of a comet by M. Coggia at Marseilles Observatory. (See Our Astronomical Column.)—On the means of recognizing the *Cysticerci* (bladder worms) of *Tænia saginata*, which cause "measles" in the calf and ox, in spite of the rapid disappearance of the *Cysticerci* on exposure to the air, by M. A. Laboulbène.—On the sensibility of plants when regarded as ordinary reagents, by M. Georges Ville. The author has extended to peas and wheat his observations in 1867 on yeast as a test for phosphoric acid, and finds that their varying growth is an indication of extreme delicacy for very minute amounts.—On the production by electric discharges of images reproducing the principal characteristics of solar activity, by M. Ch. V. Zenger.—On the combination of observations, by M. R. Lipschitz. This is an extension of Gauss's application of the calculus of probabilities to errors of observation.—The diagrammometer: an additional apparatus for the study of curves, by Colonel Kozloff.—On the physical property of the surface of contact of two liquids under the influence of mutual affinity, by M. G. Van der Mensbrugghe.—On internal crystalline reflection, by M. Bernard Brunhes.—On the double elliptic refraction of quartz, by M. F. Beaulard.—On a magnetic anomaly observed in the neighbourhood of Paris, by M. Th. Moureaux. A discussion of the earliest results of a detailed magnetic survey of France now being made indicates that regions of local disturbance exist in the Paris basin.—Researches on the double phosphates of titanium, tin, and copper, by M. L. Ouvrard.—Researches on the optical dispersion of organic compounds: the ethers, by MM. Ph. Barbier and L. Roux.—Upon certain hydrates of the haloid esters, by M. Villard. The author finds that the iodide and fluoride of methyl form hydrates like the chloride and bromide. Experiments on the haloid compounds of ethyl show that the chloride and fluoride yield similar hydrates. The fluorides were gases prepared by M. Moissan's process, and yielded colourless crystalline hydrates.—On oxygluconic acid, by M. L. Boutroux. The author has obtained by the oxidation of either glucose or gluconic acid by bacterial action an acid, to which he gives the name oxygluconic, having the formula of glucoronic acid, $C_{12}H_{10}O_{14}$, but differing from the latter in being lævorotatory, very soluble in alcohol, and not yielding crystals on evaporation. The new acid appears to be identical with one recently obtained by M. Emile Fischer by the replacement of the acid radical of saccharic acid with an aldehyde group, using the action of sodium amalgam on its lactone.—On the examination of the impurities contained in alcohol, by M. Ed. Mohler.—On a new process for the determination of mineral matters in sugar by means of benzoic acid, by M. E. Boyer.—On the mineral springs of Cransac (Aveyron), by M. Ad. Carnot.—On the combinations of hæmoglobin with oxygen, by M. Christian Bohr.—Possibility of injections into the human trachea as a means of introducing medicines, by M. R. Botey.—Claim of priority in the discovery of craniectomy, by M. Guéniot.—On the mechanism of respiration in *Ampullaria*, by MM. Paul Fischer and E. L. Bouvier.—On the repair of the shell in *Anodon*, by M. Moynier de Villepoix. Numerous experiments on the growth in water with varying amounts of chalk in solution of the shell after artificial injuries indicate that it is a product of secretion of the mantle, that it is at first a purely organic formation, and that the lime for its consolidation is obtained from the surrounding medium.—On the secretion of silk in *Bombyx mori* (common silkworm), by M. Raphael Dubois. —The gangrene of the potato stem, a bacterial disease, by MM. Prillieux and G. Delacroix.—On the angle of polarization of igneous rocks and the chief lunar deductions therefrom, by M. J. J. Landerer. (See Our Astronomical Column.)

Berlin.

Physiological Society, June 18.—Prof. du Bois-Reymond, President, in the chair.—Dr. Blumenau gave an account of his researches on the development of the corpus callosum, carried out chiefly upon the brains of embryonic pigs, from which he concluded that the grey matter on the upper and lower sides of this structure grows by a fusion with the neighbouring bundles of arched fibres.—Prof. H. Virchow spoke on the gill-slits of the sturgeon, which he had examined with a view to finding a transitional form between the gills of Selachians and the osseous fishes. His anatomical and embryological investigations showed that with reference to its gills the sturgeon does not occupy that intermediate position which has been assigned to it by zoologists.—Prof. Gad described an experimental confirmation by Dr. Zagari of Donders's statement, denied by Knoll, that the inhaling of carbonic acid at the end of an expiration materially increases the depth of the ensuing inspiration. He had further found that this reflex effect is not observed after section of the vagi, and is not affected by section of the recurrent laryngeals. It did not take place when a glass tube was pushed down the trachea and one bronchus, so as to protect these portions of the air-passages from the action of the gas; but it reappeared on withdrawing the tube until its end rested at the bifurcation of the bronchi. From this it follows that the reflex inspiration is set up by the action of the gas on the mucous membrane of the bronchi. The effect was observed when the carbonic acid gas was diluted with 50 per cent. of air, but not upon further dilution. Marshall Hall's theory of respiration receives no confirmation from the above experiments. The concentrated CO_2 which makes its exit into the lungs themselves is probably inactive owing to its inevitable dilution by the residual air.

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THURSDAY, AUGUST 7, 1890.

THE HISTORY OF BOTANY.

History of Botany (1530–1860.) By Julius von Sachs. Authorized Translation by H. E. F. Garnsey, M.A. Revised by Prof. I. Bayley Balfour, F.R.S. (Oxford: Clarendon Press, 1890.)

AFTER fifteen years' interval, this admirable book has made its appearance in English. The translation does justice to the original, and to say this is very high praise, for the "History of Botany" is perhaps the most generally interesting, and the most finished in style, of all Prof. Sachs's works.

There have been scarcely any alterations in this edition, which still represents the state of the author's mind in 1875. To quote his words, in his preface to the present translation:—

"I came to the conclusion that my book itself may be regarded as a historical fact, and that the kindly and indulgent reader may even be glad to know what one, who has lived wholly in the science, and taken an interest in everything in it, old and new, thought from fifteen to eighteen years ago of the then reigning theories, representing as he did the view of the majority of his fellow botanists."

The paragraph which follows must, we think, in fairness be quoted, though this is done with some regret:—

"However, these remarks relate only to two famous writers on the subjects with which this history is concerned. If the work had been brought to a close with the year 1850 instead of 1860, I should hardly have found it necessary to give them so prominent a position in it. Their names are Charles Darwin and Karl Nägeli. I would desire that whoever reads what I have written on Charles Darwin in the present work should consider that it contains a large infusion of youthful enthusiasm still remaining from the year 1859, when the 'Origin of Species' delivered us from the unlucky dogma of constancy. Darwin's later writings have not inspired me with the like feeling. So it has been with regard to Nägeli. He, like Hugo von Mohl, was one of the first among German botanists who introduced into the study that strict method of thought which had long prevailed in physics, chemistry, and astronomy; but the researches of the last ten or twelve years have unfortunately shown that Nägeli's method has been applied to facts which, as facts, were inaccurately observed. Darwin collected innumerable facts from the literature in support of an idea; Nägeli applied his strict logic to observations which were in part untrustworthy. The services which each of these men rendered to the science are still acknowledged; but my estimate of their importance for its advance would differ materially at the present moment from that contained in my 'History of Botany.' At the same time, I rejoice in being able to say that I may sometimes have overrated the merits of distinguished men, but have never knowingly underestimated them."

We are sorry that these words have been written. The position of Darwin in biology needs no defence, even when the assailant is Prof. Sachs. With regard to Nägeli, the case is different: but, although recent investigation has re-opened some of the questions which he appeared to have decided, we feel that here also the critic's first thoughts were best, and that the estimate of 1875 is, in the main, more just as well as more generous than that of 1889.

A very brief sketch of the contents of the work, which will already be familiar to so many botanical readers, must suffice. The first of the three books into which the whole work is divided is occupied with the history of morphology and classification, from 1530 to 1860. The early efforts at classification by the German and Dutch botanists of the sixteenth century are first discussed, and it is shown that they were already guided by the perception of natural affinity—an idea which, as the author says, "is not the discovery of any single botanist, but is a product, and to some extent an incidental product, of the practice of describing plants." But for a time these necessarily feeble attempts at a natural arrangement had to give way to artificial systems based on *a priori* principles of classification. Of this tendency Cesalpino is the first great representative, and the author shows how great was the influence of this remarkable man on the succeeding period of systematic botany. It was Cesalpino who first founded a classification mainly on the organs of fructification.

The period inaugurated by Cesalpino culminates in Linnæus.

"Linnæus," says Prof. Sachs, "in whose works the profound impression which he had received from Cesalpino is everywhere to be traced, retained all that was important in his predecessor's views, but perceived at the same time what no one before him had perceived, that the method pursued by Cesalpino, Morison, Ray, Tournefort, and Bachmann, could never do justice to those natural affinities which it was their object to discover; and that in this way only an artificial though very serviceable arrangement could be attained, while the exhibition of natural affinities must be sought by other means" (p. 81).

The author does full justice to the unrivalled excellence of Linnæus as a descriptive botanist, and further points out that his fragment of a natural system was much the most truly natural proposed up to the middle of the eighteenth century. Linnæus's famous sentence, "It is not the characters which make the genus, but the genus which makes the characters," shows, indeed, a remarkable insight into the meaning of natural affinity.

The development of the natural system by the two Jussieus, Pyrame de Candolle, Robert Brown, and other illustrious systematists is next traced. In the concluding chapter of the first book there is a fine sketch of the splendid work of Hofmeister in establishing the relations between Cryptogams and Phanerogams, and of his position relative to the theory of descent. The author says (p. 202):—

"When Darwin's theory was given to the world eight years after Hofmeister's investigations, the relations of affinity between the great divisions of the vegetable kingdom were so well established and so patent, that the theory of descent had only to accept what genetic morphology had actually brought to view."

The subject of the second book is the history of vegetable anatomy. An admirable account is given of the work of the great founders of the anatomy of plants in the seventeenth century, Malpighi and Grew, who remained the leading authorities in this branch of science

for 130 years. In speaking of the period of barrenness which followed this brilliant beginning, the author is especially severe on our own country. "In England," he says (p. 246), "the new light was extinguished with Hooke and Grew, and has so remained, we may almost say, to the present day." We may hope that, if this passage had been written fifteen years later, Prof. Sachs would have found some reason to modify his judgment.

The following chapters deal with the revival of vegetable anatomy and histology in the present century. Due justice is done alike to the patient investigations of von Mohl and to the brilliant method of the erratic Schleiden, while, as will be gathered from what has been said above, the many-sided activity of Nägeli receives in the text fully adequate recognition.

The third book is on the history of vegetable physiology, and this is of special interest from the fact that the author is himself the leading physiological botanist of our time. The first chapter is concerned with the history of the sexual theory. The chief credit for the discovery of sexuality in plants is given to Camerarius of Tübingen, who in 1694 published the first experimental researches on the necessity of fertilization for the ripening of the seed; though in special cases, as those of the fig and the date-palm, the fact of sexuality had been known even to Theophrastus and Pliny. The author justly points out that Linnæus, though his system called general attention to the existence of male and female organs in the flower, had little or nothing to do with the discovery of their functions.

The following passage discusses the relation of Kaspar Friedrich Wolff to the old theory of "evolution," according to which all the parts of the mature organism pre-exist in little in the embryo.

"Wolff conceived of the act of fertilization as simply another form of nutrition. Relying on the observation, which is only partly true, that starved plants are the first to bloom, he regards the formation of flowers generally as the expression of feeble nutrition (*vegetatio languescens*). On the other hand, the formation of fruit in the flower was due to the fact that the pistil found more perfect nourishment in the pollen. In this, Wolff was going back to an idea which had received some support from Aristotle, and is the most barren that can be imagined, for it appears to be utterly incapable of giving any explanation of the phenomena connected with sexuality, and especially of accounting for the results of hybridization. Wolff may have rejected the theory of evolution on such grounds as these, but he failed to perceive what it is which is essential and peculiar in the sexual act" (p. 405).

This passage appears of special importance, for theories akin to those of Wolff have reappeared even in our own day.

The investigations of Koelreuter on hybridization, and those of Sprengel on cross-fertilization, the full significance of which was first shown by Darwin 60 or 70 years later, mark the closing years of the eighteenth century. But, in spite of all that had been done, there were still some botanists who, on more or less feeble grounds, expressed doubts as to the sexuality even of the Phanerogams, and it was the work of Gärtner, towards the middle of the present century, which "once more confirmed the existence of sexuality in plants, and in such a manner that it could never again be disputed."

The concluding sections of this chapter give the remarkable history of the discovery of the details of fertilization in the flowering plants, and sketch the rise and progress of our knowledge of corresponding processes among the Cryptogams. These are subjects on which an immense amount of good work has been done in more recent years, and some future historian will have much to add to Prof. Sachs's brilliant summary.

The nutrition of plants forms the subject of the next chapter of the "History." The ideas of the ancients are first considered, and then the gradual rise of the modern doctrine of assimilation is traced from its first beginnings in the discoveries of Malpighi and Hales, of whom the former showed that the green leaves are the organs which prepare the food, while Hales proved that a large part of this food is taken up in a gaseous form. It would be useless to attempt to summarize this interesting story. Probably no piece of scientific history has ever been better told, and few, if any, are better worth the telling. Prof. Sachs is here, above all, on his own ground, and we are conscious that we are reading the words of a great master. It is scarcely necessary to add that here, also, more recent research has been extremely active, and modern investigations on such questions as the source of the nitrogen in plants, and the course of the ascending sap, will probably do much to modify the views expressed in this work.

The concluding chapter is on the movements of plants, and here, once more, the historian is treating of phenomena of which he is himself among the greatest investigators.

The translators and the Clarendon Press deserve the warmest thanks of English readers, whether botanical or not, for bringing before them a scientific history distinguished at once by its clearness, its fairness, and the author's unrivalled mastery of his subject.

D. H. S.

A TEXT-BOOK OF PHYSIOLOGICAL AND PATHOLOGICAL CHEMISTRY.

Text-book of Physiological and Pathological Chemistry, in Twenty-one Lectures for Physicians and Students. By Dr. G. Bunge, Professor of Physiological Chemistry at Bâle. Translated from the second German edition by the late L. C. Wooldridge, M.D., and completed for the press by his Wife. (London: Kegan Paul, 1890.)

THE appearance of Bunge's text-book in its English dress reminds us keenly of the loss which physiology has sustained by the death of the translator. It is some consolation to be able to temper this regret with the satisfaction that so interesting and instructive a work was made available to English students by one so capable as the late Dr. Wooldridge. He wisely contented himself with translating the original without those annotations or additions which are often supplied, and which, while they may be of intrinsic merit, frequently destroy the individuality of the original. Criticism of Dr. Wooldridge's share in the English version thus resolves itself into asking how he has done his work as a translator, and the answer is: "Admirably." While

the original text is closely followed and accurately rendered, the result is, unlike some translations, such pleasant reading that the student will scarcely realize that it is a translation. But in justice to the author it must be said that this is also partly due to the simple style and language of the original, and to the lecture-form of its arrangement.

The aim of the author has been to deal with such portions of the subject as are "ripe for a connected account," omitting "all disconnected facts and mere descriptive matter" and all descriptions of analytical methods; to provide such references to the literature of the subject as shall more particularly suffice to put the student on the track of the remainder; and thus as a whole to tell the reader what is most certainly known, and to enable him to pursue further any points in which he is specially interested. In all this the author has been very successful, and particularly with respect to the references to original memoirs, which are quoted judiciously and comprehensively. The work is divided into twenty-one lectures. Of these the first propounds the author's views as to the "aims and prospects of modern physiological research," and consists of a somewhat remarkable protest against the modern tendency to regard cell-activity as the expression and outcome of chemical, physical, and mechanical processes. It is indeed a distinct return to the vitalistic views of the past, and urges the existence of some psychological factor of activity, based on the belief that "for the moment it is not apparent how any further progress of importance can be made with the help of chemistry, physics, and anatomy only;" and concludes by saying that "what these sciences fail to achieve will stand out more prominently, and thus the mechanical theories of the present will assuredly carry us eventually to the vitalism of the future." There are probably few physiologists who will agree with this view. Most will rather hold with Heidenhain (*Pflüger's Archiv*, xliii., Suppl.-Hft. p. 63) that, granted the existence of the psychological factor, still it must produce its recognizable effects by purely chemical, physical, and mechanical means, and accept those views of the "activity" of a cell which stand out so clearly in his masterly work on secretion.

The second and third lectures treat of the chemical elements which constitute living organisms, their circulation through the vegetable and animal kingdoms, the principle of the conservation of energy as applied to living things, and, finally, the correlation of plants and animals. The next three lectures deal with the organic food-stuffs and foods, their composition, importance, and function, in connection with nutrition. In these a clear and comprehensive account is given of the various endeavours which have been made to determine the molecular weight of proteids. The sections on the *rôle* of gelatin and cellulose, and of a vegetarian diet in general, are most instructive, and there is a very full statement of the physiology of the organic compounds of iron, leading up to the author's views as to the mode of action of iron-salts in the treatment of chlorosis. Lecture VII., on the inorganic food-stuffs, contains an interesting and valuable account of various salts, more particularly those of sodium and potassium, in their relationship to nutrition; and Lecture VIII. concludes this part of the subject by treating of subsidiary articles of diet, such as tea, coffee, alcohol, bouillon, &c. Digestion and the absorption of digestive products form the subject-matter of Lectures IX.-XII. In these the well-selected and copious references to the literature of the subject will be found to be by no means the least valuable part. Lecture XIII., on the chemistry of blood and lymph, will probably disappoint those who turn to it for an account of the clotting of blood. Perhaps Prof. Bunge thinks the subject not yet "ripe for a connected account," and this is, perhaps, to a large extent true. Still, he would have done well to treat it from a general point of view, rather than almost entirely with regard to the part played by the leucocytes: some account at least of the work of Hammarsten and the translator seems called for in connection with this part of the subject. The gases of the blood, and their relation to the processes of external and internal respiration, are dealt with in the next two lectures. These call for no special remark apart from saying that the fact that the oxidations of the body take place in the tissues might have been more decisively brought out. Existing views as to the condition of CO_2 in the blood are clearly stated. Lecture XVI. gives an admirable exposition of recent work and existing views as to the seat and mode of formation of the nitrogenous products of metabolism, followed, in natural sequence, by a chapter on the functions of the kidneys and chemistry of urine. Hepatic metabolism is the subject of Lecture XVIII. In this the questions which arise with regard to its glycogenic activity are scarcely so clearly put forward as might be expected. On the other hand, the older and current views on fat-formation are well explained in Lecture XX. The remaining lectures (XIX. and XXI.) deal with the source of muscular energy and diabetes respectively.

It is well for those English students who cannot read the original that this interesting and instructive work by Prof. Bunge has, in this well-turned version, been made accessible to them. We cannot conclude better than by hoping it may attain the recognition and approval in this country which it so fully deserves from every point of view, and which it appears to have already secured in the original, judging by the speedy issue of the second edition, of which the copy here reviewed is a translation.

THE ADVANCEMENT OF SCIENCE.

The Advancement of Science: Occasional Essays and Addresses. By E. Ray Lankester, M.A., LL.D., F.R.S. (London: Macmillan and Co., 1890.)

UNDER this title, Prof. Ray Lankester has republished a number of essays, which have appeared at intervals during the last eighteen years. All of them are of more or less permanent interest, and we are glad to have them presented to us in the convenient form of a well-printed octavo volume. While some of the essays are somewhat too technical for the general reader, the majority are of great and very general interest, well worthy of being read and thought over by all.

These essays are nine in number. The last treats of the history and scope of zoology, and is reprinted from the last edition of the "Encyclopædia Britannica"; it forms a most excellent treatise on the subject, and fairly though briefly sketches the history of zoology from the

seventeenth century to the present day. In the second essay the relations that should exist between the State and biology are considered, and there can be little doubt but that as a result of this address to the Biological Section of the British Association at Southport, followed by the fifth essay, which gives an outline of the scientific results of the International Fisheries Exhibition, held in London in the same year (1883), we are in great measure indebted for the valuable help given by our Government towards the establishment of the Laboratory at Plymouth belonging to the Marine Biological Association of the United Kingdom.

The third and sixth essays, on Pasteur and hydrophobia—or rabies, as we would prefer to call this formidable disease—and on centenarism, are full of interest, and while in the former the author has to content himself with a narration of the chief results of Pasteur's invaluable labours, in the latter we find an account of a subject which has been critically worked out by himself.

Three of the essays relate to the subject of Darwinism, and possibly will be found the most interesting in the volume. The first is on the subject of "Degeneration, a chapter in Darwinism," and was delivered as one of the evening lectures at the British Association meeting at Sheffield, in 1879. In it Prof. Lankester calls attention to the fact that degeneration, or the simplification of the general structure of an animal, may be due to the ancestors of that animal having taken to one of two habits of life, either the parasitic or the immobile Other new habits of life appear also to be such as to lead to degeneration. Let us suppose, for example, a race of animals fitted and accustomed to catch their food, and having a variety of organs to help them in this chase; suppose such animals suddenly to acquire the power of feeding on the carbonic acid dissolved in the water around them, just as green plants have. This would lead to degeneration; for they would soon cease to hunt their food, and would bask in the sunlight, taking food in by the whole surface, as plants do by their leaves. Another possible cause of degeneration appears to be the indirect one of minute size. And so, as is well shown, this hypothesis of degeneration enables very numerous cases of animal structure to be accounted for. The second of this set, forming the seventh of the collected series, is on parthenogenesis, and in it we find the fascinating accounts given to us by the painstaking zeal of von Siebold of the habits and manners of the little wasps belonging to the genus Polistes—a story both wonderful and romantic. The third of these, the eighth of the whole set, treats of Haeckel's theory of heredity, in which the transmission of acquired characters by heredity is discussed, but this phase of belief Prof. Lankester will no longer insist upon, and he points out that Weismann's essays on this question should be carefully studied by naturalists.

The last essay to be alluded to is the fourth, on examinations. The author claims that but few have had a wider or a more continuous experience in examinations than he has had. On this somewhat vexed question he has a good deal that is to the point to say, showing that the use of examinations in schools and Universities is different from their use as a test of fitness for entrance into a profession, or a post in the Home or Indian Civil Service, or as a means of deciding a question of relative merit.

We feel sure that as each of these essays originated in a desire to promote the interests of science, so the author, in collecting the present series, will be found to have had the same aim in view.

OUR BOOK SHELF.

Agenda du Chimiste. Par MM. Salet, Girard, et Pabst. (Paris: Hachette and Co., 1890.)

In this volume will be found a most complete and exhaustive compilation of facts and numerical tables of use to the chemist. The first edition was published in 1877 by M. Wurtz, and in subsequent editions the work has been thoroughly brought up to date. It is now published annually as a chemical year-book, the publication of each year containing a few special articles called for by the events of the past twelve months. This year the following are among the special articles contributed: "The Progress of the Industry of Colouring Matters," "Review of the Exhibition of 1889" as regards matters of chemical interest, and "Views of the International Chemical Congress concerning Nomenclature." The numerical data included in the book are most full, and ought to be of great service in the reduction of observations. The collection of them represents an immense amount of labour, and the accompanying descriptions of experimental methods are very clear and concise. A most useful portion of the work is that in which all the known physical constants of the elements and numerous compounds are given. Special care appears to have been taken in collecting the published thermo-chemical data, with the result that the chapter upon this subject is one of the most valuable in the book. The tables for use in quantitative analysis, and especially those referring to commercial methods, will doubtless be fully appreciated for the saving of time and arithmetical labour which their use will effect. It is, moreover, of no mean advantage that all formulæ are given according to the ordinary nomenclature, and not according to the old notation still retained by many French chemists. The volume is small and handy in spite of its five hundred pages, and cannot fail to be of service in the laboratory. A. E. T.

The Philosophy of Clothing. By W. Mattieu Williams. (London: Thomas Laurie, 1890.)

Mr. Williams is a somewhat eccentric writer, and by most people some of the notions set forth in this little book will be regarded as "fads." He is generally able, however, to give a good reason for the opinions he advances, and much of his advice, although opposed to the rules of fashion, is sound and practicable. The subject is one which occupied the close attention of Count Rumford; and of his researches Mr. Williams, as he himself says, has made "free use."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

The Zoological Affinities of *Heliopora cerulea*, Bl.

The remarkable blue coral, *Heliopora cerulea*, of Blainville, represents, I believe, one of those species that, in common with *Stylaster*, *Millepora*, and other allied genera, have been recently relegated to the Hydrozoic subdivision of the Cœlenterata. So far as I remember, however, and without having present access

to the most recent literature of this subject, *Heliopora* was thus transferred with reference to the structure of its corallum only, the living animal having been but imperfectly if at all observed.

In the course of my professional investigations of the fisheries of Torres Straits I have on several occasions obtained specimens of *Heliopora*, but had hitherto been unsuccessful in observing the living animal. Last year I obtained this coral on the Warrior Reef near New Guinea, but while apparently living when collected, and kept for days on board ship with the water continually changed, the zooids refused to make their appearance. Through the courtesy of Captain Dawson, R.N., and the officers of H.M.S. *Rambler*, I have this season journeyed north in that ship, and was afforded the opportunity of conducting a series of investigations in the neighbourhood of the Adolphus Islands, off Cape York, close to the scene of the recent *Quetta* wreck, and with relation to which the *Rambler* had been told off to make a careful survey.

At low spring-tide on the reef adjacent to the "Mid-Brother" rock, I came across a luxuriant growth of *Heliopora*, and was fortunate on this occasion to accurately determine the nature of the fabricators of this remarkable coral. The first living manifestations presented, and those visible only with the aid of a pocket lens, were the protrusion of a transparent body and two elongate tentacles from the numerous circular pores with which the corallum is studded. At first sight some near affinity of the animal to the bitentaculate Hydrozoon *Lar sabellarum* of Gosse was suspected. The movements of the zooids during extension and retraction were, however, more active than those which usually obtain among the Cœlenterata, and together with their general aspect and comportment suggested a nearer relation to the Annelida. This last-named section of the Invertebrata was found on a closer examination to represent their actual position in the zoological scale. On splitting one of the smaller flattened branches of the coral perpendicularly and parallel with its wider axis, I found that the entire coronid system was exposed to view. The little annelid fabricators, having an average length of one-fifteenth of an inch, wriggled into the water in every direction, a large number at the same time remaining passively in the tubular chambers which they originally constructed.

The most prominent external characters of the annelid of *Heliopora cerulea* consist of the bitentaculate head and six pairs of lappet-like branchiæ, which originate in segmental pairs on the dorsal surface and commence about the sixth segment posteriorly from the head. Fine isolated or paired setæ are developed in duplicate on the majority of the residual segments, and two brush-like fasciculi of closely adpressed setæ are conspicuous on the dorsal aspect of the penultimate and antipenultimate caudal segments. On my return to Brisbane a few weeks hence, I purpose preparing and remitting a more detailed account, with illustrations, of the organization of *Heliopora*. In the interim it has occurred to me that this brief announcement of its nature may prove of interest to many of your readers, more especially as it may assist in throwing fuller light on the affinities of the many fossil genera that have hitherto been affiliated with this type among the Cœlenterata, but which in common with *Heliopora* should probably find their true position among the more highly organized section of the Tubicolous Annelida.

W. SAVILLE-KENT,
Commissioner of Fisheries, Queensland.
Thursday Island, Torres Straits, June 18.

Chambers's "Hand-book of Astronomy."

As the writer of the article on "Spectroscopic Astronomy" in the above work, I should like to be permitted to comment upon two points wherein your reviewer has, though doubtless inadvertently, scarcely done me justice.

On p. 292 (NATURE of July 24) the reviewer says that I have "*selected* certain determinations and arranged them in parallel columns to demonstrate the efficiency of the method adopted." The reference is to the comparison which I gave of the results obtained by Dr. Huggins, Mr. Seabroke, and at Greenwich, for motions of stars in the line of sight. But I made no selection. The list I gave contains *all* the stars that had been observed at two or more Observatories, and the mean of *all* the observations of each star. I might further add that I think your reviewer is scarcely fair in his description of the discordances of my observations: expressed in wave-length, the average difference from the mean is but a small fraction of a tenth metre. But this is an unimportant matter compared with the suggestion that I have published a "selected"—that is, a "cooked"—comparison.

Then your reviewer complains that I make no reference to Prof. Vogel's observations of Algol, whilst I give my own "later division" of my observations into groups. I made no reference to Prof. Vogel's observations, because they were not published until some considerable time after the final revise of my article had been passed for press; whilst, so far from my division of my observations into groups being later than Prof. Vogel's work, it was two full years earlier, having been communicated to the Royal Astronomical Society in January, 1888, by the Astronomer-Royal (see *The Observatory*, vol. xi. p. 109). I also gave my results in one of the Gresham Lectures, Easter, 1888.

E. W. MAUNDER.
Royal Observatory, Greenwich, S.E., August 1.

I REGRET that my words allowed the interpretation which Mr. Maunder points out, for I had no intention of insinuating that the comparisons were "cooked." What I take exception to is that, according to the values given, γ Cassiopeiæ has a motion in the line of sight of -12, although on February 19, 1887, Mr. Maunder determined it as $-54{\cdot}2$, and eight minutes afterwards as $+60{\cdot}9$; and again, β Pegasi is stated to have a motion in the line of sight of -8, although in November 1881 two determinations, made within ten minutes of each other, differed by nearly 114 miles per second. It would seem, therefore, that in making a tabular statement, even of the mean of such values found by different observers, the magnitude of the probable error should be mentioned; for, as I remarked at the time, "To one unacquainted with instrumental difficulties, the motion of stars in the line of sight would appear to be a quantity that may be determined with some accuracy," whereas this is not the case. I have no intention of questioning Mr. Maunder's skill as an observer, but the fact that the discordances, when expressed in wave-lengths, are very small, only supports my contention that, until more perfect instrumental conditions are possible, many of the values are useless, and their determination an affectation of accuracy.

Mr. Maunder has himself to blame for my want of information with respect to Algol. He gives no reference to the report of the remarks made by the Astronomer-Royal in January 1888, and his own comments, at the meeting of December 1889, upon Prof. Vogel's work, led me to suppose nothing had been done previously.

THE REVIEWER.

Gregory's Series.

GREGORY'S series, on which are founded nearly all the methods of obtaining the approximate value of π, is made to depend, in works on trigonometry, on De Moivre's theorem and results flowing from it.

The following does not require the use of $\sqrt{-1}$, but depends only on two things—that the circular measure of an angle and its tangent are practically equal when the angle is indefinitely small, and that $\tan(A-B) = \dfrac{\tan A - \tan B}{1 + \tan A \,.\, \tan B}$.

Let

$$\tan\theta \equiv \tan(a_0 + a_1x + a_2x^2 + \&c.) = x;$$

$$\therefore \tan\{a_0 + a_1(x+h) + a_2(x+h)^2 + \&c.\} = x + h;$$

$$\therefore \tan h \,.\, \{a_1 + 2a_2x + 3a_3x^2 + \&c. + \text{terms involving } h, \text{ say H}\} = \frac{h}{1 + x(x+h)};$$

$$\therefore \frac{\tan h\{a_1 + 2a_2x + 3a_3x^2 + \&c. + \mathrm{H}\}}{h(a_1 + 2a_2x + 3a_3x^2 + \&c. + \mathrm{H})} = \frac{1}{\{1 + x(x+h)\} \,.\, (a_1 + 2a_2x + 3a_3x^2 + \&c. + \mathrm{H})}.$$

Let $h = 0$;

$$\therefore 1 = \frac{1}{(1+x^2) \,.\, (a_1 + 2a_2x + 3a_3x^2 + \&c.)}.$$

Equating coefficients of like powers of x,

$$a_1 = 1,\ a_2 = 0,\ a_3 = -\tfrac{1}{3},\ \&c.;$$

$$\therefore \theta = a_0 + x - \tfrac{1}{3}x^3 + \&c.,$$

where evidently $a_0 = 0$, or a multiple of π.

Taking $\theta = \dfrac{\pi}{4}$,

$$\frac{\pi}{4} = 1 - \tfrac{1}{3} + \tfrac{1}{5} - \tfrac{1}{7} + \&c.$$

R. CHARTRES.

The Perseid Meteor Shower.

WITH reference to the letter of Mr. Monck in NATURE of July 24 (p. 296), I would remark that his attempted explanation of the displacement in the Perseid radiant point is altogether futile. If your correspondent were better acquainted with the facts in detail, I think he would readily admit this.

My observations in this branch have been effected in the hope that they might prove useful, and I am sorry to see that Mr. Monck has so thoroughly misapprehended them. The shifting radiant of the Perseids is fully proved, and anyone who will take the trouble to watch the sky at the proper season may readily observe the fact for himself. W. F. DENNING.

Bristol, August 2, 1890.

COMPARISON OF THE SPECTRA OF NEBULÆ AND STARS OF GROUPS I. AND II. WITH THOSE OF COMETS AND AURORÆ.

I.

THE first step towards my present views as to the evolutions of the various groups of cosmical bodies was taken when one day I was attempting to trace the origin of the absorption flutings in stars of Vogel's Class III.*a*. So far, no one had endeavoured to trace their origin, all the work having been confined to the absorption *lines*. It is true that both Dr. Huggins and Vogel, as well as others, had published maps of the spectra of these stars, showing the absorption flutings as well as the lines, but the origins of the former were not inquired into.

It was at once perfectly obvious that among the chief absorption flutings were the most prominent of those seen in the spectrum of manganese at the temperature of the oxy-coal-gas flame—a temperature at which only one *line* is visible, while in the sun all the lines of manganese are visible. In order to investigate this further all the flutings seen when the principal metals were exposed to this temperature were mapped, with a view of determining whether any others besides those of manganese were visible in the stellar spectra. Several others, notably one of lead, were found to be present.

Here, then, was proof positive of low temperature; from solar absorption to the absorption of these stars of Class III.*a* we passed from phenomena which we can reproduce at the temperature of the arc to those visible at the temperature of the oxy-coal-gas flame.

It was next found that identical absorption phenomena are seen in comets long before they reach perihelion. This was a striking result, considering the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and bearing in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

But another revelation still more startling was in store for me, when my assistants and myself had exhausted all the flutings then known to us as origins for the so-called dark bands which remained, and found that none would fit, and we seemed at the end of our tether.

My ten years' work on carbon made itself quite unconsciously felt at this juncture. It suddenly flashed upon me that the 517·2, 516·7, 516·6, 516·7, 517·1, &c., recorded by Dunér in his observation of α Orionis as the edge of a dark band, *could be nothing but the edge of the brightest band of carbon*, the bright cometary band *par excellence*, and therefore that these so-called stars not only resemble comets in their absorption flutings, as we now learn, but in their radiation flutings as well; in short, these stars *were* comets, with the difference—a trifling one from my then point of view—that they were not moving round our sun.

This surmise has since been abundantly confirmed. The dark band of Dunér is a *contrast band*—the spectrum looks dark there on account of the extreme brilliancy of the carbon fluting. The other carbon flutings were next sought for and easily found.

These "stars," then, instead of being like our sun, consisted of swarms of meteorites. We have in these bodies a spectrum integrating the *radiation* of carbon and the *absorption* of manganese and lead vapour, as in the case of some comets.

The law of parsimony compels us to ascribe the bright fluting of carbon in these "stars" to the same cause as that at work in comets, where we know it is produced by the vapours between the individual meteorites or repelled from them. Hence we are led to conclude that the absorption phenomena are produced by incandescent vapours surrounding individual meteorites which have been rendered intensely hot by collisions, while the carbon light comes from the interspaces.

I propose in the present paper to give a summary of the evidence of cometary kinship, so to speak, among the other cosmical bodies; and I shall follow this by an historical statement showing how previous observers have suspected the presence of carbon in "stars."

First as to cometary kinship.

The discussion of cometary spectra which I communicated to the Royal Society in November 1888 (Roy. Soc. Proc., vol. xlv. pp. 159–217), contained, among other matters, conclusions which have a special bearing on the relations of their spectra to those of other bodies.

It is obviously desirable to compare this material with the more complete lists of lines which I have now obtained from a very thorough search after all the observations hitherto made of other groups of celestial bodies, since such a comparison—a much more complete one than was possible in the first instance—would strengthen or weaken my hypothesis according as the increased area of observation increased or decreased the number of coincidences in the spectra of the various groups.

The more the coincidences are intensified the greater is the probability that comets, nebulæ, stars with bright lines, stars with mixed flutings, and the aurora have a common origin, independent of the chemical origins which have been assigned to the various lines by laboratory observations.

In the tables which follow, the individual observations are not given, but under each heading all the lines or flutings which have been recorded find place.

I. *Comparison of Comets and Nebulæ.*

We may conveniently begin with a comparison of comets and nebulæ. The Great Comet of 1882 and Comet Wells, when near perihelion, are excluded from the list of cometary lines and flutings, as their temperature was too high for fair comparison with most of the nebulæ and other low temperature phenomena.

In cases where any of these higher temperature lines correspond to lines in the comparison spectrum, however, they have been added to the list of cometary lines, in

brackets, as sometimes the phenomena compared may attain a temperature slightly higher than that of comets at mean temperature.

For the nebulæ, all the lines recorded in the visible spectrum by Messrs. Huggins, Vogel, Copeland, Fowler, and Taylor, are given. The list of lines has been considerably extended since my preliminary discussion of the spectra of nebulæ in November 1887. D_3 and a line at 447 have been observed in the spectrum of the nebula in Orion by Copeland, and Mr. Taylor has also recorded D_3 and lines, or remnants of flutings, at 559 and 520. In the nebula in Andromeda, carbon flutings and the lead flutings at 546 have been observed by Mr. Fowler and confirmed by Mr. Taylor; since these observations were made, I find that Vogel (*Bothkamp, Beob.*, Heft 1, 1872, p. 57) observed a line at 518, probably carbon 517, in nebulæ numbered in Sir J. Herschel's General Catalogue 4234, 4373, and 4390.

Other nebula lines with which I was not previously acquainted are 479, 509, and 554. All these lines were observed by Vogel in the nebula G.C. 4378 (*Bothkamp, Beob.*, Heft 1, 1872, p. 57).

With reference to the appearance of D_3 in nebulæ and bright-line stars, I wrote, in November 1887 (Roy. Soc. Proc., vol. xliii. p. 139):—"It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while, on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

"I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far has only been seen in the very hottest part of the sun which we can get at. It is there associated with *b*, and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars where the absence of iron lines and of *b* shows that the temperature is very low. Further, no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent, and once as the edge of a fluting.[1]

"It is even possible that the line in question merely occupies the position of D_3 by reason of the displacement of D by motion of the 'stars' in the line of sight. On this point no information is at hand regarding any reference spectrum employed.

"*If, however, it should eventually be established that the line is really D_3, which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the sun, is brought about in the spaces between meteorites by extreme tenuity.*"

The observations of Dr. Copeland (*Monthly Notices R.A.S.*, vol. xlviii. p. 360), have now, I think, established the identity of the yellow line, in the nebula of Orion at all events, with D_3. In a letter to Dr. Copeland, I suggested that the line at 447 was in all probability Lorenzoni's *f* of the chromosphere spectrum, seeing that it was associated both in the nebulæ and chromosphere with hydrogen and D_3. This he believes to be very probable. The line makes its appearance in the chromosphere spectrum about 75 times to 100 appearances of D_3 or the lines of hydrogen.

The association of the line at 447 with D_3 therefore strengthens the view that there is an action in space, away from condensations, whereby matter is reduced to its finest forms.

[1] ". . . . The spectrum is very bright; two strong bands are seen in the red, then the D line, followed by a bright line (D_3) as the edge of a band. . . . ." (Konkoly, "Neuer Stern bei χ Orionis," *Astr. Nachr.*, No. 2712).

With regard, then, to the comparison of the spectra of comets and nebulæ the case stands as follows:—

Comets.	Nebulæ.	Probable Origins.	λ of Probable Origins.
—	411	H	4101
431	—	CH	431
—	434	H	434
—	447	?	—
468–474	468–474	C (hot)	468–474
—	479	?	—
483	—	C (cool)	483
486	486·3	H	486
—	495	?	—
500	500	Mg	500
—	509	?	—
517	517	C (hot)	517
519	—	C (cool)	519
521	520	Mg	521
[527]	527	Fe	527
546	546	Pb	546
—	554	?	—
558	559	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	5872	? (D_3)	—

The table shows that there are many striking similarities between the two spectra, and there is no doubt that many of the lines are identical. The flutings of hot carbon, for example, are common to both, as are also the flutings of magnesium, manganese, and lead. The hydrogen line 486 has only been seen in one comet, namely, Comet III. 1880, by Konkoly ("O'Gyalla Observations," 1881, p. 5.)

Other flutings and lines again are special to comets and others to nebulæ. Thus, there are practically no indications of hydrogen in comets, although the hydrogen lines are amongst the brightest in nebulæ. Again, the lines 447, 479, 495, 509, and 554 are seen in nebulæ, but not in comets. On the other hand, the cool carbon flutings and the fluting at 568 are seen in comets, but not in nebulæ. Most of these apparent discrepancies are explained by a consideration of the differences in the conditions of comets and nebulæ. It must be remembered that in the case of comets there is an action which repels the vapours produced by collisions, and the vapours first affected will, of course, be those which are least dense. Hydrogen will thus be repelled from the comets, whilst the denser vapours of magnesium and carbon remain. There is then a good reason why hydrogen lines should *not* be seen in cometary spectra. As there can be no such repulsion in the sparse swarms which constitute nebulæ, hydrogen lines are seen in them.

Two other lines special to nebulæ are 5872 and 447, to which reference has already been made. The evidence tends to show that D_3 and *f* are finer vapours than hydrogen, and hence there is even greater reason for the absence of these lines from cometary spectra, even were the temperature higher, than for the absence of the lines of hydrogen.

The line at 527 is probably the iron line E; this was seen in the hotter comets, namely, Comet Wells and the Great Comet of 1882, so that there is no discordance with regard to the appearance of this line. The other lines special to nebulæ are 479, 495, 509, and 554; but as no origins for these have yet been determined, it is not possible to explain their absence from cometary spectra. It is not improbable that 554 is an error in measurement for the manganese fluting at 558, the latter having been recorded by Mr. Taylor in the nebula of Orion.

The apparent absence of the cool carbon flutings from nebulæ is in all probability due to insufficient observations, as indicated by the discussion of comets. The lowest

temperature (magnesium) and the hot carbon stages of comets are both represented in nebulæ, and the intermediate cool carbon stage is therefore not likely to be entirely absent.

The absence of the hot carbon fluting at 564 from the spectra of nebulæ may possiby be due to two causes. It is much fainter than either 517 or 468–474, and may have escaped notice on that account; or, as in the nebula in Andromeda, it may be masked in the same way as in comets.

It is suggested that the ordinary nebulæ are not hot enough to give the line or fluting at 568, but it appears when the swarms become more condensed—that is, in bright-line stars. The absence of 568 is therefore probably due to the low temperature of nebulæ.

II. *Comparison of Comets and Auroræ.*

If we exclude the exceptional cases of Comet Wells and the Great Comet of 1882, the number of lines and flutings recorded in comets is small, and therefore only the most general list of auroral lines must be taken for comparison. It would be unfair, for example, to take the long list of lines given by Gyllenskiöld. The lines stated are taken from the table which I gave in a note in January 1888 (Roy. Soc. Proc., vol. xliii. p. 321) which has since been slightly rearranged before taking the means.

Comets.	Auroræ.	Probable Origins.	λ of Probable Origins.
—	411	H	4101
[426]	426	?	?
431	431	CH	431
—	435	H	434
468–474	474–478	C (hot)	468–474
483	482	C (cool)	483
486	486	H	486
500	500	Mg	5006
517	517	C (hot)	517
519	519	C (cool)	519
521	522	Mg	521
—	531	?	—
—	535	Tl	535
—	539	Mn	540
546	545	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	606	?	—
[615]	620	Fe	615
—	630	?	—

Here, again, it will be seen, that there are many striking coincidences. The hydrocarbon fluting at 431 and the hot and cool carbon flutings at 468–474, 483, 517, and 519 are common to both. The flutings of magnesium 500 and 521 and the flutings of lead and manganese at 546 and 558 are also common. The iron fluting at 615 is not seen in comets at ordinary temperatures, but since it was recorded in the Great Comet of 1882, it has been added, in brackets, to the list of cometary flutings. The line at 426, which was seen in Comet Wells, has also been added. It will be noted also that there are apparent discrepancies; some lines appearing only in comets and others only in auroræ. The explanation of the absence of hydrogen lines from comets which has already been given applies equally in this case. As there is no repulsion in the aurora similar to that exercised upon comets by the sun, there is no reason for the absence of hydrogen. In the aurora the hydrogen lines may also be produced partly from aqueous vapour. The citron carbon flutings 561 and 564 have not been recorded in the aurora, although they are often seen in comets; their apparent absence from the aurora is probably because they fall in the brightest part of the continuous spectrum, and are consequently masked.

The lines special to auroræ are 531, 535, 539, 606, and 630.

III. *Comparison between Comets and Bright-line Stars.*

In the Bakerian Lecture for 1888 I gave a complete discussion of the spectra of bright-line stars, as far as the observations then went, and the conclusion arrived at was that they are nothing more than swarms of meteorites a little more condensed than those which we know as nebulæ. The main argument in favour of this conclusion was the presence of the bright fluting of carbon which extends from 468 to 474. This, standing out bright beyond their short continuous spectrum, gives rise to an apparent absorption-band in the blue. The varying measurements made by different observers may possibly have thrown a little doubt upon the conclusion that the bright band was due to carbon, but recent observations at Kensington have placed this beyond doubt. Direct comparisons of the spectrum of all the three stars in Cygnus with the flame of a spirit lamp have been made by Mr. Fowler, and these showed an absolute coincidence of the bright band in the stars with the blue band of carbon seen in the flame. It was found quite easy to get the narrow spectrum of the star superposed upon the broader spectrum of the flame, so that both could be observed simultaneously.

Other evidence of carbon flutings was shown by slight rises in Vogel's light-curves near 517 and 564. These, however, could not be as well seen as the band in the blue, because they fall on the bright continuous spectrum from the meteorites. In the stars in Cygnus, Mr. Fowler detected brightenings near 517, and perfect coincidences were found with the fluting at 517 in the spirit-lamp flame. In this case both 517 and 468–474 were simultaneously seen to be coincident with flame-bands.

Measurements were made of the brightenings in the spectrum of γ Cassiopeiæ by Mr. Fowler on September 18, and these were also found to be coincident with the carbon flutings 517 and 468–474; the citron fluting at 564 was not seen. It may be remarked that C, F, and D_3 were seen very bright.

The conclusions drawn from my suggestions as to the presence of carbon, as well as hydrogen, in bright-line stars, are therefore strengthened.

In the following table, all the lines and flutings recorded in bright-line stars, with the exception of γ Cassiopeiæ, are given. The lines recorded by Sherman in γ Cassiopeiæ have not yet been confirmed.

Comets.	Bright-line Stars.	Probable Origins.	λ of Probable Origins.
—	4101	H	4101
431	—	CH	431
—	434	H	434
468–474	468–474	C (hot)	468–474
483	—	C (cool)	483
486	486	H	486
500	—	Mg	500
—	507	? Cd	508
517	517	C (hot)	517
519	—	C (cool)	519
521	—	Mg	521
[527]	527	Fe	527
—	540	Mn	540
546	—	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	564	(C hot)	564
568	568	Pb, Na	568
[579]	579	Fe	579
—	5872	?(D_3)	—
[589 (D)]	589	Na (D)	5889, 5895
—	635	?	—

The coincidences here are between the flutings of hot carbon, manganese 558, and lead or sodium 568. D has only been seen bright in one of the stars (γ Argûs), which is probably one of the hottest; since D was seen bright in two of the hottest comets, I have inserted it in the list of cometary lines and flutings, and [527] and [579] are added for the same reason.

Although nine lines or flutings are common to comets and bright-line stars, six occur in comets which do not appear in bright-line stars, and five in bright-line stars which do not appear in comets.

The apparent absence of hydrogen from comets has already been referred to, as well as the absence of D_3. The cool carbon flutings are not seen in the bright-line stars because the temperature is too high, and the line at 500 is absent for the same reason; 521 is probably also absent because of the higher temperature. The lead fluting at 546 may be masked by continuous spectrum in the bright-line stars; at all events, it appears as an absorption-band when the swarms further condense. Besides the hydrogen and D_3 lines, the lines 507, 540, and 635 appear in bright-line stars, but not in comets.

IV. *Comparison of Comets and Stars of the Mixed Fluting Group.*

In the Bakerian Lecture I also gave evidence to show that stars of Group II. (Vogel's Class III.*a*) are of a cometary character, and a little more condensed than the bright-line stars. The ground on which this conclusion was arrived at was the probable presence of bright carbon flutings, in addition to the metallic absorptions. Observations of α Herculis and Mira Ceti by Mr. Fowler at Kensington and by myself at Westgate-on-Sea have fully confirmed this view. The rapid increase of brilliancy of the flutings of Mira at its maximum in 1888 left little doubt in my mind that they were due to carbon, and Mr. Fowler's comparisons showed perfect coincidences with the carbon flutings, with the dispersion of two prisms of 60°.

Some of the origins which I suggested for the dark bands have also been tested by direct comparisons. Dunér's bands 4 and 5 were found to be coincident with the manganese and lead flutings at 558 and 546 respectively, and band 3 was found to be coincident with the manganese fluting about 586.

Mr. Maunder observed the spectrum of α Orionis on December 16, 1887, and made comparisons with the spectra of carbon, sodium, and manganese, as given by a Bunsen flame. He states the results as follows ("Greenwich Observations," 1887, p. 22):—"The carbon band at 5164 was coincident (within the limits of observation with this dispersion) with the bright space towards the blue of Band VI. (Dunér's band 7), and the sodium lines were clearly represented by two dark lines near the middle of Band II. (Dunér's band 3), but the two manganese bands observed, not only did not coincide with any great band of the spectrum, but were very far distant from any of them. There were, indeed, faint lines about the neighbourhood of either manganese band, but the entire spectrum is full of such lines, and no fluting, nor anything corresponding to one, could be detected near the place of these two bands. A third manganese band was very close to Band II. (Dunér's band 3) of the stellar spectrum." On the other hand, Vogel measured the position of the sharp edge of a fluting in α Orionis as 559·1, and Dunér's measures for the same vary from 557·5 to 559·3, none of which can be described as "very far distant" from the manganese fluting near 558. Mr. Maunder's observation can only be explained by assuming that the band in question is variable. This might be produced by variations in the intensity of the carbon flutings; the manganese fluting falls on the carbon fluting near 564, and, according to their relative intensities, the manganese fluting will be visible or will be masked by the carbon. According to Gore, the star was at a minimum in December 1887.

The fluting near 586 corresponds to Dunér's band 2, for which Dunér measures wave-lengths varying from 585·4 to 586·1. It apparently escaped Mr. Maunder's notice, at the time he made his observations, that no reference was made in my paper of November 1887 to any band in the star spectra which fell near the third fluting of manganese near 535. The first two flutings, near 558 and 586, fell so near to two of the dark bands in the spectra of the stars of Group II. that there was strong ground for believing them to be due to manganese. This has since been abundantly confirmed by Mr. Fowler's direct comparisons of the manganese flutings with the spectra of several stars of the group.

Under the heading of "Dunér's Bands" I give the mean wave-lengths measured by Dunér for the dark bands, and the limits of the bright spaces which are due to carbon.

The figures first given refer to the sharp edges of the flutings; the other figures indicate approximately where the flutings fade away.

This comparison shows that there is a very close relation between comets and Group II. independent of the probable origins suggested. Bright carbon flutings, the manganese fluting at 558, the lead fluting at 546, the iron fluting at 615, and the magnesium fluting 521, are common.

Comets.	Dunér's Bands.		Probable Origins.	λ of Probable Origin.
—	461–451	Bright space	C_B	460–451
—	461–473	(10) Dark space	—	—
468–474	472–476	Bright space	C (hot)	468–474
—	476–486	(9) Dark space	—	—
483	—	—	C (cool)	483
—	495–486	? Bright fluting	?	—
500	495–502	(8) Dark fluting	Mg	500
517	516–502	Bright fluting	C (hot)	517
519	—	—	C (cool)	519
521	516–522	(7) Dark fluting	Mg	521
—	524–527	(6) Dark fluting	Ba (2)	526
546	544–551	(5) Dark fluting	Pb	546
558	559–564	(4) Dark fluting	Mn (1)	558
561	—	—	C (cool)	561
564	—	—	C (hot)	564
—	585–594	(3) Dark fluting	Mn (2)	586
[615]	616–630	(2) Dark fluting	Fe	615
—	647–668	(1) Dark fluting	?	—

The cool carbon flutings are seen in comets, but not in stars of Group II. the reason being that the temperature is too high. The hot carbon fluting at 564 is in all probability present in stars of Group II., but is always masked, in some cases by continuous spectrum, and in others by the absorption fluting of manganese, which is nearly coincident with it.

The line, or probably fluting, at 495 has not yet been recorded in comets, but its association with the fluting at 500 in Nova Cygni indicates that its apparent absence is entirely due to incomplete observations.

The second fluting of manganese, near 586, though one of the most prominent in stars of Group II., has not been observed in cometary spectra, probably because there is not sufficient continuous spectrum from the sparse meteoritic background of the comet to produce the absorption of more than the first fluting of manganese.

Dunér's band 1, 647 to 668, has not yet had an origin assigned to it. J. NORMAN LOCKYER.

(To be continued.)

ON THE STUDY OF EARTHQUAKES IN GREAT BRITAIN.[1]

THERE can be little doubt that the more important contributions to our knowledge of earthquakes must be made in countries like Switzerland, Italy, and Japan; countries where earthquakes are frequently occurring, where, occasionally, they are so disastrous as to arrest universal attention, and where, at the same time, there are many skilled observers aided by a sympathetic and intelligent public. In England, as every strong shock shows, there is no lack of observers. But our earthquakes that are strong enough to attract general notice within the disturbed area are few and far between. If we exclude special districts, like Comrie and the Durham coast, we shall probably be well within the mark in stating the average number recorded as less than one a month.

The number of earthquakes that occur in Great Britain must, however, be far greater than this. From various causes, many shocks that are felt are never placed on record. Others, again, that might be felt, must certainly pass unnoticed, for, wherever seismic studies are newly organized, it is found that people become educated in detecting earthquake-shocks. But, however skilful observers may become, there must always be a large number of shocks that never could be felt, either from the small amplitude or the long period of their vibrations. Even in Tokio, where they talk about earthquakes as we in England talk about the weather, "the majority of shocks pass unfelt by people, while seismographs register them sufficiently to allow measurements" (S. Sekiya, Japan Seism. Soc. Trans., vol. x. p. 59).

There is every reason to conclude, then, that, with the aid of simple time-recording seismoscopes, the earthquakes of Great Britain would be found sufficiently numerous to repay a more careful and systematic study. That we shall have to be content, as a rule, with observing shocks that would elsewhere be considered slight is evident of course; but in their very feebleness we possess advantages which are not afforded by severer shocks of other lands. Not only are the phenomena much less complex; but, not being unnerved by danger, the observer is able to concentrate his attention on them more calmly and completely. Still more important is the fact, and in this lies their greatest value, that, the smaller the area disturbed, the more nearly can the position of the epicentrum be determined. If, as is frequently the case, the shock be felt only within a small circular area, we cannot be far wrong in regarding the centre of that area as the approximate site of the epicentrum. And thus we easily obtain the solution of what, in a great earthquake, is one of the most difficult and important of the problems to be attacked.

Methods of Study in Great Britain.—Owing to the feebleness of our shocks, and their comparative rarity in a given district, the methods of study employed by us must clearly be different from, and inferior to, those adopted with such signal success in Italy and Japan. We can hardly expect, for instance, that costly recording instruments will be widely used in this country; for, even in Italy, as Prof. de Rossi points out (*Bull. del Vulc. ital.*, anno iv., 1877, p. 5), it has been found better to have a large number of observatories containing cheap and simple instruments, than a few equipped with seismographs more perfect and refined.

If, on the one hand, then, our methods of earthquake study are limited by the nature of the shocks we experience, on the other we possess advantages, apart from those already mentioned, that are more or less wanting in regions where the phenomenon attains a more destructive and interesting development. For instance, most parts of England at any rate are so densely populated that we are able, almost wherever a shock occurs, to procure a large number of observations of very considerable value. And again, in the ease and accuracy with which we can regulate our clocks in the neighbourhood of every large town, we have an aid in our work which is as valuable as it is rare in foreign countries. These two facts in particular I mention here because they form the foundation of our two most promising methods of investigation.

Looking at earthquakes chiefly from a geological point of view—that is, regarding them as mere incidents, but at the same time delicate indices, of the progress of terrestrial evolution—the prime object of our inquiries is in every case to determine the position of the epicentrum, and, if possible, that of the seismic focus. For this purpose, we have three methods at our disposal, depending severally upon observations of the direction, intensity, and time of occurrence, of the shock in different parts of the disturbed area.

The first method is interesting historically from its having been used by Mallet in the earliest scientific study of an earthquake. But modern seismologists have with good reason generally discarded it; and, in any case, it could hardly be employed with success in this country.

The method of intensities is far more trustworthy, and is attended with good results whenever the observations are sufficiently numerous and made at places that are fairly evenly distributed over the disturbed area. With the aid of such a scale as that drawn up by MM. de Rossi and Forel, the intensity of the shock at any point may be roughly estimated. Then, drawing lines including all places where the intensity is at or above a certain degree of the scale, we obtain a series of lines of equal intensity (isoseismal lines), which, closing in towards the epicentrum, enable its position to be approximately determined. For the slighter shocks that we experience, it would be difficult to over-estimate the value of this method, the only one that in certain cases can be applied.

The last of the three methods, I think I may say, is still upon its trial; and if, so far, it has not yielded all the results that are to be expected from it, I believe the reason is that it has not yet been attempted in a country where the conditions are so favourable for its application as they are in many parts of England. What we require for the purpose is, not a network of time-recording instruments extending over the whole country, so much as a moderate number suitably placed and regularly observed in specially selected districts. If, by means of these instruments or otherwise, the times of a shock can be ascertained with accuracy at five or more places, these, under certain conditions, are theoretically sufficient to determine the position of the epicentrum, the depth of the seismic forces, the velocity of the earth-wave, and, consequently, the time of occurrence at the focus. And it should be noticed that time-recorders in Great Britain are practically free from the objection which attends them in Japan and other regions where earthquakes frequently last for one or several minutes. For, in such cases, the character of the shock varies so greatly throughout the disturbed area, that it need not, and probably will not, be one and the same vibration which is registered in different places, and considerable errors may thus be introduced.[1] If, then, we remember that our earthquake shocks seldom last for more than a few seconds at most, and that, in many parts of England and some parts of Scotland, it should be possible to ascertain the time of occurrence correctly to within a small fraction of a minute, I think there can be little doubt that, for all but the slightest shocks, a most fruitful method of earthquake

[1] A Paper by Charles Davison, M.A., King Edward's High School, Birmingham; read before the Birmingham Philosophical Society on February 5, 1890. A few passages added since the paper was read are enclosed in brackets.

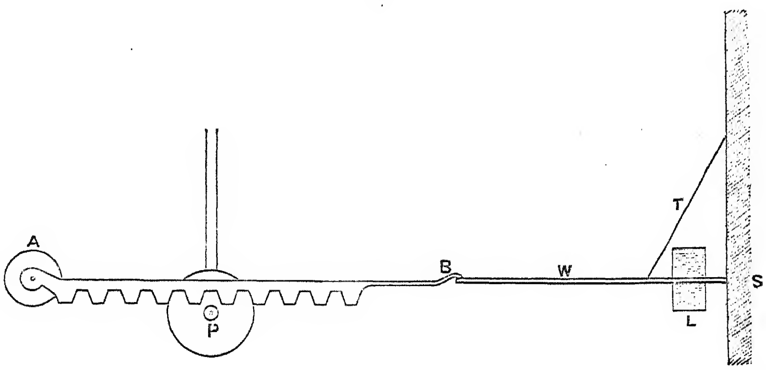
[1] E. Knipping and H. M. Paul, Japan Seism. Soc. Trans., vol. vi. p. 37; also J. A. Ewing, vol. iii. pp. 63–54, and J. Milne, vol. iv. pp. 100–101.

study in this country would be a system for securing accurate time-records whenever a shock is felt.

Seismoscopes.—A large number of simple and inexpensive seismoscopes have been devised and used for recording the time of occurrence of an earthquake shock; but it is difficult to find one that in all respects is thoroughly satisfactory.[1] To be so, they should fulfil the following conditions:—They should be inexpensive, simple in their construction, easy to arrange, and require little attention when once erected. They should record the occurrence of shocks and tremors with equal facility in whatever direction they may arrive; and they should be equally sensible in recording a feeble shock. It is very desirable also that they should be of similar construction, at any rate in a given district, if not throughout the whole country, so that observations from different places may be rightly comparable.

Again, in countries where earthquakes are frequent, and where the shocks may succeed one another at short intervals, it is important that the record should be made without stopping the clock. In Great Britain, however, our catalogues show that, except at Comrie, it is not usual for sensible earthquakes to follow one another rapidly, and it is therefore worth while considering whether, on account of their much greater cheapness and simplicity, it might not be well to avail ourselves in this country of clock-stopping apparatus. Such instruments, are, of course, defective in that, until re-set, they are incapable of recording a second earthquake. But they possess a compensating advantage in the accuracy with which it is possible to time the occurrence of a shock.

Clock-stopping Apparatus.—As it is possible to make with ease, and at little cost, an extremely delicate apparatus for stopping a clock at the moment of a shock, I quote the following description of one devised by Prof. J. Milne (Japan Seism. Soc. Trans., vol. iii. pp. 61–62):—"P is the pendulum of a clock with a small piece of wire standing out at right angles to its face. . . . This wire, as the pendulum swings, passes beneath a series of teeth cut in a strip of wood lightly hinged at A and terminating at the other end, B, with a piece of stiff wire. . . . If such a contrivance is allowed to fall, the teeth catch on the projecting pin of the pendulum, and it may arrest it at any portion of its swing." The arrangement which, at the time of an earthquake, allows the toothed lever to fall "consists of a piece of stiff wire, W, on which, near to one end, is a small cylinder of lead, L. The short end of this wire is pointed, and rests on a pivot-hole made in the head of a drawing-pin pressed into the side of the clock-case, S. To prevent this wire from falling, it is held up by a small silk thread, T, fastened to a second drawing-pin. As suspended, it is very unstable, and, instead of remaining at right angles to the clock-case, it swings round against it. When, however, the wire, B, rests on the end of W, it retains its position, as shown in the figure."



This instrument is so sensitive that it is difficult even to shut the clock-case without stopping the clock. The reason of this appears to be "that, if the clock-case receives a small displacement at right angles to W, the weight remains steady by its inertia, whilst the long arm of W in contact with B multiplies the initial motion" approximately in the proportion of the length of the long to that of the short arm of W.

It would appear that a displacement parallel to the wire W would not give this multiplication; but, practically, Mr. Milne observes, "it seems impossible to give a motion in that direction to which the apparatus does not seem to be just as sensible as to a motion in any other direction. The only other motion which does not result in stopping the clocks appears to be a *very slow* easy swing;" and thus the instrument will probably be incapable of recording the occurrence of the dying-out vibrations of a very distant shock.

The instrument may be placed in a cellar or out-house, or out of doors under the cover of a close-fitting box. A strong stake should be driven into the ground, to the depth of two or three feet, the floor, if any, being removed for a few inches round the stake to prevent the instrument being disturbed by the vibrations of the house. The clock-case should then be screwed firmly to the stake.

If several of these instruments are erected in a district, they should be placed at distances of not less than 5 to 10 miles apart.[1] The sites selected should, if possible, be free from the vibrations of passing carts and trains. If two or three of these record the occurrence of a shock at very nearly the same instant, it may be inferred that the disturbance is not accidental in its origin, and the inference will be strengthened if several instruments closely agree in their indications. But a record from one alone must obviously be regarded as doubtful, if all the others were at the time in good working order.

Suggestions for the Observation of Earthquakes (without the use of special instruments).—Lists of questions for aid in the study of earthquakes have been drawn up by Prof. Heim and Prof. Milne.[2] The following questions are founded partly on these lists, but chiefly on the accounts of earthquakes in different places, and especially in this country. It is hardly necessary to insist that all notes should be written down on the spot, or as soon after the shock as possible; but it may be useful to remark that it is often just as important to note when a given phenomenon is *not* observed as to describe it fully

[1] A Committee of the British Association is at present considering the form of seismoscope most suitable for use in this country.

[1] If the clock be carefully rated, it should be possible to obtain the time of a shock correct to a tenth of a minute. The velocity of earth-waves is subject to wide variations, even in traversing the same rocks; but, taking it at 1000 feet per second, it follows that the earth-wave will pass over more than a mile in one-tenth of a minute. A good deal more than a mile, then, should separate every pair of stations where seismoscopes are placed.

[2] A. Heim, "Die Erdbeben und deren Beobachtung"; *Arch. des Sc. phys. et nat.*, 3me sér. t. iii. pp. 286–7; Fouqué, "Les Tremblements de Terre," pp. 133–4; Japan Seism. Soc. Trans., vol. i. part ii. pp. 3–4.

when it is observed. This applies particularly to the mere fact of the perception of the shock, as a knowledge of the places where it is just not felt is of service in enabling us to determine the boundary of the disturbed area.

The questions are arranged in the following sections: A, for places where the shock is felt; B, for those where it is not felt; and C, inquiries to be made after the shock. In each case, the questions to which it is most important that answers should be given are marked with an asterisk.

A.—FOR PLACES WHERE THE EARTHQUAKE IS FELT.

1.—*Place of Observation.*

* (*a*) Its name and position.

(*b*) Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, &c.

2.—*Situation of Observer.*

* (*a*) Whether indoors or in the open air; if indoors, on which floor of the house.

(*b*) If indoors, the direction of the street or of the longer axis of the house (if detached).

* (*c*) How occupied at the moment of the shock—lying down, working, &c.

3.—*Time of Occurrence.*

* (*a*) Time at which the shock was felt; if possible to a tenth of a minute.

* (*b*) Is the time given the correct time obtained after comparison with an accurately rated chronometer? and, if so, how long after the shock was the comparison made, and how is the chronometer regulated?

4.—*Nature of Shock.*

* Describe the nature of the shock as closely as possible, stating especially: (*a*) the number of the more prominent vibrations; (*b*) their relative intensity; (*c*) whether there was any tremulous motion before or after the vibrations; (*d*) whether any vertical motion was perceptible; [(*e*) whether, in the latter case, the movement in the principal vibration was first upwards and then downwards, or *vice versâ*].

5.—*Duration of Shock.*

* Total duration, exclusive of that of the sound phenomena (stating whether estimated, or determined by watch).

6.—*Intensity of Shock.*

* Was the shock strong enough (*a*) to make windows, doors, fire-irons, or crockery, &c., rattle; (*b*) to cause the chair or bed on which you were resting to be perceptibly raised or moved; (*c*) to make chandeliers, pictures, &c., swing, or to stop clocks; (*d*) to overthrow ornaments, vases, &c., or cause plaster to fall from the ceiling; (*e*) to throw down chimneys or make cracks in the walls of buildings?

7.—*Direction of Shock.*

(*a*) Direction of the principal shock or shocks.

(*b*) Means by which the direction was ascertained.

(*c*) Was any change of direction perceptible during the earthquake?

8.—*Sound-Phenomena.*

* (*a*) If any rumbling sound was heard at the time of the shock, what did it resemble?

[* (*b*) Did the sound end abruptly, or die away gradually?

* (*c*) Did the sound become deeper or higher towards the end?]

* (*d*) Did it precede, accompany, or follow the shock? (Times useful, especially the intervals between the beginning of the shock and of the sound, and between the ending of the same; stating whether estimated, or determined by a watch.)

* (*e*) Duration (given by 5*a* and 8*b*, if not determined separately).

9.—*Effect on the Water of Ponds, &c.*

Were any movements observed in the water of ponds, rivers, lakes, or the sea, at, or shortly after, the time of the shock; if so, of what kind?

Accessory Shocks.

* Were there any slight shocks preceding or following the principal shock or shocks? If so, a list of these, with the place of observation, time of occurrence, and answers to any of the above questions, would be of great value.

B.—FOR PLACES WHERE THE EARTHQUAKE IS NOT FELT.[1]

1.—*Place of Observation.*

* (*a*) Its name and position.

* (*b*) Nature and form of the surrounding ground, especially with reference to its geological structure and the neighbourhood of mountains, rivers, cliffs, &c.

2.—*Situation of Observer.*

* (*a*) Whether indoors or in the open air; if indoors, on which floor of the house.

* (*b*) How occupied at the moment of the shock—lying down, working, &c.

C.—INQUIRIES TO BE MADE AFTER THE SHOCK.

1.—*Damage to Buildings.*

(*a*) Nature of the building damaged.

(*b*) Situation to the building, direction of its longer axis; neighbourhood to the edge of a cliff or bank, and on which side of this it lies; nature of the rock on which it rests.

(*c*) If any cracks formed, state in which walls; the direction and width of the cracks, and the points from which they start (sketches useful).

(*d*) If it is noticed that some walls are much damaged, while others at right angles to those are but little affected, what are the directions of these walls?

2.—*Rotation of Objects.*

* (*a*) If objects, such as chimneys, grave-stones, gate-pillars, &c., have been rotated on their bases during the shock, describe the initial and final positions of the objects (sketches useful); or state the direction and amount of the rotation (looking down on the object from above, is the direction the same as that in which the hands of a watch rotate, or opposite to that direction?).

(*b*) Is there any evidence of rotation in bodies with a circular base?

3.—*Effects on the Ground, Springs, &c.*

(*a*) Were any fissures or cracks formed in the ground? If so, state their length, width, depth, and direction, the nature of the ground in which they occur, and their relation to neighbouring cliffs, banks, &c.

(*b*) Was the height, quantity, or temperature of the water in springs affected by the shock?

4.—*Observations in Mines.*

If the earthquake was felt in a mining district, inquiries should be made as to the nature of the shock and of the sound-phenomena when observed by men in the mines; the depth of the workings in such cases, &c.

[1] [The value of observations under this heading would be greatly increased if they are the result of numerous inquiries made in a district.]

5.—*Records of Self-registering Instruments.*

* An examination should be made of the records of self-registering instruments within or near the disturbed area—particularly of recording barometers, magnetic and tidal apparatus—with a view to determine the effects of the shock on these instruments, and also to ascertain by their means the exact time of occurrence.

While answers to any of the above questions would be useful in the study of an earthquake, especial pains should, if possible, be taken to determine accurately the time at which the principal shock occurs. Immediately it is felt, the time should be noted to the nearest second, and written down at once, a few seconds (to be ascertained by trial) being allowed for taking out the watch and reading off the time. As soon afterwards as possible, the watch used should be compared with an accurately regulated clock. But if this cannot be done, if the record cannot be relied on as correct to within a small fraction of a minute, a less close approximation cannot as a rule possess much value. The chief use of such a record is then to determine the epoch of the shock; and, in a matter of this kind, when two consecutive shocks in a given district may be separated by an interval of several years, a question of a few minutes, more or less, is of very little moment.

Next in importance to time-observations are those on the intensity of a shock. Without the aid of delicate instruments it is of course impossible to estimate the intensity with accuracy. But good results have been obtained by the use of a rough scale, according to which the intensity is determined by its effect on men and their dwellings. The following is the Rossi-Forel scale,[1] which is widely adopted by Italian and Swiss seismologists:—

Rossi-Forel Scale of Intensity.

I. Micro-seismometric shock: noted by a single seismograph, or by some seismographs of the same model, but not by several seismographs of different kinds; the shock felt by an experienced observer.

II. Extremely feeble shock: recorded by seismographs of different kinds; felt by a small number of persons at rest.

III. Very feeble shock: felt by several persons at rest; strong enough for the duration or the direction to be appreciable.

IV. Feeble shock: felt by persons in motion; disturbance of movable objects, doors, windows; cracking of ceilings.

V. Shock of moderate intensity: felt generally by everyone; disturbance of furniture and beds, ringing of some bells.

VI. Fairly strong shock: general awakening of those asleep; general ringing of bells, oscillation of chandeliers, stopping of clocks; visible disturbance of trees and shrubs; some startled persons leave their dwellings.

VII. Strong shock: overthrow of movable objects; fall of plaster; ringing of church bells; general panic, without damage to buildings.

VIII. Very strong shock: fall of chimneys, cracks in the walls of buildings.

IX. Extremely strong shock: partial or total destruction of some buildings.

X. Shock of extreme intensity: great disasters, ruins; disturbance of strata; fissures in the earth's crust; rock-falls from mountains.

Results to be expected.—It may be useful, in conclusion, to point out some of the results we may expect to obtain from a systematic study of earthquakes in this country.

The mere indication of the occurrence of a shock felt at a given place on a given day is of service in the compilation of earthquake statistics, and will tend to give completeness to our seismic record. With the help of such a record we can study the laws of the periodicity and geographical distribution of earthquakes. The time is past for drawing up chronological tables of shocks felt over the whole earth; but the importance of making our records complete for a definite area of study is becoming more and more evident.

The accurate determination of the time of occurrence in different places is of the very highest importance. Such observations, if sufficiently numerous, will help us in investigating the position of the area which constitutes the epicentrum; the way in which the vibrations are propagated outwards from the epicentrum; the velocity of the earth-wave, and the laws according to which the velocity varies with the distance from the origin. A knowledge of the time will also determine the question of the coincidence of shocks in distant areas, separated by a region in which the shock is not felt at all, and of other phenomena which may seem to be more or less intimately connected with the earthquake.

By a study of the intensity in the different parts of the disturbed area, we are enabled to draw one or more isoseismal lines with a fair approach to accuracy. From the form of these lines we can ascertain the approximate position of the epicentrum; and, from the relative distances between consecutive pairs of such lines, we can determine the way in which the intensity decreases as the earth-wave radiates from the origin, and the relations of this decrease with the form and geological structure of the ground.

The chief point to which our researches at present tend is thus the discovery of the position of seismic foci. But our ultimate object is something higher than and beyond all this. With certain exceptions, the slightest earthquake that occurs must indicate the site and mark the epoch of a step in the process of terrestrial evolution. To determine the laws of seismic distribution in space and time would therefore be to discover, in part, the laws that regulate the development of the earth's great surface-features. The study of earthquakes is fascinating enough in itself, but it acquires a loftier significance when viewed in its wider relations; for through it we may press forward to the solution of the great problem of geology—the origin and growth of mountain-chains.

[1] *Arch. des Sc. phys. et nat.*, 3me pèr. t. xi. pp. 148–149; Fouqué, "Les Tremblements de Terre," p. 22 (footnote); *Bull. del Vulc. ital.*, anno iv. (1877), pp. 39–40.

THE HORNED DINOSAURS OF THE UNITED STATES.

IN vol. xxviii. of NATURE (pp. 439 and 515), an account was given by Prof. Moseley of the magnificent skeletons of Iguanodons now mounted in the Brussels Museum of Natural History, which were at that time regarded as among the most remarkable of that extinct group of giant reptiles commonly known as Dinosaurs. Since that date, however, we have been gradually—thanks to the indefatigable labours of the transatlantic palæontologists—acquiring a fuller knowledge of the representatives of this curious group, of which the remains are preserved in such fine condition in the Secondary rocks of the United States. Within the last few years, from the writings of Profs. Marsh and Cope—and more especially the excellent figures by which those of the former are illustrated—we have acquired so much information as to the form and structure of the gigantic Jurassic species belonging to the Sauropodous sub-order of the Dinosaurs—such as *Brontosaurus*—that we have begun to regard these extinct creatures as old friends (or should we rather say enemies?) and to flatter ourselves that our knowledge of the whole class is well nigh complete.

Recent discoveries in the topmost Cretaceous or Laramie deposits of North America have, however, brought to light the existence of a group of Dinosaurs, hitherto only very imperfectly known, which are remarkable, not only on

account of their gigantic dimensions, but as being the most bizarre and uncouth-looking forms which palæontology has yet brought to our notice. These are the so-called horned Dinosaurs of the Laramie, in regard to which several important memoirs have been published both by Prof. Cope and Prof. Marsh. There is, however, unfortunately some difference of opinion between these two eminent palæontologists as to the comparatively trivial point of the proper nomenclature to be applied to the various genera; and we must not be supposed to prejudge this question if we adopt the names employed by Prof. Marsh, to whom we are indebted for our illustrations.

As their name implies, one of the most striking features in the organization of these uncouth monsters is the presence of large horn-cores on the skull, as shown in Fig. 1.

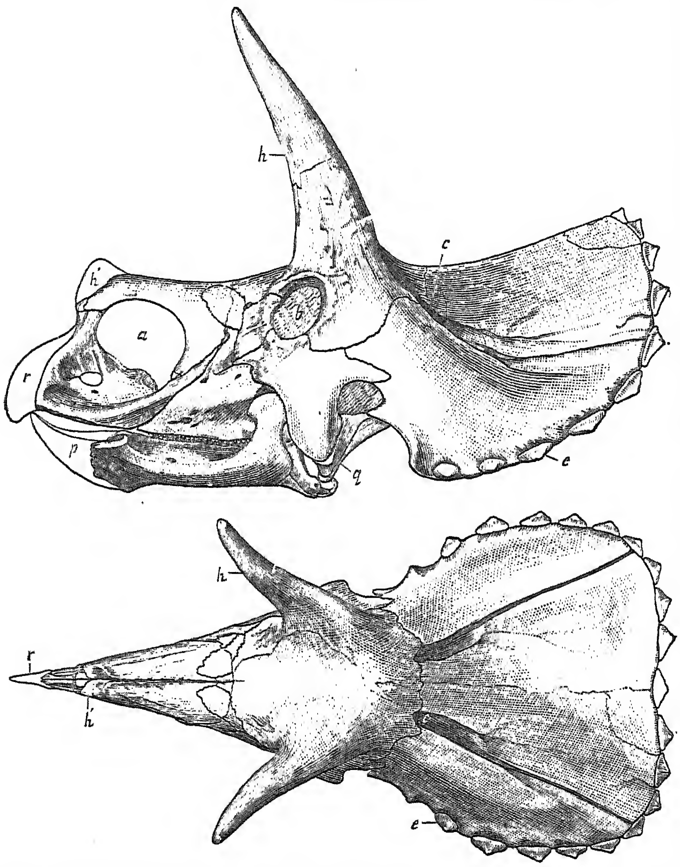


FIG. 1.—Left lateral and superior aspects of the skull of *Triceratops flabellatus*; from the Cretaceous of the United States. $\frac{1}{20}$ nat. size. *a*, nostril; *b*, eye; *c*, supratemporal fossa; *e*, epoccipital bone; *h*, frontal, and *h'*, nasal horn-core; *p*, predentary bone; *q*, quadrate bone; *r*, rostral bone. (After Marsh.)

These horn-cores are so like those of the oxen that some detached specimens found lying on the surface of the ground were actually described as belonging to an extinct bison.

The type of skull of which we give a figure belongs to the best known genus, for which Prof. Marsh proposes the name of *Triceratops*. It is remarkable not only for its gigantic size—the length of the figured specimen, which is said to indicate an immature individual, being about six feet—but also for its peculiar armature and structure. An imperfect skull of another species exceeds these dimensions, huge as they are, and is estimated when entire to have had a length of over eight feet. No other known animals, except whales, have a skull making any approach to these dimensions; that of the huge *Brontosaurus* being very small in comparison with the bulk of its owner. The skull of *Triceratops* is remarkable for its wedge-like form when viewed from above, and carries a pair of large horn-cores immediately over the eyes, and a short and single core above the nose. During life it may be inferred with a high degree of probability that these cores were sheathed with horn, like those of oxen, and that they proved equally effective weapons of defence. Equally remarkable is the huge flange-like expansion of the posterior region of the skull, evidently necessary for the attachment of muscles sufficiently powerful to support such a ponderous structure; and it is also peculiar for the presence of an *epoccipital* bone (*e*), which is quite unknown in all other animals. The structure of the teeth is somewhat similar to that obtaining in *Iguanodon*, but each tooth has two distinct roots. As in the latter, the extremity of the lower jaw is devoid of teeth, and likewise has a separate predentary bone at its extremity. The upper jaw is, however, quite peculiar in having a distinct toothless *rostral* bone at the extremity of the premaxillæ. It would thus seem probable that the mouth of these reptiles formed a kind of beak sheathed in horn like that of a tortoise. In young individuals the nasal horn-core is a separate ossification, but in the adult it becomes firmly anchylosed to the underlying bones; so that in this respect we have a precise analogy with the horn-cores of the giraffe. The brain of the creature is very minute—relatively smaller, indeed, than in any known vertebrate; this, however, might have been expected from the size of the brain in other Dinosaurs, since, in the same groups, large animals always have relatively smaller brains than their smaller allies.

Besides mentioning that the limb-bones resemble those of the armed Dinosaurs known as *Stegosaurus*, the only other portion of the skeleton to which we shall allude is the pelvis, of which a representation is given in Fig. 2. In this portion of the skeleton the haunch-bone or ilium (*il*) is remarkable for its great extension both in front of and behind the cavity, or acetabulum (*a*), for the head of the thigh-bone; and also for its horizontal or roof-like expansion, which is in marked contrast to the vertical plate-like form which is assumed by this bone in most other members of the order. With one important exception, the general contour of the pubis and ischium also comes nearest to that found in *Stegosaurus*; this being especially shown in the relation of the former bone to the ilium, and in the shape of the plate which it gives off to form the inner wall of the acetabulum. The remarkable exception is, however, that whereas in *Stegosaurus*, *Iguanodon*, and all other allied forms the pubis gives off a long backwardly projecting process running parallel with the ischium, in the present form there is no trace of any such process.

Mainly from the absence of this postacetabular process of the pubis, Prof. Marsh is disposed to regard the horned Dinosaurs as constituting a distinct primary group of the order; equivalent to those generally known as Sauropoda, Theropoda, and Ornithopoda. The resemblance in the structure of the limb-bones, and in a less degree that of the pelvis, to the loricated forms known as *Stegosaurus*, together with the nature of the dentition, render it, however, far more probable that we should regard these strange reptiles as peculiarly modified forms referable to the sub-order Ornithopoda—the group which includes *Iguanodon* and *Stegosaurus*. In the

course of his description Prof. Marsh remarks that, from the relatively large size of the humerus, the horned Dinosaurs were evidently quadrupedal; and since the presence of a postacetabular process to the pubis is evidently (as exemplified in birds and *Iguanodon*) in some way connected with the bipedal progression, it may be a fair inference that, owing to the resumption of a quadrupedal progression in the forms under consideration, this process has been lost. We may note that the pubis of *Triceratops* seems undoubtedly to correspond with the pre-acetabular portion of the pubis of *Stegosaurus*, and not with the pubis of *Megalosaurus*, which represents the postacetabular portion of the latter.

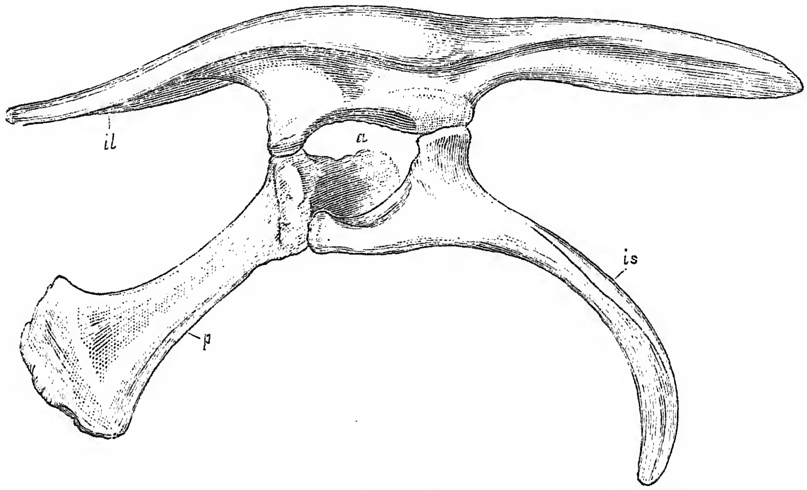


FIG. 2.—Left lateral aspect of the pelvis of *Triceratops flabellatus*. $\frac{1}{12}$ nat. size. *a*, acetabulum; *il*, ilium; *p*, pubis; *is*, ischium. (After Marsh.)

The nature of the dentition clearly shows that the horned Dinosaurs of the Laramie were of herbivorous habits, and as it seems impossible that any carnivorous Dinosaurs could have successfully waged war against such giants, we may fairly regard them as the lords of the plain in the distant Cretaceous epoch.

In conclusion we may venture to express the hope that future "finds" will enable the palæontologists of the United States to give us ere long a complete restoration of the skeleton of these mighty denizens of a long-past epoch.

R. L.

THE MEETING OF THE BRITISH ASSOCIATION AT LEEDS.

ON September 3 the sixtieth meeting of the British Asso ciation will be opened at Leeds by the President-elect, Sir F. A. Abel, F.R.S. The address, the lectures to the Association, and that to the operative classes will be delivered in the Coliseum, in which upwards of 3400 persons can be well and easily accommodated. By the courtesy of the Mayor and Corporation, the Victoria Hall will be used as the reception-room, and other rooms in the Town Hall will be provided for the various offices. Excellent Section rooms within short distances of the Town Hall have been secured by the kindness of various public and private bodies.

A guide-book, giving an account of the geology, history, places of interest, and manufactures of Leeds and the district, has been prepared, and a list of lodgings and hotel-accommodation has been drawn up. Various facilities are offered by the railway companies.

His Worship the Mayor of Leeds proposes to invite the members and associates to a reception and *conversazione* in the Municipal Art Gallery; a *soirée* will be given by the Executive Committee; and an afternoon reception at the Yorkshire College.

From the facility of access due to its central position in the railway system, from the number and variety of its industries, and from the beauty and interest of the country by which it is surrounded, Leeds offers exceptional advantages to visitors, of which many eminent members and foreign men of science have already expressed their intention of availing themselves.

Members interested in applied science and manufactures will be able by the courtesy of employers of labour to acquaint themselves with most of the modern processes by which the wealth of England is being augmented. They will be able to follow the smelting and working of iron until it is converted from clay-ironstone and hematite into tools, engines, pumps, textile machinery, and, in short, into everything which can be made of iron or mild steel. They can inspect the modern improvements in the old industry of Leeds by which wool, shoddy, and mungo are converted or reconverted into woollens or worsteds, and subsequently into clothes. Tanning, boot and shoe making, brewing, and the manufacture of sanitary, fire-resisting, and artistic earthenware employ a large number of hands; while among minor industries may be noted the manufacture of sulphuric acid and other chemicals, of bottles, of paper, of soap, of matches, and of soda-water.

Those interested in geology or scenery will find on the coast and in the diversified strata exposed, much that will instruct and interest them; while, to the historian, the architect, and the archæologist, the minsters, the cathedrals, the abbeys, the churches, the castles, the Roman remains, and the historic houses will furnish many objects worthy of attention.

Excursions may be taken or will be organized, in many cases by invitation, to most of the following places:—Add Church, Kirkstall Abbey, Temple Newsam, Farnley Hall, Harewood House, Boston Spa, Low Moor Iron-works, Pontefract Castle, the Ruskin Museum, Walton Hall and Wakefield, Aldborough, Beverley Minster, Bolton Abbey and Skipton, Castle Howard, Ingleborough, Harrogate, Hemsley and Rivaulx Abbey Malham Tarn and Gordale Scar, Richmond, Ripon Cathedral and Fountains Abbey, Settle and the Victoria Cave, Scarborough and the coast, Wensleydale, and York.

NOTES.

A MEETING was held at Stonyhurst College, on Tuesday, to consider a proposal for the establishment of some memorial of the late Father Perry. Sir Edward Watkin, M.P., presided, and he was supported by the Bishops of Salford, Shrewsbury, and Mangalore (India), Sir John Lawson, and a large body of Catholic gentry. It was resolved that the memorial should consist of a 16-inch equatorial telescope. A Committee of scientific men was appointed.

THE Town Council of Edinburgh has resolved to renew the invitation to the British Association to meet in Edinburgh in 1892.

THE Australasian Association for the Advancement of Science will hold its third annual meeting at Christchurch, New Zealand. The first general meeting will take place on January 15, 1891, at 8 p.m., when Baron F. von Müller, F.R.S., will resign the chair, and Sir James Hector, F.R.S., President-elect, will deliver an address. The railway authorities of Queensland, New South Wales, Victoria, and South Australia have consented to allow members who are going to attend the Christchurch meeting to obtain return tickets to Sydney or Melbourne at single fares; and various steamship companies have undertaken to convey members to Sydney or Melbourne and back at a reduction of 20 per cent. on the ordinary rates. Application has been made to the New Zealand Shipping Company and to the Shaw, Savill, and Albion Company for passages at reduced rates to members of the British Association visiting New Zealand to attend the meeting, and it is expected that this will be granted. Information may be obtained from Mr. A. Vaughan Jennings, 27 Chancery Lane, the local secretary in London.

THE International Medical Congress, now at work in Berlin, held its first meeting on Monday in the Renz Circus. The Berlin correspondent of the *Times* says it is calculated that no fewer than 4500 members of the medical profession were present, representing every State and city in Europe. Many also came from North and South America. The French delegates, 34 in number, were received with marked cordiality. The medical profession in England was largely represented, among those present being Sir James Paget, Sir Henry Acland, Sir Joseph Lister, Sir John Banks, Sir William Turner, of Edinburgh, Sir William Stokes, of Dublin, Prof. Grainger Stewart, of Edinburgh, Dr. Dick, Director-General of the Naval Medical Department, Mr. Ernest Hart, representing the British Medical Association, Surgeon J. K. Notter, of Netley, representing the War Office, and Dr. Lauder Brunton. The proceedings began with the opening address of the President, Prof. Virchow, who heartily welcomed to Berlin his *confrères* from all parts of the world. The President was followed by Dr. Lassar, Secretary-General of the Congress, who sketched the general plan of the labours of the Congress, and gave some interesting statistics regarding the representation of the countries taking part in it. After Herr von Gossler, Minister of Public Worship, and Herr von Forckenbeck, Burgomaster of Berlin, had welcomed the members in the name of the State and of the town of Berlin, several of the foreign délegates addressed the Congress. Dr. Hamilton, Surgeon-General of the American Army, was the first speaker. He was followed by Sir James Paget, who, on mounting the tribune, was warmly received. A paper on "The Present Position of Antiseptic Surgery," by Sir Joseph Lister, brought the proceedings to a close. At the end of the plenary sitting, the Congress resolved itself into its various Sections, which met in the halls of the Exhibition buildings in Moabit. The proceedings in the Sections on Monday were, for the most part, confined to the election of the various office-bearers. The serious work began on Tuesday.

ON July 22 Messrs. D. C. Worcester and F. S. Bournes left Minneapolis for the Philippine Islands, where they will spend two years in the study of distribution, the collection of birds and corals, and the prosecution of general zoological and botanical work. The expedition was fitted out at a cost of over $10,000, by Mr. L. F. Menage, of Minneapolis, and the collections made by Messrs. Worcester and Bournes will be deposited in the museums of the Minnesota Academy of Sciences at Minneapolis, where also the work upon the collections will be conducted after the return of the explorers.

ON Friday evening an important decision was arrived at in the House of Commons with regard to the revenue to be derived from the new duties on spirits in England. Mr. A. Acland moved that the Council of any county or county borough should receive power to use for the promotion of technical education any part of the share allotted to it, and that the remainder might be used as an educational endowment within the meaning of the Endowed Schools Act, the County Council acting as the governing body of the endowment. To the second part of this amendment—that relating to intermediate education—the Government declined to assent. The first part, however, they accepted. Mr. Mundella, Sir Lyon Playfair, and Sir Henry Roscoe expressed regret that the entire proposal of Mr. Acland was not adopted, but were unanimous in thinking that the decision of the Government, so far as it went, was most satisfactory. Sir Henry Roscoe said he wished to be allowed to say how gratified he was at the acceptance by the Goverment of the first portion of his hon. friend's amendment. To the great centres of industry which had already accepted the Technical Education Act it would be a matter of very great importance. In small places also, especially in the country, the money would be of the very greatest consequence.

DR. NANSEN's expedition to the North Pole will start in the spring of 1892. Captain Sverdrup, who will take the nautical command, is at present on board a fishing-boat in the Polar seas, practising manœuvring among ice. Dr. Nansen wishes that his crew may consist wholly of Norwegian sailors, but will admit some foreigners among the scientific staff.

Science announces that Prof. R. S. Woodward, who was for many years chief geographer of the U.S. Geological Survey, has been appointed assistant in the U.S. Coast and Geodetic Survey. Before his connection with the Geological Survey, Prof. Woodward was assistant astronomer of the U.S. Transit of Venus Commission. He was chairman of the Section of Mathematics and Astronomy of the American Association for the Advancement of Science in 1889, and is well known for his investigations in mathematics, astronomy, and physics.

WE learn from *Science* that records have been received, at the office of the U.S. Coast and Geodetic Survey, of observations made during the last cruise of the *Pensacola*. The stations include the West Coast of Africa, and some islands in the North and South Atlantic. The work was done by an officer of the survey, Assistant E. D. Preston, aided by members of the ship's company. Gravity and magnetic measures were made at St. Paul de Loanda (Angola), Cape of Good Hope, St. Helena, Ascension, Barbadoes, and Bermuda. In addition, magnetic observations alone were made at the Azores (Fayal), Cape Verde Islands (Porto Grande), Sierra Leone (Freetown), Gold Coast (Elmina), and in Angola at Cabiri. The pendulums used in the gravity work were the ones employed in 1883 in Polynesia, and in 1887 at the summit of Haleakala and other stations in the Hawaiian Islands. The computations are now under way at the office in Washington.

PROF. HUXLEY contributed to the *Times* of Tuesday a valuable letter on medical education—the subject with which Dr. Wade had dealt in his Presidential address to the British Medical Association. In this letter Prof. Huxley urges that the scientific training of medical students, and of those who propose to

become medical students, should be much more thorough and exact than it has hitherto generally been. "Those who know what modern medicine is," he says, "are well aware that four years would be but a brief period of study, even if it could be allotted exclusively to the practical branches of the medical science and art. But in the present condition of middle-class education the youth of 17 too commonly enters the medical school, not only devoid of the slightest tincture of scientific knowledge, but, what is worse, so completely habituated to learn only from books or oral teaching that the attempt to learn from things and to get his knowledge at first hand is something new and strange. Thus a large proportion of medical students spend much of their first year in learning how to learn, and when they have done that, in acquiring the preliminary scientific knowledge, with which, under any rational system of education, they would have come provided." Prof. Huxley does not, of course, underrate the importance of a proper literary training for medical students. This, with adequate instruction in science, they might, he thinks, obtain, if our methods of education were improved. The reform for which he especially pleads is that "the time wasted in forcing upon the medical student a sham acquaintance with Latin should be devoted to teaching him the use of his own language and the right enjoyment of its literary wealth, no less than to the study of science."

THE third summer meeting of the University Extension and other students began at Oxford on Friday last. At the opening meeting Prof. Max Müller delivered an address. He defended the method of teaching by means of lectures, but admitted that most lectures were too long, and recommended that they should be limited, as in Germany, to three-quarters of an hour. He also defended the annual gatherings at Oxford against the charge of being mere academical picnics. He showed how well the different classes of lectures had been arranged so as to meet the requirements of different classes of students. He pointed to the large and zealous classes attending these lectures, and to the substantial work done by students who stayed at Oxford for two or three weeks after the public lectures given during the first fortnight were over. Finally, he dwelt on the silent influence which a stay at Oxford must exercise on everyone. "I doubt not," he concluded, "that while teachers and hearers are exploring together in this place the ruins of ancient thought and the labyrinth of modern science, they will feel the silent influence of Oxford, and take to heart the lesson which our University has taught to so many generations of Englishmen, Scotchmen, and Irishmen—respect for what is old and the warmest sympathy for what is new and true."

A CONFERENCE in connection with the University Extension movement was held on Tuesday in the debating hall of the Oxford Union, Mr. Arthur Sedgwick, of Corpus Christi, presiding. There was a large attendance. The subject for discussion was—Is it desirable that local committees should seek to obtain a Treasury grant in aid of the expenses of University Extension teaching? If so, on what conditions is it desirable that the grant should be distributed? The chairman said, speaking as a private individual, and not as a delegate, he most heartily assented to the proposal to ask for State aid for University extension. It seemed to him that there was no test which they could apply in order to see whether an object was worthy of State aid which could not be successfully applied to University extension. In order that the movement might have its proper development it was absolutely necessary that there should be elements of permanence in it. Experience had shown that, at any rate with existing machinery and existing resources, it was extremely difficult to establish this element of permanence. Mr. Macan moved, "That this conference supports the proposal of State aid to University extension, provided that aid could be given without undue State interference." Mr. Mackinder seconded the resolution, which was carried by an overwhelming majority. In the evening a second conference was held in the Examination Schools, the chair being occupied by Mr. J. G. Talbot, M.P. The subjects discussed included University extension teaching in training colleges, village lectures, students' associations, and University extension teaching in connection with free public libraries.

MR. COSMO NEWBERY, the analyst of the Mines Department, Victoria, has been speaking strongly as to the necessity for a central School of Mines in Victoria. He would like that such a school should, if possible, be established in connection with the Melbourne University. If that proved to be impossible, he would be content with the development of the well-known school at Ballarat. Mr. Newbery's views on the subject are vigorously supported by the *Australian Mining Standard*, which thinks that a central school, thoroughly organized, could not fail "to exercise an important influence in the development of mining science in Australasia."

THE University College of Bristol has issued its Calendar for the session 1890–91.

A "*Bibliothèque Darwinienne*" has recently been started in Paris. The series will deal for the most part with sociological subjects. The first volume is by M. P. Combes, and relates to animal civilizations.

ADVANTAGE is being taken of the Eiffel Tower to obtain high pressure through a manometric tube (the height of the tower) containing mercury. M. Cailletet proposes to utilize the enormous pressure—about 400 atmospheres—for his researches on the liquefaction of gases, and interesting results may be looked for.

WE extract from *La Nature* of July 26 the following facts relating to exceptional seasons in past centuries. They have been collected by M. Villard, of Valence, for France especially, and for Europe generally. In 1282 the winter was so mild that cornflowers were sold in Paris in February. New wine was also drunk at Liége on August 24. In 1408 the winter was so severe that nearly all the Paris bridges were carried away by the ice. Ink froze in the pen, although a fire was in the room. [A similar fact is quoted by Dove as occurring at Sebastopol on December 13, 1855.] All the sea between Norway and Denmark was frozen. The summers of 1473 and 1474 were disastrously hot. In the winter of 1544–45 wine was frozen in barrels all over France. It was cut with hatchets and sold by the pound. In 1572–73 nearly all the rivers were frozen. The Rhone was traversed by carriages at various places. In 1585 the winter was very mild; corn was in ear at Easter, but the third week in May was extremely cold.

THE *Annalen der Hydrographie und Maritimen Meteorologie* for July contains an article by Dr. G. Meyer, on the influence of the moon on weather. Although such investigations have hitherto given a negative result, the author thought that with the materials furnished by synoptic charts he might eliminate local influences, and he gives tables extending over a number of years, which seem to show the influence of the moon in lowering the height of the barometer in the months of September to January, at the time of full moon, and in raising it during the first quarter. The Deutsche Seewarte, which communicates the article, points out that a similar result has been independently arrived at by Captain Seemann, one of the assistants of the institution. The same effect or any other is not perceptible in other months.

AN ingenious contrivance has been recently adopted at the Hippodrome in Paris, with a view to producing scenic effects, in the central oval space, without the spectators opposite being seen at the same time. An elliptical screen of fine steel netting is let down in comparative darkness, so as to be about 12 feet in front of the benches. This is painted on the inner side with

a representation of the Place du Vieux Marché at Rouen (the piece being *Jeanne d'Arc*), and, as it is strongly illuminated, at a given moment, from the centre, the light outside being low, a spectator at any point has an excellent view of the scene, while seeing nothing of the crowd beyond.

The additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Miss Florence Schuler; an American Black Bear (*Ursus americanus*) from Canada, presented by Mr. John Sands; a Common Otter (*Lutra vulgaris*) from Ross-shire, presented by the Hon. J. S. Gathorne Hardy, M.P., F.Z.S; two Cape Doves (*Œna capensis*) from South Africa, presented by Miss Grace Debenham; two Imperial Eagles (*Aquila imperialis*) from Spain, presented by Mr. Walter Buck; two Smooth Snakes (*Coronella lævis*) from Hampshire, presented by Mr. E. Penton, F.Z.S; a Hairy Armadillo (*Dasypus villosus*) from La Plata, a Greater Sulphur Crested Cockatoo (*Cacatua galerita*) from Australia, deposited; five Common Peafowls (*Pavo cristatus*), six Ring-necked Pheasants (*Phasianus torquatus*), three Gold Pheasants (*Thaumalea picta*), five Silver Pheasants (*Euplocamus nycthemerus*), seven Californian Quails (*Callipepla californica*), a Vulpine Phalanger (*Phalangista vulpina* ♀) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on August 7 = 19h. 5m. 29s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4485	—	—	19 12 17	+29 53
(2) G.C. 4499	—	—	19 26 19	+ 9 0
(3) D.M. + 30° 3409 ...	6	Yellowish-red.	19 0 43	+30 34
(4) ε Aquilæ	4	Yellow.	18 54 36	+14 55
(5) λ Aquilæ	3	Yellowish-white.	19 0 24	− 5 3
(6) 222 Schj.	9	Very red.	18 53 30	+14 13
(7) S Herculis	Var.	Reddish-yellow.	16 46 53	+15 8

Remarks.

(1) This cluster is thus described in the General Catalogue: "A globular cluster; bright; large; irregularly round; gradually very much compressed in the middle; easily resolved." Dr. Huggins has observed that the spectrum is continuous, with "a suspicion of unusual brightness in the middle," but he apparently made no attempt to determine the position of the brightness. Such a maximum of light in one part of the spectrum is suggestive of radiation phenomena, though of course it is possible that it may be simply a contrast effect due to the presence of dark lines or bands. In any case trustworthy measures may give some clue to the constitution of the stars of which the cluster consists.

(2) The G.C. description of this object is as follows: "Considerably bright; small; irregularly round; easily resolvable." It is thus apparently an undoubted cluster, and it is therefore very remarkable that Dr. Huggins records: "I believe that the spectrum consists of one bright line." If this be confirmed, the object must evidently be a cluster of "nebulous stars," and resolvability can no longer be a criterion for non-nebulosity."

(3) Dunér describes the spectrum of this star as a feebly-developed one of Group II.; only the bands 2, 3, and 7 being seen. As the complete series of bands has been recorded in stars of much smaller magnitude with the same instruments, it is clear that there are decided specific differences. A more detailed examination, with special reference to the presence or absence of bright lines or flutings and dark lines, is suggested.

(4 and 5) These are stars of the solar type and of Group IV. respectively. The usual observations are required in each case.

(6) The spectrum of this star is one of Group VI. The dark bands are strong, but the blue zone is very feeble. Further details should be looked for.

(7) This variable has a spectrum of Group II., and the approaching maximum of August 15 may be utilized for ascertaining whether, in common with other variables of the same group, bright lines appear at or near maximum. The magnitude at maximum appears to vary between 6·6 and 7·7, whilst that at minimum is about 11·5, the period being about 408 days. The line of hydrogen at G is apparently the most easily seen in this class of objects. The bright flutings of carbon should also be carefully observed as the star fades. A. Fowler.

Catalogue of Red Stars.—No. V. of the Cunningham Memoirs of the Royal Irish Academy contains a new edition of Birmingham's "Catalogue of Red Stars," by the Rev. T. E. Espin. The work undertaken by Mr. Espin is (1) the observation of such stars of Mr. Birmingham's Catalogue as seemed to merit special attention; (2) a search for new red stars; (3) the spectroscopic observation of all stars not previously observed with the spectroscope. This comprehensive programme was commenced about four years ago, and much important work has been done under each of the heads. The original catalogue contained ruddy and orange stars in addition to those having a decided red colour, but these are now given in a separate list.

In some remarks on the spectroscopic observations of the stars in the Catalogue, Mr. Espin brings forward "one of the most striking examples of the disagreements among spectroscopic observers," viz. the difference between the spectrum of 152 Schjellerup as observed by Secchi and Dr. Huggins. The former observer remarked that the dark zones coincided with the carbon flutings given by an alcohol flame. Dr. Huggins made the comparison, and, either from imperfect instrumental conditions or a different comparison spectrum, found there was no such coincidence, although later observations, by Vogel, Dunér, and others, have established Secchi's view.

A useful list is given of stars with bright lines in their spectra discovered up to the date of publication, and no one has worked more assiduously in this direction than Mr. Espin himself. After an admirable and extended account of the discovery and the spectra of these stars the following conclusions are arrived at:—

(1) That in stars of type I.*c* (Group I.) where the hydrogen lines and D_3 are bright, the lines vary, and this variation is not simultaneous.

(2) That in stars with type III.*c* one or more of the hydrogen lines may be brilliant and the others invisible, as in Mira, where γ and δ were conspicuous, but there was no trace of ε and F.

(3) In the cases of R Andromedæ, R Cygni, and S Cassiopeiæ, the extremely brilliant F line was detected *after* the maximum.

(4) In Vogel's type I.*b*, the hydrogen lines may really be faintly bright, and in one of the stars of this class the existence of other bright lines is proved, and they will hence, probably, be found in others.

It should be remarked that the stars of Group II. which have bright lines in their spectra (*e.g.* Mira Ceta) are classified by Mr. Espin as a new type, III.*c*.

The total number of stars contained in the Catalogue is 1472, of which 766 are given in the red star catalogue, 629 in the list of ruddy stars, and 77 in an addendum. Besides these there are 52 "bright-line" stars. Seven new variables were detected by Mr. Espin during the four years of observation, and he concludes that the work of discovering new red stars in the northern heavens is complete as far as magnitude 8·5. Every spectroscopist appreciates this valuable and important Catalogue, and Mr. Espin is to be congratulated on having been able to complete it in so short a time.

Ancient Eclipses.—In the *Astronomical Journal*, No. 220, Mr. John Stockwell continues his discussion of the secular and long-period inequalities in the moon's motion. The following are the dates of the sixteen eclipses that have been investigated, and some particulars referring to them.

No.	Date.	No.	Date.
1.	A.D. 1140 March 20	9.	B.C. 480 April 19
2.	A.D. 1030 August 30	10.	B.C. 546 October 23
3.	A.D. 364 June 16	11.	B.C. 556 May 19
4.	A.D. 360 August 28	12.	B.C. 584 May 28
5.	A.D. 348 August 29	13.	B.C. 602 May 18
6.	B.C. 309 August 15	14.	B.C. 609 September 30
7.	B.C. 423 March 21	15.	B.C. 762 June 15
8.	B.C. 430 August 3	16.	B.C. 1184 August 28

1. This eclipse is mentioned by Halley, by William of Malmesbury, and in the Saxon Chronicle. It is shown that the line of central eclipse passed over Cambridge.

2. This is the eclipse of Stiklastad, and Mr. Stockwell's computations appear to satisfy the account given by Hansen in vol. ii., p. 388, of his "Darlegung."

3. Observed at Alexandria by Theon.

4. An annular eclipse which occurred before sunrise in any part of Mesopotamia, so that it could not have occasioned the phenomenon mentioned by Ammianus Marcellinus (book xx. chap. 3).

5. This eclipse was total in the eastern parts of Mesopotamia at 9h. 50m., and satisfies the phenomenon described by Ammianus.

6. The eclipse encountered by the fleet of Agathocles while on its voyage from Sicily to Africa.

7. The eclipse described by Thucydides as having occurred during the eighth year of the Peloponnesian War.

8. This eclipse is shown to be identical with that described by Thucydides as having occurred during the first year of the Peloponnesian War, when the darkness was so great that some of the stars were visible.

9. The account given by Aristides ("Scholiast," ed. Frommel, p. 222) of the eclipse which took place while Xerxes was on the march from Sardis to Abydos at the beginning of the Persian War is confirmed by the computations.

10. This is shown to explain the disappearance of the sun described by Xenophon ("Anabasis," Book iii.) as having occurred at Larissa.

11. Contrary to the conclusions of Hansen and Prof. Airy, Mr. Stockwell finds that this eclipse does not satisfy Xenophon's account.

12, 13, and 14. Each of these has been supposed to be Thales's eclipse. Mr. Stockwell finds that both 13 and 14 satisfy equally well the astronomical conditions of the problem, but thinks the former is rather the more probable of the two.

15. The record of this eclipse was discovered on the Assyrian tablets in the British Museum, and the computations show that an eclipse happened at Nineveh at two o'clock in the afternoon on the date given.

16. Homer mentions a singular darkness that occurred during one of the great battles of the last year of the Trojan War ("Iliad," Book xvi.). Mr. Stockwell explains the darkness by means of this total solar eclipse.

Many of the conclusions arrived at with respect to the dates of eclipses differ widely from those generally accepted, and are open to much discussion.

COGGIA'S COMET (*b* 1890).—*Edinburgh Circular* No. 9 contains the following elements and ephemeris, computed by Dr. Berberich, of Berlin, from observations made at Marseilles on July 19, and at Kiel on July 21 and 22. Dr. Berberich finds there must be an error in the comet's place deduced at Marseilles on July 18, the date of discovery. He also points out that the orbit closely resembles that of the comet of A.D. 1580.

Elements of Comet Coggia.

T = 1890 July 7·9775 Berlin Mean Time.

$$\left.\begin{array}{rl} \pi - \Omega = & 84^\circ\ 20'\ 52'' \\ \Omega = & 14\ \ 4\ 56 \\ i = & 63\ 28\ 17 \end{array}\right\} \text{Mean Eq. } 1890{\cdot}0.$$

$$\log q = 9{\cdot}88007.$$

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Aug. 7	10 43 24	+28 1·0	0·2526	9·9826	0·50
8	10 47 12	27 10·4			
9	10 50 53	26 20·3	0·2601	9·9929	0·46
10	10 54 27	25 30·7			
11	10 57 55	24 41·7	0·2676	0·0033	0·43
12	11 1 16	23 53·2			
13	11 4 31	23 53	0·2752	0·0137	0·39
14	11 7 41	22 18·0			
15	11 10 45	21 31·2	0·2827	0·0240	0·36

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE annual summer meeting of the Institution of Mechanical Engineers was held last week in Sheffield. There could be no more appropriate centre around which either this Institution, or the sister Society, the Iron and Steel Institute, could gather. Sheffield has, however, of late years been somewhat tardy in offering a welcome to visitors. Six years ago it was proposed that the Iron and Steel Institute should hold a session in Hallamshire, but Hallamshire would not open its doors, and the Iron and Steel Institute had to journey to Chester. It is 29 years since the Mechanical Engineers met in Sheffield, and now, when they once more congregate there, they find but a partial welcome. The fact is, the big Sheffield steel makers—the Browns, Firths, Cammells, Jessops, and Vickers—have always pursued an absurd policy of secrecy. There is as much Abracadabra about these Sheffield steel makers as ever was practised by the alchemists of old. One can walk into the other steel works of the country with no more formality than presenting one's card, and see all that is to be seen; but these Sheffield works remain a sealed book. The reason given for this is that "The Foreigner" comes over here and learns too much, imparting no information in return. Unhappily for the cogency of this argument it is just the foreigner that the steel makers must admit. All those firms who do work for foreign Governments must admit foreign Government inspectors. These men come into the works to stay for months or even years. They are experts in the business they are engaged upon. They come and go where they will, ask what questions they will, make analyses, tests, and experiments at will; in short, they obtain a thorough and complete knowledge of everything that goes on. When they return home they would look on two or three hundred a year as an ample income, or a hundred pound note as a handsome consulting fee.

In the face of such facts is it not childish to shut out the necessary engineer, who simply wants to satisfy his scientific curiosity regarding the chief material he uses?

Although the big steel makers had shut their doors on the Sheffield visitors, there were still some things of interest left. Many of the older class of crucible steel makers were willing to explain the whole process of steel production as introduced by Huntsman one hundred years ago, and indeed were able to give practical illustrations of the same. Steel affords as much food for contemplation to the industrial economist as to the physicist and chemist. That the addition of less than one half of one per cent. of carbon should so entirely change the character of the metal is curious enough, although so familiar; but that the making of crucible cast steel should have stood, as it has, through the last century of industrial change and revolution is still more surprising. Watt, Faraday, and Thomson, nay, even Bessemer and Siemens, have lived and laboured without writing a single record on the process. Crucibles are still made by hand, charged by hand, pulled out of the fire by hand, teemed by hand, and in fact the steam-engine is not called into requisition throughout the process. The steel manufacturer makes no chemical analysis to find the grade of his steel. He breaks a piece, and his eye tells him by the fracture the percentage of carbon nearly enough for all practical purposes; *i.e.* as nearly as his neighbour knows, who does the same. And yet if one wants trustworthy steel of the highest grade one has to go to Sheffield for it, and pay the Sheffielder's price. All the scienee of all the engineers, chemists, and physicists of the last hundred years, allied with the industrial activity engendered by the fierceness of modern competition—even the mingling of science and commercial acumen, as in the persons of Siemens and Bessemer—has failed to unseat the ancient steel trade of Sheffield. No wonder the grimy town remains the stronghold of industrial empiricism, where they fall down and worship with the prophets of the rule of thumb.

But though the crucible steel maker is conservative in his method of working, he proved liberal in showing his work to others, and the members of the Institution had a good opportunity of seeing the way in which the finer kinds of steel they use are produced. The works of Messrs. Seebohm and Dieckstahll, Samuel Osborn and Co., and many others in which crucible steel making is carried on, were open to inspection; but, had not Park Gate come to the rescue, those who were unacquainted with the Bessemer or Siemens processes would have had to go to South Wales, Glasgow, or the north-east, where they could find works open to their inspection quite as well organized as any they missed seeing at Sheffield.

There were eight papers down for reading during the meeting, the sittings being held on the 29th and 30th ult. in the large hall of Firth College. The President of the Institution, Mr. Joseph Tomlinson, presided throughout. The papers on the agenda were as follows:—

"On Steel Rails, considered chemically and mechanically," by C. P. Sandberg, London.

"On Recent Improvements in the Mechanical Engineering of Coal Mines," by Emerson Bainbridge, of Sheffield.

"Description of the Park Gate Iron and Steel Works," by C. J. Stoddart.

"Description of the Sheffield Water Works," by Edward M. Eaton, Engineer.

"Description of the Loomis Process for making Gas Fuel," by R. N. Oakman, Jun., of London.

"On Milling Cutters," by George Addy, of Sheffield.

"On some Different Forms of Gas Furnaces," by Bernard Dawson.

"On the Elihu Thomson Electric Welding Process," by W. C. Fish, of London.

The first five papers only were read, the other three being adjourned until the next meeting.

Steel rails first occupied the attention of members, Mr. Sandberg opening the business part of the meeting by reading his paper. The author attributed the well-known greater durability of the first Bessemer rails made in Sheffield, to the hammered blooms and slow running mills of early days. There is no doubt that hardness is a virtue in railway lines, and hardness may be obtained by work; but it can also be obtained chemically. By the latter means, however, other desirable features may be jeopardized. In the tables showing results of tests, given as an appendix to the paper, this point was strongly brought out, the amount of phosphorus being, in the case of some Russian rails, exceedingly high, in fact dangerously so in the opinion of some of our best authorities. In dealing with the question of silicon, the author gave a seasonable reminder as to the different composition required for steel which was to be used in bridge and ship work, and that intended for rails. This point was taken up in the discussion, Messrs. Windsor Richards, Jeremiah Head, and others speaking on the question of mechanical tests. Tensile tests were generally pronounced as undesirable, being costly, and of little or no use; the falling weight test, and a test for hardness, together with such light as might be thrown by chemical analysis, being considered sufficient. It may be pointed out, however, that there is no well-established means of testing for hardness. Mr. Wicksteed spoke to the point when he referred to the desirability of ascertaining the percentage of elongation, although, as will be seen, this is not a sure guide. This question leads up to another which arose in the discussion. Some of the rail-makers present exclaimed against engineers insisting on steel containing a given percentage of certain alloys. The engineers have nothing to do with chemical analysis, the metallurgists say; it is a subject they know little or nothing about, and yet they lay down the law to the steel makers, whose business it is. Let the engineers be satisfied with results, and leave to those who understand the question the means of attaining these results. This is very good logic as far as it goes, but unfortunately it is not easy to make tests which will definitely settle the question of practical use. One speaker very well said that he looked on the Metropolitan Railway as the best testing machine for rails in the world; and so long as engineers find that a given chemical analysis gives a durable and safe steel rail, they will be justified in asking for that analysis as supplementary to mechanical tests. Speaking on the latter question, the author says in the paper: "As for tensile tests, they tell us very little; for soft rails broke at only 33 tons per square inch, instead of 41 tons for the good rails; while the brittle rails gave almost the same tensile strength as the good rails, and even more elongation and contraction." Could the transverse test under a falling ball have been substituted for these slow and costly tensile tests, it would have shown better the merits of safe or brittle rails. It may be mentioned in passing that the hardness machine of Prof. Turner, of Mason's College, Birmingham, to which Mr. Hadfield made reference, appears to promise well as a means of determining the second desirable feature in steel rails. By the tables to which reference has been made, it was shown that 0·24 per cent. of silicon in steel rails gave the best results. This the author considers the most striking feature in the analysis.

Mechanical engineering in coal-mines, as described in the contribution of Mr. Emerson Bainbridge, next occupied the attention of the meeting. We do not propose giving an abstract of this paper in the present notice; it would be like trying to run the River Thames through a 12-inch main. The author ranged over the whole field of mining engineering; the illustrations, which were shown by aid of the magic lantern, being more than a hundred in number. This paper had evidently cost the author much trouble and time in its preparation, and was one eminently fitted for the consideration of the members of the Institution of Mechanical Engineers. Mr. Bainbridge is well known in the north as a mining engineer of ability—a fact which it is well to emphasize, as his paper was received by some members, not themselves acquainted with its subject, in a very ungracious spirit. It is to be hoped that the proposal which he made to withdraw the paper from publication in the Transactions will not be carried out.

The papers of Messrs. Sandberg and Bainbridge were the only two taken on the first day of the meeting, the sitting being adjourned about one o'clock for the members to visit the various works open to their inspection.

On the members reassembling on the next day, Wednesday, the 30th ult., the first paper taken was that of Mr. C. J. Stoddart. The author is the managing director of the Park Gate Iron and Steel Works, and in his contribution he dealt with the new plant for steel making lately erected there. Should it ever be necessary to put these works into the market, the paper would form an excellent auctioneer's catalogue, it reading more like a document of that nature than a memoir to be put before a meeting of a scientific or technical Society. There were, however, a few passages of historic interest which we reproduce. These works, which are near Rotherham, were founded in 1823, and here many of the iron rails used on the first railways were rolled; amongst some of the later ones were those for the Metropolitan Railway, many of which were case-hardened. Here, also, were rolled a large part of the iron plates used in the construction of the *Great Eastern*; whilst armour plates were first rolled here also. The latter were presumably for the *Warrior*, as she was our first armour-clad ship, and they were very different from the compound steel and iron plates now so elaborately prepared, being, it will be remembered, no more than 4½ to 5 inches thick. Park Gate has, however, had to abandon these early methods of iron working, and, advancing with the times, has laid down within the last two years a costly steel plant, the outline particulars of which are duly set forth in Mr. Stoddart's catalogue. We are not, however, disposed to quarrel with the author of the paper for not going more fully into particulars, as he was liberal enough, in his capacity as managing director, to invite the meeting to make an excursion to his works on the day following the reading of his paper. The members were therefore enabled to see for themselves the five blast furnaces, plate and sheet mills, bar mills and their appurtenances, four 25-ton Siemens furnaces, cogging mills, slab rolls, billet mills, and plate mills duly set forth in the author's list of plant. The capacity of the steel works is from seven to eight hundred tons of steel and from four to five hundred tons of plates per week.

Mr. Oakman's paper on the Loomis process of making gas fuel was next brought before the meeting. The apparatus in which Loomis gas is made consists mainly of a generator and steam boiler. The generator is not novel in principle, the air being drawn through the charging door in the top, whilst an exhauster is used to set it in motion. The result is producer gas, which is superheated and then led through the boiler to produce steam, finally passing to the gas-holders. This part of the process is carried on for about five or six minutes, after which the admission of air is suspended, and the steam which has been generated is carried through the incandescent fuel, having been previously superheated in the superheater. The second operation produces, of course, water-gas, which, however, has one great advantage over ordinary water-gas, inasmuch as it possesses a strong and characteristic odour. This proceeds from the hydrocarbons taken up from the fuel, a bituminous coal being used. The apparatus has been applied with success in Sheffield, notably by the big steel house of T. Firth and Sons. A representative of that firm stated during the discussion that a saving of at least 50 per cent. in the cost of fuel in the manufacture of crucible steel was made by using Loomis gas, as compared with the old method of melting by coke—a statement which we have no difficulty in accepting when it is remembered how extremely wasteful is the present usual method of firing.

The discussion which followed the reading of the paper soon fell into the familiar groove which seems to have become stereotyped for use whenever the question of gas fuel comes to the fore. Mr. John Head and Sir Lowthian Bell both spoke. The former naturally soon brought the subject round to the Siemens furnace; upon the merits of which he was speaking

—especially in the matter of cheapness—when he was stopped by the President. The practice Mr. Head follows at meetings of the scientific and technical Societies is not tending to enhance the respect felt for the once honoured name of Siemens. Sir Lowthian Bell said what he said in Paris last year over again. The position he takes up—that no more heat can be got from a pound of fuel than Nature put in it—is perfectly sound, but there is no need to repeat the truism at such great length and so often.

The Sheffield Water Works was the subject of the last paper read at the meeting.

On the whole, it cannot be doubted that the meeting of the Mechanical Engineers at Sheffield was below the average, and badly managed. If Mr. Eaton's paper on the water-works had been taken as read, and Mr. Addy's contribution on milling cutters had been brought forward, the meeting might have done something to redeem its character as a representative assemblage of Mechanical Engineers. The Catalogue of the Park Gate Iron and Steel Works might also have been taken as read. Both the latter and the water-works paper were acceptable as guides to the respective excursions, but that was no reason why members should be required to sit and listen whilst Mr. Bache read through them at a speed which rendered it quite impossible to follow.

We have not space to refer to the visits to works in Sheffield open to visitors, and indeed there was not much of exceptional interest. Exception must be made, however, to a loom for weaving horse-hair cloth, which was seen at the works of Laycock and Sons. The wonderful ingenuity displayed by designers of textile machinery appears here to have reached its culminating point. Horse-hair has several undesirable features from a textile point of view. The filaments are generally no longer than 3 or 4 feet; though exceptional hairs have been known as long as 6 feet, we believe. The thickness differs considerably at each end; the material is very elastic, and it is so hard that it will speedily wear away the hardest steel over which it may be dragged. In order to overcome these difficulties, the designer of this loom, Mr. W. S. Laycock, has introduced a shuttle with jaws that take hold of each hair as it is presented, and a device which is known as the selector. The latter is a hand—for we can call it nothing less—which picks up one hair, and only one, to present to the jaws of the shuttle. It has to let go at the very instant the shuttle takes hold, otherwise the hair would be dragged through its fingers, which would soon be worn away. Sometimes, however, the fingers fail to grasp this single hair; it must be remembered if it were to take two hairs the cloth would be spoiled. It then makes a second try, and, if the second fail, yet a third. Supposing the third attempt also prove unsuccessful, there being no time to make a fourth, the selector promptly stops the weft motion, so that no change takes place whilst the shuttle is making its traverse without a hair to form the weft. Theophrastus Such, after a visit to a textile factory, had a nightmare, in which mechanism usurped the place of humanity, and became the inexorable master of mankind. The conceit is worked out with much skill, and appears quite plausible when viewed in the light of mechanism which not only performs the most delicate operations, but knows when it misses, tries again as long as trying is of avail, and, if it fail at last, takes steps to prevent mischief following.

ON THE ORIGIN OF THE DEEP TROUGHS OF THE OCEANIC DEPRESSION: ARE ANY OF VOLCANIC ORIGIN?[1]

THE consideration of the question with regard to the origin of the ocean's deep troughs requires, as the first step, a general review of oceanic topography; for according to recent bathymetric investigations, the deep troughs are part of the system of topography, and its grander part. We need, for this purpose, an accurate map of the depths and heights through all the great area. Such a map will ultimately be made through the combined services of the Hydrographic Departments of the civilized nations. At the present time the lines of soundings over the oceans, especially over the Pacific and Indian, are few, and only some general conclusions are attainable. It is to be noticed that the system of features of the oceanic area are involved in the more general terrestrial system; but since the former comprises nearly three-fourths of the surface of the sphere, it is not a subordinate part in that system.

[1] This paper is accompanied in the *American Journal of Science* from which it is reprinted, by a bathymetric map.

With reference to this discussion of the subject I have prepared the accompanying bathymetric map.

I. THE BATHYMETRIC MAP, AND THE GENERAL FEATURES OF THE OCEANIC DEPRESSION DISPLAYED BY IT.

1. *The Map.*—In the preparation of the bathymetric map I have used the recent charts of the Hydrographic Departments of the United States and Great Britain,[1] which contain all depths to date, and the lists of new soundings published in German and other geographical journals. In order that the facts on which the bathymetric lines are based may be before the reader a large part of the depths are given, but in an abbreviated form, 100 fathoms being made the unit: 25 signifying 2500 fathoms or nearly (between 2460 and 2550); 2·3, about 230 fathoms, ·4, about 40 fathoms. Only for some deep points is the depth given in full. The addition of a plus sign (+) signifies no bottom reached by the sounding.[2]

In the plotting of oceanic bathymetric lines from the few lines of soundings that have been made, the doubts which constantly rise have to be settled largely by a reference to the general features of the ocean, and here wide differences in judgment may exist in the use of the same facts; but through the depths stated on the map, the reader has the means of judging for himself. In the case of an island the lines about it may often have their courses determined by those of adjoining groups, or by its own trend; but in very many cases new soundings are needed for a satisfactory conclusion.

Some divergences on the map from other published bathymetric maps require a word of explanation. The northern half of the North Pacific is made, on other deep-sea maps, part of a great 3000-fathom area (between 3000 and 4000) stretching from the long and deep trough near Japan far enough eastward to include the soundings of 3000 fathoms and over in mid-ocean along the 35th parallel. It has seemed more reasonable, in view of present knowledge from soundings, to confine the deep-sea area off Japan to the border-region of the ocean, near the Kurile and Aleutian Islands, and leave the area in mid-ocean to be enlarged as more soundings shall be obtained. Again, in the South Pacific, west of Patagonia, the area of relatively shallow soundings (under 2000 fathoms) extending out from the coast, is on other maps bent southward at its outer western limit so as to include the area of similar soundings on the parallels of 40° and 50°, between 112° and 122° W. The prevailing trends of the ocean are opposed to such a bend, and more soundings are thought to be necessary before adopting it.

It may be added here that in the Antarctic Atlantic, about the parallel of $66\frac{1}{2}°$ S. and the meridian of $13\frac{1}{2}°$ W., a large area of 3000 and 4000 fathoms has been located. It was based, as I have learned from the Hydrographic Department of the British Admiralty, on a sounding in 1842 by Captain Ross, R.N., in which the lead ran out 4000 fathoms without finding bottom. The sounding was, therefore, made before the means available were "sufficient to insure the accuracy of such deep casts."[3]

2. *The Feature-lines of the Oceanic and Bordering Lands.*—The courses of island-ranges and coast-lines have a bearing on the question relating to the courses of the deep-sea troughs, and

[1] I am indebted to the Hydrographic Departments of Great Britain as well as the United States for copies of these charts.

[2] On the map the bathymetric lines for 1000, 2000, 3000, and 4000 fathoms, besides being distinguished in the usual way by number of dots, have been made to differ in breadth of line, the deeper being made quite heavy in order to exhibit plainly the positions of the areas without the use of colours. The line for 100 fathoms is, as usual, a simple dotted line. As the bathymetric map herewith published is necessarily small, and none of the ordinary maps of the oceans give either deep-sea soundings or a correct idea of the trends of the oceanic ranges of islands, I state here that the charts of the U.S. Hydrographic Department for the Atlantic, Pacific, Indian, and Arctic Oceans may be purchased of dealers in charts in the larger sea-board cities for 50 cents a sheet and less according to size. (There are several large charts to each ocean.) One of the firms selling them in New York City is that of T. S. and J. D. Negus, 140 Water Street. The British Admiralty have published a map of the Pacific with its soundings on a single sheet, and for the Atlantic and Indian Oceans with part of the Pacific, besides charts of the Antarctic and Arctic seas. The occasional Bulletins from the Hydrographic Department and *Petermann's Mitteilungen* contain nearly all the new data issued for the perfecting of such a chart.

[3] The communication received from the Admiralty Office adds that "Some of Ross's soundings up to 2660 fathoms have been proved correct, and hence the sounding in 68° S., referred to, has been retained on our charts until disproved." "Another sounding obtained by Ross in the Atlantic has had strong doubts thrown upon it by a sounding of 3000 fathoms obtained not very far from its position." See the accompanying map, near latitude 14° S.

therefore, by way of introduction, they are here briefly reviewed.[1] The system of trends in feature-lines takes new significance from a bathymetric map, for the courses are no longer mere trends of islands or emerged mountain peaks; they are the trends of the great mountain ranges themselves; and, in the Pacific, these mountain courses are those of half a hemisphere. Some of the deductions from such a map are briefly as follows:—

(1) Over the Pacific area there are *no* prominent north-and-south, or meridional, courses in its ranges, and none over the Atlantic, except the axial range of relatively shallow water in the South Atlantic. And to this statement it may pertinently be added that there are none in the great ranges of Asia and Europe, excepting the Urals; none in North America; none in South America, excepting a part of those on its west side.

(2) The ranges in the Pacific Ocean have a mean trend of not far from north-west-by-west, which is the course very nearly of the longer diameter of the ocean. One *transverse* range crosses the middle South Pacific—the New Zealand—commencing to the south in New Zealand and the islands south of it, with the course N. 35° E., and continuing through the Kermadec Islands and the Tonga group, the latter trending about N. 22° E., and this is the nearest to north and south in the ocean, except toward its western border.

(3) The oceanic ranges are rarely straight, but, instead, change gradually in trend through a large curve or a series of curves. For example, the chain of the central Pacific becomes, to the westward, north-north-west; and the Aleutian range and others off the Asiatic coast make a series of consecutive curves. Curves are the rule rather than the exception. Moreover, the intersections of crossing ranges, curved or not, are in general nearly rectangular.

(4) Approximate parallelisms exist between the distant ranges or feature-lines; as (1) between the trend of the New Zealand range and that of the east coast of North America; and also that of South America (which is continued across the ocean to Scandinavia); also (2) between the trend of the foot of the New Zealand boot with the Louisiade group and New Guinea farther west, and the mean trend of the islands of the central Pacific both south and north of the equator, and also that of the north shore of South America. These are a few examples out of many to be observed on the map.

(5) The relatively shallow-water area which stretches across the North Atlantic from Scandinavia to Greenland—the Scandinavian plateau, as it may well be called—is continued from these high latitude seas south-westward, in the direction of the axis of the North Atlantic (or parallel nearly to the coast of eastern North America and the opposite coast of Africa), and becomes the "Dolphin Shoal."

It may be a correlate fact in the earth's system of features that a Patagonian plateau stretches out from the Patagonia coast, or from high southern latitudes, in the direction of the longer axis of the Pacific, and embraces the Paumotu and other archipelagos beyond.[2]

The above review of the earth's physiognomy, if accompanied by a survey of the map, may suffice for the main purpose here in view: to illustrate the general truths—that system in the feature-lines is a fact; that the system is world-wide in its scope; and —since these feature-lines have been successively developed with the progress of geological history—that the system had its foundation in the beginning of the earth's genesis and was developed to full completion with its growth.

II. FACTS BEARING ON THE ORIGIN OF THE DEEP-SEA TROUGHS.

In treating of this subject, the facts from the vicinity of volcanic lands that favour a volcanic origin are *first* mentioned; *secondly*, those from similar regions that are not favourable to such an origin; *thirdly*, facts from other regions bearing on the question.

A. *Facts apparently favouring a Volcanic Origin.*

1. The Pacific soundings have made known the existence of two deep-sea depressions, if not a continuous trough, *within forty miles of the Hawaiian Islands;* one situated to the north-east of Oahu, or, north of Molokai, with a depth of 3023 fathoms, or 18,069 feet, and the other east of the east point of Hawaii, 2875 fathoms, or within 750 feet of 18,000 feet. Again, 450 miles north-east of Oahu, there is a trough in the ocean's bottom, over 800 miles long, which runs nearly parallel with the group and has a depth of 3000 to 3540 fathoms; and, as far south, another similar trough of probably greater length has afforded soundings of 3000 to 3100 fathoms. The depths about the more western part of the Hawaiian chain of islands have not yet been ascertained, and hence the limits of the deep areas are not known. Such depths, so close to a line of great volcanic mountains, the loftiest of the mountains not yet extinct, appear as if they might have resulted from a subsidence consequent on the volcanic action.

The subsidence might have taken place (1) either from underminings—which the amount of matter thrown out and now constituting the mountain chain, with its peaks of 20,000 to 30,000 feet above the sea-bottom, shows may be large; or (2) from the gravitational pressure in the earth's crust, about a volcanic region which speculation makes a source of the ascensive force and of the upward rising of the lavas, the subsiding crust following down the liquid surface beneath. In either case the mass of ejected material might be a measure more or less perfectly of the maximum amount of subsidence.

2. In the western part of the North Pacific, at the south end of the volcanic group of the Ladrones off the largest island of the group, Guam, the *Challenger* found a depth of 4475 fathoms, one of the two deepest spots yet known in the Pacific. The situation with reference to the group is like that off the east end of the Hawaiian group.

3. East of Japan and the Kuriles, a region of ranges of volcanoes, there is the longest and deepest trough of the ocean, the length 1800 miles, the depths 4000 to 4650 fathoms; and farther north-east, south of one of the Aleutian Islands, a depth of 4000 fathoms occurs again; and depths of 3100 to 3664 fathoms also still farther east. It is probable that the 4000-line trough continues from the Kurile to this deep spot off the Aleutian volcanic range; and if so, the length of the trough is over 2500 miles. The map is made to suggest its extension still farther eastward; but among the very few soundings made, none below 3664 fathoms have yet been obtained off the more eastern Aleutians.

Other similar facts may be found on the map; and still others may exist which are not now manifest owing to the sinking of oceanic areas and islands. But no cases can be pointed to which are more decisively in favour of volcanic origin.

B. *Facts from the Vicinity of Volcanic Regions apparently not referable to a Volcanic Origin.*

The ocean off the western border of North and South America affords striking examples of the absence of deep troughs from the vicinity of regions eminently volcanic. The South American volcanoes are many and lofty; and still the ocean adjoining is mostly between 2000 and 2700 fathoms in depth; and just south of Valparaiso, it shallows to 1325 fathoms. The only exception yet observed is that of a short trough of 3000 to 3368 fathoms close by the Peruvian shore. It may, however, prove to be a long trough, although certainly stopping short of Valparaiso. The waters, however of the Pacific border of America deepen abruptly compared with those of the Atlantic border; and the significance of this fact deserves consideration.

The facts off Central America are more remarkable than those off the coast to the south. The volcanoes are quite near to the Pacific coast, and still the depths are between 1500 and 2500 fathoms.

The condition is the same off the west coast of North America. Of the two areas of 3000 and over, nearest to the east coast of the North Pacific, one is 600 miles distant in the latitude of San Francisco, and the other is within 10° of the equator and 20° of the coast; both too far away to be a consequence of volcanic action in California, Mexico, or Central America.

[1] This subject of the system in the earth's feature-lines is presented at length, with a map, in my Expedition Geological Report, pp. 11-23 and 414-424; and also more briefly in the *American Journal of Science*, II. ii. 381, 1846.

[2] As parallelisms may have importance that is not now apparent, I draw attention to one between the Mediterranean Sea that divides Europe from Africa, and the West India (or West Mediterranean) sea that divides North from South America. Both have an *eastern*, *middle*, and *western* deep basin. Their depths (see map) in the East Mediterranean, are 2170, 2040, and 1585 fathoms; in the West Mediterranean (the three being the Caribbean, the West Caribbean or Cuban, and the Gulf of Mexico), 2804, 3428, and 2080 fathoms. Further, in each Mediterranean Sea, a shallow-water plateau extends from a prominent point on the south side, northward, to islands between the eastern and middle of the deep basins; one from the north-east angle of Tunis to Sicily, the other from the north-east angle of Honduras to Jamaica and Haiti, the two about the same in range of depth of water. And this last parallelism has its parallels through geological history, even to the Quaternary, when the great Mammals made migrations to the islands in each *from the continent to the south*.

In the North Atlantic the European side has its volcanoes, and has had them since the Silurian era, and yet the non-volcanic North American side of the ocean has far the larger areas of deep water and much greater mean depth. The Azores or Western Islands, which are all volcanic, have depths around them of only 1000 to 2000 fathoms, and no local troughs. Iceland, the land of Hecla, is in still shallower waters, with no evidence of local depressions off its shores. The Canaries are volcanic, but no deep trough is near them.

C. *Facts from Regions not Volcanic which are unfavourable to the idea of a Volcanic Origin.*

1. In the North Pacific, near its centre, the area of 3000 or more fathoms about 35° N.; the two similar but smaller areas toward its eastern border; the areas north of the Carolines in the western part of the ocean; the broad equatorial area about the Phœnix group; the area in the South Pacific in 170° W., east of Chatham Island, and another just south of Australia, are all so situated that no reason is apparent for referring them to a volcanic origin. Some of the areas are in the coral island latitudes, and the supposed volcanic basis of coral islands makes a volcanic origin possible, but their probable size and position appears to favour the idea of origin through some more fundamental cause. The area in the South Pacific, east of Chatham Island, is 450 miles distant from the land. The border of southern Australia, abreast of the deep-sea trough, has no known volcano.

2. *In the Atlantic, away from the West Indies.*—The 3000-fathom areas of the North and South Atlantic—that is, the three in the North Atlantic, the two in the South Atlantic, and the two equatorial, one near the coast of Guinea and the other near that of South America—occupy positions that suggest no relation to volcanic conditions. The Cape Verdes, north of the equator, are partly encircled by one of the deep areas, somewhat like the eastern end of the Hawaiian group; but this bathymetric area appears to be too large to owe its origin directly to volcanic work in the group. The coast of Guinea near the 3000-fathom area has nothing volcanic about it, and the opposite coast of South America, near another, is free from volcanoes.

The only facts in the Atlantic that suggest a volcanic origin are the depression of 2445 fathoms within 40 miles of the west side of the volcanic Cape Verde archipelago, and that of 2060 fathoms within 20 miles of Ascension Island; and a connection is possible.

3. *In and near the West Indies.*—The most remarkable of the depths of the Atlantic area are situated in and near the region of the West Indies, as is well illustrated and discussed by Mr. Alexander Agassiz in his instructive work on the "Three Cruises of the *Blake*." The deepest trough of the ocean, 4561 fathoms, occurs within seventy miles of Porto Rico; and yet this island has no great volcanic mountain, though having basaltic rocks. By the north side of the Bahama belt of coral reefs and islands, for 600 miles, as Mr. Agassiz well illustrates, the depth becomes 2700 to 3000 fathoms within twenty miles of the coast-line, and at one point 2990 within twelve miles, a pitch-down of 1 : 3·5; and nothing suggests a volcanic cause for the abrupt descent. Cuba and Hayti are not volcanic, and look as if they were an extension of Florida, so that no grounds exist for assuming that the Bahamas rest on volcanic summits.

One of the strangest of 3000-fathom troughs is that which commences off the south shore of Eastern Cuba, having there a depth of 3000 to 3180 fathoms. It is within 20 miles of this non-volcanic shore, and nearly three times this distance from Jamaica. No sufficient reason appears at present for pronouncing its origin volcanic. It is continued in a west-by-south direction to a point beyond the meridian of 85° W., or over 700 miles, making it a very long trough, and the depths vary from 2700 to 3428 fathoms. The depression extends on into the Gulf of Honduras, carrying a depth of 2000 fathoms far toward its head, and in a small indentation of the coast it stops; for nothing of it appears in the outline of the Pacific coast or the depths off it, and nothing in the range of volcanic mountains on the coast. Against the three deepest parts of the trough there are, *first*, the Grand Cayman Reef, 20 miles north of a spot 3428 fathoms deep; *second*, banks in 13 and 15 fathoms within 15 miles of a depth of 2982 fathoms; and *third*, Swan Island Reef, 15 miles south of a depth of 3010 fathoms; the first of the three indicating a slope to the bottom of 1 : 5, and the last of 1 : 4·4. Why these greatest depths in the trough, so abrupt in depression, should be on one side of shoals or emerged coral reefs, it is not easy to explain; and the more so that the part of the trough south of Cuba has nothing volcanic near by in the adjoining mountain range, and the fact also that the westernmost end of the trough extends on for 175 miles, and there has a depth of 3048 fathoms, with 2000 fathoms either side and no coral reefs.

D. *Arrangement of the Deep-sea Troughs in the two halves of the Oceans, pointing to some other than a Volcanic Origin.*

The *western* half of the Atlantic and Pacific oceans contains much the larger part of the 3000-fathom areas and all the depths over 4000 fathoms. In the North Atlantic the areas of 3000 and over in the western half, or off the United States, are very large; and the bathymetric line of 2500 fathoms extends westward nearly to the 1000-fathom line. This important feature can be appreciated for both oceans from a look at the map without special explanations.

As a partial consequence of this arrangement, the Pacific, viewed as a whole, may be said to have a westward slope in its bottom, or from the South American coast toward Japan. This westward slope of the bottom exists even in the area between New Zealand and Australia—the ocean in this area being shallow for a long distance out on the east side and deepening to 2500–2700 fathoms close to that non-volcanic land, New South Wales, in eastern Australia. In the Atlantic, the slope is in the direction of its north-east–south-west axis, either side of the Dolphin Shoal, but especially the western side, rather than from east to west, it commencing in the Scandinavian plateau and ending in the great depths adjoining the West Indies.

Owing to the system in the Atlantic topography, the Dolphin Shoal—the site of the *Atlantis* of ancient and modern fable—is really an appendage to the eastern continent, that is to Europe, and is shut off by wide abyssal seas from the lands to the west that have been supposed to need its gravel for rock-making.

But the view that the west half of an oceanic basin is always the deepest becomes checked by finding in the Indian Ocean that the only areas that are 3000 fathoms deep or over are in the *eastern* part of the ocean and off the north-west coast of Australia, and near western Java and Sumatra. The greatest depths in its western half or toward Africa, are 2400 to 2600 fathoms.[1]

III. CONCLUSIONS.

1. The facts reviewed lead far away from the idea that volcanic action has been predominant in determining the position of the deep-sea troughs. It has probably occasioned some deep depressions within a score or two of miles of the centre of activity, but beyond this the great depths have probably had some other origin.

2. It is further evident that the deep-sea troughs are not a result of superficial causes of trough-making. Erosion over the ocean's bottom cannot excavate isolated troughs. The coldest water of the ocean stands in the deep holes or troughs instead of running, as the reader of Agassiz's volume has learned.

The superficial operation of weighting the earth's crust with sediment, or with coral or other organic-made limestone, and filling the depressions as fast as made, much appealed to in explanations of subsidence, has not produced the troughs; for filled depressions are not the kind under consideration. Moreover, the areas are out of the reach of continental sediments and too large and deep to come within the range of possibilities of organic sedimentation or accumulation. The existence of the troughs is sufficient proof of this. The deep troughs of the West Indian and adjoining seas are in a region of abundant pelagic and sea-border life, and yet the marvellous depths exist. And the depths of the open oceans are no less without explanation. Those close by the Bahamas, extending down to 16,000 and 18,000 feet, are evidence of great subsidence from some cause; and the coral reefs for some reason have manifestly kept themselves at the surface in spite of it.[2]

3. If superficially acting causes are insufficient, we are led to look deeper, to the sources of the earth's energies, or its interior

[1] In the Arctic seas, going north from the Scandinavian plateau, the water deepens north of the latitude of Iceland, between Greenland and Spitzbergen, to 2000 fathoms, and farther north to 2650 fathoms, in the latitude nearly of Greenwich; and it is probable that the 2000-fathom area extends over the region of the North Pole. The continents of Europe (with Asia probably) and North America are proved by the shallow soundings over the adjoining Arctic seas and the islands or emerged land, to extend to about 82½° N., which is about 450 miles from the Pole.

[2] The migrations from South America alluded to in note [2] on page 358, proving an elevation of 2000 feet to make it possible, prove also that a large part of the West India seas *afterward* suffered subsidence in the Quaternary. How far the Bahama and Florida region participated in the subsidence is not known. That it did not participate in it has not been proved.

agencies of development, to which the comprehensive system in its structure and physiognomy points. Whatever there is of system in the greater feature-lines, whether marked in troughs or in mountain chains, or island ranges, must come primarily from systematic work within. The work may have been manifested in long lines of flexures or fractures as steps in the process, but the conditions which gave directions to the lines left them subject to local causes of variation, and between the two agencies, the resulting physiognomy has been evolved.

We have from the Pacific area one observation of a volcanic nature bearing on the comprehensiveness of the system of feature lines in the oceans, and although I have already referred to it, I here reproduce the facts for use in this place.

If the ranges of volcanic islands were, in their origin, lines of fissures as a result of comprehensive movements, the lines should continue to be the courses of planes of weakness in the earth's crust. The New Zealand line, including the Kermadec Islands and the Tongan group, has been pointed to as one of these lines, and one of great prominence, since it is the chief north-eastward range of the broad Pacific, and nearly axial to the ocean. The series of volcanoes along the axis of New Zealand is in the same line. It was noticed, at the Tarawera eruption of 1883, that *four or five days after* the outbreak, and three after it had subsided, White Island, in the Bay of Plenty, at the north end of the New Zealand series, became unusually active; and *two months later* there was a violent eruption in the Tonga group, on the Island of Niuafou. The close relation in time of the latter to the New Zealand eruption is referred to by Mr. C. Trotter, in NATURE of December 7, 1886.[1] May it not be that these disturbances were due to a slight shifting or movement along a series of old planes of fractures, taking place successively from south to north; and, hence, that even now changes of level may take place through the same comprehensive cause that determined the existence of the earth's feature lines? Owing to the long distance of the Tonga group from New Zealand an affirmative reply to the question cannot be positively made. But there is probability enough to give great interest to this branch of geological enquiry. JAMES D. DANA.

[1] *American Journal of Science*, III., xxxiii., 311.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, July 28.—M. Hermite in the chair.—Aquatic locomotion studied by photo-chronography, by M. Marey. The author has made similar investigations on animal locomotion to those of Mr. Muybridge, but with different apparatus. A single camera, the sensitive plate of which takes the form of an endless band moving past the focus of the lens, has been used in the investigations, and appears to possess many advantages over the multiple camera system. The contractions and dilatations of the body of the medusa, the undulations of the lateral fins of the ray, and the rapid movements of the dorsal fin of the *Hippocampus* (sea-horse), have all been analyzed, and in the zoëtrope the successive photographs appear to have reproduced the motions to perfection.—Observations, orbit, and ephemeris of the comet discovered by M. Coggia (*b* 1890) at Marseilles Observatory, by M. Stephan.—On the observation of the annular eclipse of the sun of June 17, by M. A. de la Baume Pluvinel. A detailed description of the instruments employed by the author for his observations in Canea (Island of Crete) is given. As previously noted (NATURE, July 10), the results give further support to the view that the oxygen absorption bands in the solar spectrum are of telluric origin.—Observations of the minor planet recently discovered by M. Charlois (294), made with the *coudé* equatorial and the Foucault telescope at Algiers Observatory, by MM. Rambaud and Sy. Some observations of position and comparison stars are given.—Observations of Coggia's comet, made with the great equatorial of Bordeaux Observatory, by MM. Picart and Courty.—Observations of the same comet made at Paris Observatory, by Mdlle. D. Klumpke.—On a new method of exposition of the theory of theta functions, and on an elementary theorem relative to hyperelliptic functions of the first dimension, by M. F. Caspary. It is shown that the fifteen hyperelliptic functions of the first dimension are proportional to the fifteen elements of an orthogonal system.—Earthquakes in Madagascar, by M. R. P. Colin, Director of the Antananarivo Observatory. The five earth-tremors observed this year appear to have had an influence on the azimuth error of the transit instrument.—On the water of crystallization of neutral sulphate of alumina; analysis of a natural product, by M. P. Marguerite-Delacharlonny. The analysis of two samples of definitely crystallized natural sulphate of alumina from Bolivia supports the author's previous conclusion that its formula should be written with sixteen instead of eighteen molecules of water of crystallization.—On the optical rotatory power of camphor in solution in various oils, by M. P. Chabot. The author finds that the rotation produced by the solutions is sensibly proportional to their strengths, and that, after allowing for the slight rotation due to the oil, the calculated molecular rotatory power of camphor is practically constant.—On the malonates of lithia and on the malonate of silver, by M. G. Massol. Some experiments on the heats of formation are given.—Researches on the optical dispersion of organic compounds; fatty acids, by MM. Ph. Barbier and L. Roux. The authors have examined the normal fatty acids from formic to pelargonic as well as isobutyric and isovaleric acids, and find that the specific dispersive powers increase with the molecular complexity, and that those of isomeric acids are practically equal, though the normal acids have slightly the higher value.—On the presence of furfural in commercial alcohols, by M. L. Lindet.—Contribution to the study of artificial musk, by M. Albert Baur.—Mode of action of bacterial secretions on the vasomotor nervous system; connection between these phenomena and diapedesis, by MM. A. Charrin and E. Gley.—Does hæmoglobin exist in the blood as a homogeneous substance?, by M. Christian Bohr.—On the identity of structure of the central nervous system of Pelecypoda and other Mollusca, by M. Paul Pelseneer.—On the bathymetric distribution of the deep-sea Brachiopods collected in the *Travailleur* and *Talisman* expeditions, by MM. P. Fischer and D. P. Œhlert.—On the position in the plant of the compounds which produce the sulphuretted essential oils of the Cruciferæ, by M. Léon Guignard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Theory of Light: T. Preston (Macmillan).—Weather Forecasting of the British Isles: Captain H. Toynbee (Stanford).—Psychology: M. Maher (Longmans).—Geometrical Conics, Part 1: Rev. J. J. Milne and R. F. Davis (Macmillan).—Text-book of Mechanics: T. W. Wright (New York, Van Nostrand).—Sap: Does it rise from the Roots? J. A. Reeves (Kenning).—The History of Federal and State Aid to Higher Education in the United States: Dr. F. W. Blackmar (Washington).—Proceedings of the Department of Superintendence of the National Educational Association at its Meeting in Washington, March 6 to 8, 1889 (Washington).

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THURSDAY, AUGUST 14, 1890.

THE INCOME-TAX AND THE PROMOTION OF SCIENCE.

THE case of the Commissioners of Inland Revenue *v.* Forrest (the latter representing the Institution of Civil Engineers), which was finally decided on the 1st inst. by the House of Lords, is of great importance to all scientific corporations, associations, and institutions in this country, and, incidentally, the judgments cannot fail to interest, and possibly also to amuse, men of science, because it became necessary for their Lordships to consider what is science, or, rather, what the Legislature meant by the word science in a particular statute. Shorn of all technicality, the question was whether the Institution of Civil Engineers was liable to pay income-tax under the Revenue Act of 1885, section 11 of which was framed with the object of imposing a duty of 5 per cent. on the yearly value, income, or profits of bodies which escape probate, legacy, and succession duties, inasmuch as they never die and have no legal heirs or successors. The net was thrown with the object of catching trading corporations, companies, and associations, and compelling them to pay, in the shape of an annual impost, an equivalent for the various death duties levied on private individuals. The Act imposing this tax, however, exempted different classes of associations, and notably in sub-section 3 of section 11 it exempted all property the income or profit of which is applied for religious or charitable purposes, "or for the promotion of education, literature, science, or the fine arts." The whole question therefore resolved itself into this: Is the Institution of Civil Engineers an association "for the promotion of science"? The Commissioners thought it was not, in the sense used in the Act; Lord Coleridge and Mr. Justice (now Lord) Field sitting in one Court agreed with the Commissioners; Lord Justice Lopes in the Court of Appeal, and the Lord Chancellor in the House of Lords, were of the same opinion; but Lord Esher and Lord Justice Fry in the Court of Appeal, and Lord Watson and Lord Macnaghten in the House of Lords, held that the Institution was one for the promotion of science, and therefore exempt from the tax. The Institution therefore had a majority of the judges in the Court of Appeal and in the House of Lords, and it is now the law of England, until the Legislature chooses to alter it, that the Institution and all similar associations and bodies are exempt from this tax. Science, and, indeed, literature and the fine arts as well, owe a debt of gratitude to the Institution for its sturdy stand against the demand for payment. Although it is successful, its costs, over and above what it will receive from the Crown as the losing party, would, if invested, probably yield an income sufficient to satisfy the demand made upon it; by continuing the fight it has been the means of relieving the revenues of every association of the kind in the country from a burden of 5 per cent. per annum, an impost which in some cases would be intolerable, and would perhaps lead to the extinction of many struggling associations which are worthy of more support than they receive. In a sense, all science is relieved of a tax, and this it owes to the Institution of Civil Engineers.

We have said that the question turned on the meaning to be attached to the phrase "promotion of science," and ultimately to the word "science." The consideration of this question was complicated by the circumstance that in 1843 an Act was passed for dealing with the application of local rating (the Act of 1885, which was in question in this case, dealt wholly with Imperial taxation) to "exclusively" scientific and literary institutions supported wholly or in part by voluntary contributions. Under this Act it has been decided, for example, that the Zoological Society and the Russell Institution are liable to local rates, and it was against decisions such as these, and the instinct of judges to seek for a precedent, that the Institution of Civil Engineers had to fight in the present instance. Lord Macnaghten, however, boldly threw over the Act of 1843 and the decisions under it altogether, and refused to regard them as throwing any light on the Act of 1885. It referred, he said, merely to local rates, exemption from which is an invidious distinction, and throws a burden on everyone in the neighbourhood; while the present case being one of Imperial taxation, the range of exemption is far more extensive, and the conditions far more liberal.

The previous statute, and all the decisions under it, being thus disposed of, the judges were deprived of precedents, and had to answer for themselves what was science in the intention of the Legislature in 1885. Most intelligent people have a satisfactory working definition of the word; but it evidently perplexed the keen and experienced legal intellects of the judges in the House of Lords. The Lord Chancellor thought it could not, in this place, be equivalent to knowledge, because this would exempt almost every institution in the country, but that it did refer to science generally, and not to any particular branch of it. The Institution was, he argued, established for the benefit and interest of civil engineers, and not directly (though, no doubt, indirectly) for the advantage of the whole community. "I think a member of it makes a very good bargain for himself in becoming a member of it," and hence he did not regard it as exempt from taxation. Lord Watson took quite a different view, without going largely into questions of definition. It was indisputable, he said, that there was a science of civil engineering, that its development is of the utmost consequence to the national interests, that the labours of the Institution are of value to the profession at large, and constitute a substantial addition to the sum of human knowledge, and that it would be difficult to say what more effective measures could be adopted for the promotion of engineering science than those of the Institution. He found, therefore, that the latter applied its income, not to the professional ends of individuals, but for "the promotion of science," and that it was entitled to the exemption. Lord Macnaghten faced the question of the meaning of the word "science" in the Act:—

> "I see no reason why it should be confined to pure or speculative science. The expression plainly includes applied science, and it was intended, I think, to denote a particular branch of science, as well as universal science or science generally."

This being his view, Lord Macnaghten, like Lord Watson, found no difficulty in arriving at the conclusion

that the Institution of Civil Engineers did in fact promote science:—

"Substantially, as it seems to me, the whole of the Society's income is applied to the promotion of science. My Lords, I cannot conceive in what better way the promotion of mechanical science, and in particular of those branches of mechanical science which lie within the province of civil engineering, could be effected. I cannot doubt that by means of the discussions on the papers read at the ordinary meetings of the Society much new light has been thrown on scientific questions, and much knowledge, which would otherwise have perished, has been preserved. I see no trace of a selfish or illiberal spirit in the proceedings of the Society, nor do I find anything to lead me to suppose that its property and income are applied otherwise than *boná fide* for the promotion of science. The action of the Society may incidentally benefit the profession to which its members belong—I have no doubt that is so—but I agree with the Master of the Rolls in thinking that 'that which this Society does is something higher and larger than the mere education of students and others for the profession of civil engineers.'"

The admirable definition of the object of the Institution, embodied in the charter of 1828, was stated in the course of one of the judgments to have been drafted by Thomas Tredgold. The Institution, it says, is established for the purpose of

"the general advancement of mechanical science, and more particularly for promoting the acquisition of that species of knowledge which constitutes the profession of a civil engineer, being the art of directing the great sources of power in nature for the use and convenience of man, as the means of production and of traffic in States both for external and internal trade, as applied in the construction of roads, bridges, aqueducts, canals, river navigation, and docks, for internal intercourse and exchange, and in the construction of ports, harbours, moles, breakwaters, and lighthouses, and in the art of navigation by artificial power for the purposes of commerce, and in the construction and adaptation of machinery, and in the drainage of cities and towns."

It is only right to say, in conclusion, that the utility of the work done by the Institution was admitted in the warmest manner by those judges who found themselves compelled to decide against its claim to exemption, now happily established.

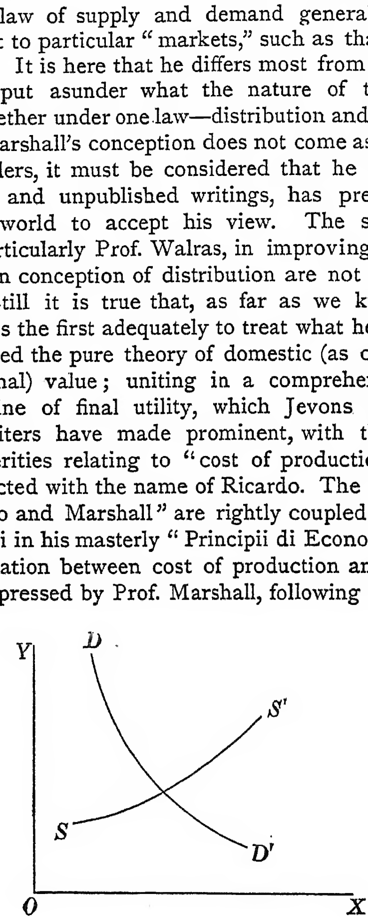
PRINCIPLES OF ECONOMICS.

Principles of Economics. Vol. I. By Prof. Alfred Marshall. (London: Macmillan and Co., 1890.)

ECONOMICS admit of being reduced to principles more than other sciences dealing with human actions, for the reason which Prof. Marshall has thus expressed: "Wide as are the interests of which the economist takes account when applying his doctrines to practice, the centre of his work is a body of systematic reasoning as to the quantities of measurable motives." These measurable motives are not necessarily self-interested: "The range of economic measurement may gradually extend to much philanthropic action." Even now the supply of labour and of capital is largely due to the motive of family affection. The uniformities of action resulting from such measurable motives may be regarded as the laws of motion in what Jevons called *the mechanics of industry*—a science which Prof. Marshall has cultivated with more success than any of his predecessors, owing to an unexampled combination of antithetical powers, the comprehensive grasp of mathematical reasoning, and the careful handling in detail of the observed facts.

As in physical mechanics innumerable conditions may be comprehended under the principle of virtual velocity, so also there is a unifying principle in the mechanics of industry. "Most economic problems have a kernel relating to the equilibrium of demand and supply." It is the peculiar merit of Prof. Marshall's arrangement to treat the law of supply and demand generally, before applying it to particular "markets," such as that relating to labour. It is here that he differs most from Mill, who seems to put asunder what the nature of things has joined together under one law—distribution and exchange. If Prof. Marshall's conception does not come as a surprise to his readers, it must be considered that he himself, in published and unpublished writings, has prepared the scientific world to accept his view. The services of others, particularly Prof. Walras, in improving upon the old wooden conception of distribution are not to be forgotten. Still it is true that, as far as we know, Prof. Marshall is the first adequately to treat what he has elsewhere called the pure theory of domestic (as opposed to international) value; uniting in a comprehensive view the doctrine of final utility, which Jevons and other recent writers have made prominent, with the equally eternal verities relating to "cost of production," which are connected with the name of Ricardo. The "theorems of Ricardo and Marshall" are rightly coupled by Signor Pantaleoni in his masterly "Principii di Economia Pura."

The relation between cost of production and demand is thus expressed by Prof. Marshall, following Cournot.



In the annexed diagram the abscissa indicates the amount of a product, and the ordinate the price thereof. DD' is the demand curve, representing the quantity of the product which is demanded at each price; SS' the supply curve, representing the quantity which is offered at each price. The intersection of these curves determines the equilibrium of the *market*—a generic term used in a wide sense, covering the temporary equilibrium of a fish-market and those slow processes of competition which it requires a generation to work out.

From this point of view is apparent the inaccuracy of those who describe value as altogether an affair of final utility, and speak of Ricardo as being "preposterous" in the classic sense of putting the cart before the horse. To use our own illustration, these economists might be compared to a physicist who should insist that in the determination of the position at which a balloon reaches

equilibrium, the buoyant gas plays a more important part than the heavy car. To be sure balloons could not go up without gas; whereas they might, and sometimes do, without a car. Still, from a mathematical point of view, we submit, it is legitimate to attribute to the positive and negative forces a "fundamental symmetry"—as Prof. Marshall characterizes the equilibrating motives towards utility and from *disutility*. By parity of reasoning they also are to be condemned who, neglecting final utility, worship only cost of production. But it may well be doubed whether this form of what we may call the monophysite heresy in regard to the doctrine of value is attributable in a serious degree to Ricardo. It is tenable that

"the older economists seem to have been rightly guided by their intuition when they silently determined that the forces of supply were those the study of which was the more urgent and involved the greater difficulty."

The theory of cost of production would be easy if all economic action were as simple as in the case of one who goes on picking and eating blackberries until the labour of picking just compensates the pleasure of eating. The concrete case is greatly complicated by the element of *time*. Under cost of production we must include the less direct efforts and sacrifices, such as that of the parent who, vicariously competing in the labour market, supplies an educated employer or artisan to that occupation where there appears to be the best opening. Before the education is completed perhaps the opening has ceased to be the best. The normal tendency to equilibrium is thus ever interrupted by the introduction of some new condition:—

"There is a constant tendency of the surface of the sea towards a position of rest, but the moon and sun are always shifting their places and always therefore changing the conditions by which the equilibrium of the sea is governed; and meanwhile there are ceaseless currents of the raging winds; the surface is always tending towards a position of normal equilibrium, but never attains it."

In this troubled scene everything is in flux, and subject to the theory of fluxions:—

"The amount of the commodity and its price, the amounts of the several factors or agents of production used in making it and their prices—all these elements *mutually determine one another* [we italicize words which convey a lesson which has never before been taught thoroughly], and if an external cause should alter any one of them, the effect of the disturbance extends to all the others."

If there is any of the economic variables of which it may be said that it is determined without determining, it might be the old Ricardian "inherent properties" of land, about which Prof. Marshall has much that is new to say. As for the quasi-rents which more recent theory has evolved, they are all affected with the fallacy which Prof. Marshall's scientific method is particularly adapted to guard against—the treating as constant quantities which are variable. The "margin" from which the remuneration in any skilled occupation is measured is itself a variable, varying with the remuneration; because the supply of competitors is dependent upon the prospect held out by the great prizes in that occupation. The apologist of the existing economic *régime* who defends the profits of the successful employer as being a rent of ability, the Socialist who attacks the interest of capital as being a rent of opportunity, are alike building their insecure constructions upon the sands of a shifting coast-line.

We are prevented by the narrowness of our limits from exhibiting the important results obtained by the full treatment of the subject to which we have barely adverted—namely, the *simultaneous* determination both of the relative value of products, and the remuneration of producers, in a *régime* of free competition. We must hasten on to observe that the same methods of abstract reasoning are applicable, *mutatis mutandis*, to a *régime* of monopoly. This case is important, not only for itself, on account of the prevalence of trusts and monopolies, but also by reason of the analogy between governmental and monopolistic action. Prof. Marshall, by original methods, deduces the startling conclusion

"that it might even be for the advantage of the community that the Government should levy taxes on commodities which obey the law of diminishing return, and spend part of the proceeds on bounties to commodities which obey the law of increasing return."

This reasoning is, of course, very abstract; abstracting the indirect evils which governmental interference may produce. But it at least suffices to destroy the *a priori* presumptions in favour of "economic harmonies" and unqualified *laissez faire*. Prof. Marshall reaches these and other important conclusions by estimating the "consumers' rent"—that is, the advantage which consumers derive from a fall of price. In connection with this subject we should advert to his beautiful theory of the elasticity of demand. The more elastic or expansive demand is, the greater is the increase of consumers' rent due to a given fall of price.

We should like to dwell upon the practical importance of these conceptions. But it is impossible here to analyze a work almost every page of which presents a new idea. We must be content with indicating methods as distinguished from particular theories. The mathematical method appears to be established in its proper position by the precept and example of Prof. Marshall:—

"Our observations of nature, in the moral as in the physical world, relate not so much to aggregate quantities as to increments of quantities. . . . It is not easy to get a clear full view of continuity in this aspect without the aid either of mathematical symbols or diagrams." . . .

Prof. Marshall expresses some preference for diagrams:—

"Experience seems to show that they give a firmer grasp of many important principles than can be got without their aid, and that there are many problems of pure theory which no one who has once learnt to use diagrams will willingly handle in any other way."

Developing a metaphor suggested by our author, we might compare these mechanical aids to reason to the machinery employed in material production. Appliances useful to one producer will not be equally so to another. There is what Prof. Marshall calls the "law of substitution," according to which each producer selects the expedients most serviceable in his own case. Usefulness will depend much on familiarity. "It seems doubtful whether anyone spends his time well in reading lengthy translations of economic doctrines into mathematics that have not been made by himself."

By way of illustrating this character of intellectual

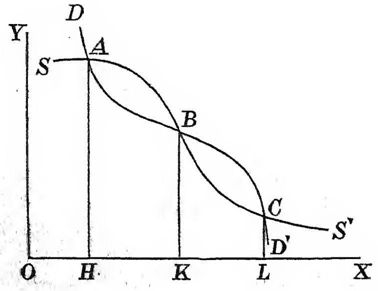
machinery, we shall advert to some passages which seem to us to contain things which some would rather have expressed otherwise. In the first note of his "Mathematical Appendix," Prof. Marshall, referring to the weakened motive power of distant or deferred pleasures, thus writes:—

"Let h be a pleasure of which the probability is p, and which will occur, if at all, at time t. Let r be the rate of interest per unit which must be added to present pleasures before comparing them to future, and let $R = 1 + r$; then the present value of the pleasure is phR^{-t}."

Should it not be more clearly expressed here, or elsewhere, that this formula holds only of marginal utility, and that it is not a general psychological truth irrespective of conditions imposed by a money market? For instance, I anticipate a series of pleasant hours extended over several weeks during which I shall be occupied in mastering this stupendous work. But I do not observe that the anticipated pleasure of the third week differs from that of the first according to an exponential law of variation.

Another verbal modification is suggested by the frequent use of the "law of substitution"; which, as above intimated, imports that producers will, as a rule, substitute the less for the more expensive methods of production. Might it not be well more often to substitute the simpler statement that the producer will seek to maximize his net advantages, considered as a function of different variables, *e.g.* labour, capital borrowed, &c.? From the principle that the partial differential of this function with respect to each of the variables is equated to zero follow, more easily perhaps than by verbal exposition, propositions of the form that "wages tend to equal the net produce of the worker's labour" (pp. 547-48). No doubt it is convenient to have a term which, as we understand, covers two distinguishable cases: where the maximum of advantage is pursued by varying the variable, or by discontinuously passing from one function to another. Indeed, this is a distinction on which Prof. Marshall, true to his motto, *Natura nil facit per saltum*, has, probably for good reasons, not insisted as much as might have been expected.

The condition above mentioned, that the first term of variation should be equated to zero, may of course indicate a minimum, as well as maximum, of utility. Prof. Marshall, following the analogy of physics, attributes to



a minimum the property of equilibrium. For example, in the case represented by the annexed figure, which corresponds to the author's Fig. 20 (p. 424)—SS′ and DD′ being, as above explained, the supply and demand curves —Prof. Marshall says, "H and L are points of stable equilibrium, and K is a point of unstable equilibrium." This interpretation may appear doubtful, when we consider that the supply curve, when *descending*, is the locus of *minimum advantage* for the producer. At any assigned price, *e.g.* SH or BK, this locus represents the very worst arrangement for the producer, the very bottom of the trough, where he cannot, even theoretically, be supposed content to stay. If this view be accepted, some doubt will be thrown on the "theory of multiple positions of equilibrium" (*ibid.*). A solution of these little difficulties, consistent with the author's conclusions, will probably be found by those who follow out the hints afforded by his pregnant notes.

A comparison with the eminent mathematical economists Messrs. Auspitz and Lieben suggests one more scruple. Those theorists regard the demand curve as the *envelope* of a series of discontinuous curves, each of the sort contemplated by Prof. Marshall, corresponding to different scales of living. This conception, if accepted as important, might have some bearing on the theory of "consumers' rent."

But in dwelling on such technical points we should run the risk of conveying an unfair impression of the worth and accuracy of Prof. Marshall's work. The theoretical subtleties about which a difference of opinion is possible "have a very narrow range of practical bearing." Prof. Marshall is the first to admit of his theory, "when pushed to its more remote and intricate logical consequences, especially those connected with multiple position of equilibrium, that it slips away from the conditions of real life, and soon ceases to be of much service in dealing with practical problems." Besides, this is a subject on which, as Disraeli said, the author is much more likely to be right than the critic. In this sort of mixed mathematics the authority of one who is, above all others, conversant with both ingredients of the mixture is almost supreme. He, of all mathematical economists, has best complied with his own maxim that the economist, while he employs "systematic reasoning as to the quantities of measurable motives, ... must never lose sight of the real issues of life; and these are all, with scarcely any important exceptions, affected more or less by motives that are not measurable."

Of the two parts of the economist's work we have here dwelt somewhat exclusively on that which best admits of being viewed synoptically, the more abstract side. We must be content with recording, without illustrating, the judgment that the moral and mathematical parts of Prof. Marshall's work are on a level of excellence. He not only applies the differential calculus to measure increments such as "a shilling's worth of happiness," but he also brings a higher faculty to judge of goods which cannot be measured by money, such as "the fulness and nobility of human life," "a pure heart, and a love towards God and man." He renders to the queen of the sciences the things which belong to her province, and to the spiritual side of our nature things which transcend man's power of calculation.

F. Y. E.

SADI CARNOT'S ESSAY.

Reflexions on the Motive Power of Heat, &c. From the original French of N.-L.-S. Carnot. Edited by R. H. Thurston, M.A., LL.D., Dr. Engr., Director of Sibley College, Cornell University; "Officier de l'Instruction Publique de France"; etc., etc., etc. (London: Macmillan and Co., 1890.)

NE soyons pas exigeants: la perfection est si rare! This is one of the rules laid down by Sadi Carnot for his own guidance:—and we will endeavour, as far as possible, to give his present Editor and Translator the benefit of it. They need it sadly.

There is no Press-mark on the book before us, but it bears internal evidence of having been printed in the United States. Surely there are few, if any, British printers who, at the end of a line, would divide words into such startling fragments as knowl-edge, quan-tity, uncer-tainties, transfor-mation, mecha-nism, hypothe-sis, mo-tive, &c., &c.!

The book is (described as) a "Translation of the famous work of Carnot." It is made from the Reprint of 1878, to which Carnot's surviving brother had added a slight but very interesting biographical sketch of the Author, as well as some extremely important excerpts from his unpublished MS. These additions are translated also. An exceedingly inconvenient arrangement, the separation of the longer foot-notes from the text of Carnot's Essay and their collection at the end of the book, is explained as "*simply a matter of convenience in book-making.*" We presume, though it is not stated, that the quite unnecessary reprinting of Sir W. Thomson's paper, on Carnot's Theory, is also a simple matter of book-making!

The book is prefaced by a *Publisher's Note*, a *Note by the Editor*, and an Essay (also by the Editor) on *The Work of Sadi Carnot*. We forbear to comment on the first two of these. On the third we would make two remarks:—

(1) It is somewhat difficult for us who have lost so recently (and from our little island alone) men like Faraday, Joule, and Clerk-Maxwell, to feel the full justice of the statement that Sadi Carnot was perhaps "*the greatest genius, in the department of physical science at least, that this century has produced.*" Exaggeration like this leads the reader to doubt the judicial competence of the man who employs it. We yield to none in our estimation of the value and originality of Carnot's work:—but such feelings must not blind us to the relative merits of others.

(2) Our opinion of the competence of the Editor is not enhanced by his informing us that at eighteen Hamilton "conceived" Quaternions (he means, presumably, the *Characteristic Function*, a totally different thing); nor by his even more striking novelties in scientific history.

As to the Translation itself, two questions arise. Was it necessary, and is it satisfactorily carried out? We have much doubt as to the propriety of translating *any* scientific work from French, German, Italian, or Latin, into English. If a man cannot read it in the original, his ignorance (all but criminal) should be punished. But if the propriety of translating at all is doubtful, the possibility of procuring a really adequate translation is much more doubtful. It may be confidently laid down, as an axiom, that no adequate translation of a really scientific work can be made except by a man whose knowledge of the subject is at least nearly on a par with that of the Author. Such men are always scarce, and can usually employ their time more profitably than in reproducing, in a different idiom, the thoughts of another.

But if a translation must be made, accuracy is essential. Change of idiom is inevitable, change of meaning (however slight) intolerable. Let us see how the present Translator stands in this respect. The task before him was a difficult one, for Carnot's reasoning is in several places somewhat delicate; and in one or two places a little obscure. Failure was therefore *à priori* more probable than success; and, while even complete success was not likely to be of much use to any one, failure was certain to make the result misleading:—*i.e.* a great deal worse than useless.

One of the first passages which we chanced to read, on opening the book (p. 21), runs thus:—

"Scarcely a year had passed when the proscription, *which included* the Director, *obliged him to give up his life, or at least his liberty, to* the conspirators of fructidor. . . . (Our mother) *fled to* St. Omer, *with* her family, while her husband *was exiled* to Switzerland, then to Germany."

Compare the words we have italicised with the corresponding ones in the original (given below):—and then judge of the fitness of the perpetrator for the translation of a work of real difficulty and of particular nicety of reasoning.

"Une année à peine s'était écoulée quand la proscription *vint frapper* le Directeur, *obligé de dérober sa vie, tout au moins sa liberté, aux* conspirateurs de fructidor. . . . (Notre mère) *se réfugia* à Saint-Omer, *dans* sa famille, tandis que son mari *s'exilait* en Suisse, puis en Allemagne."

There is more than one first-class blunder for every single line in the passage translated!

On p. 26 we read:—

". . . . for his name . . . . was henceforth the cause of his advancement (*sic*) being long delayed."

Who, attempting to put this bad English back again into French, could possibly hope to reproduce the original? It runs thus:—

". . . . car son nom . . . . devait suffire pour que désormais il n'attendît son avancement que de la longeur du temps."

The word "Anvers," which occurs more than once, is not translated at all; while for "plusieurs places fortes" we find (p. 26) the extraordinary substitute (we cannot call it an *equivalent*) "many *trying* places"!

After these experiences we might have dispensed with any further examination of the book. But we felt bound to examine at least a part of the translation of the Essay. We selected as a first test a well-known passage, in which Carnot elegantly meets a supposed objection to his reasoning. The original is as follows:—

". . . la quantité de chaleur nécessaire pour reporter le liquide à sa température première sera aussi infiniment petite et négligeable relativement à celle qui est néces-

saire pour donner naissance à la vapeur, quantité toujours finie."

The meaning is absolutely clear, the contrast being between an infinitesimal, and an essentially *finite*, quantity of heat. What sort of notion of Carnot's reasoning can he have who *translates* the passage as below (p. 59)?

"The quantity of heat necessary to raise the liquid to its former temperature will be also indefinitely small and unimportant relatively to that which is necessary to produce steam—a quantity always *limited*."

The sting is, of course, in the tail:—its proper place. But this one word suffices to destroy the entire argument.

In the translation of the foot-note (preparatory to the discussion of the air-engine) where Carnot gives experimental facts as to the temperature-effects of condensation and rarefaction of gases, we have, among other blunders, a really amusing one. Carnot says:—

". . . l'air qui vient toucher immédiatement la *boule* du thermomètre reprend peut-être par son choc contre cette *boule*, ou plutôt par l'effet du détour qu'il est forcé de prendre à sa rencontre, une densité &c."

The translator would almost seem to have thought that a game at Bowls is here alluded to; for he gives the passage in the form:—

"The air which has just touched the *bowl* of the thermometer possibly takes again by its collision with this *bowl*, or rather by the effect of the *détour* which it is forced to make by its *rencounter*, a density &c."

Since so much of this passage has been left untranslated, it is to be regretted that the whole sentence (and, for that matter, the whole Essay) has not been left in its own strikingly original and well-chosen language.

Many years ago we met with a book something like this one. The writer was translating from Laplace, and rendered the passage

"Si l'on prend, pour unité de temps, la seconde décimale ou la cent-millième partie du jour moyen . . ."

in the following exquisite fashion:—

"If we take the *second decimal*, or the $\frac{1}{100000}$ of the mean day as the unity of time . . ."

This is perhaps finer than anything in Mr. Thurston's translation, but he occasionally rises nearly to its level.

We conclude, as we commenced, with a maxim of Carnot's:—

De l'indulgence, de l'indulgence!

P. G. T.

TRIASSIC FISHES AND PLANTS.

Fossil Fishes and Fossil Plants of the Triassic Rocks of New Jersey and the Connecticut Valley. By J. S. Newberry. (Washington: Government Printing Office, 1888.)

THE fourteenth of the splendid series of monographs issued, and so liberally distributed, by the U.S Geological Survey, is by Prof. Newberry, and deals with the fossil fishes and plants of the east coast Triassic areas known as the Palisades and the Connecticut Valley. Their red shales, sandstones, and conglomerates occur for the most part in narrow basins parallel to the coast or coast ranges, intersected by sheets and dykes of diabase, and average about 5000 feet in thickness. In a very few spots the almost barren shales are charged with carbonaceous matter, and in these plant and fish remains have been met with. The two areas are separated by the wide Hudson Valley tract of older rocks, and are distinguished by all the Palisade beds dipping at an angle of 3° to 15° west, while the Connecticut beds dip as uniformly to the east. Various theories accounting for their deposition are discussed, but the simplest would be to regard them as local deposits of a flat, shallow, sandy, thoroughly sheltered coast-line, subjected to heavy tides. With gradual and intermittent subsidence, and consequent continued encroachment on the land, most extensive beds of varying fineness might be formed. A dip, such as that observed, would ensue, as the beds passed successively under low-water mark, and fell under pressure of the sea into the ordinary slopes of a shelving shore. Sun cracks, ripple marks, and footprints would be formed in each bed in the belt exposed between the high-water marks of neap and spring tides. That the deposits originally swarmed with prey is evident from the footprints of nearly 100 varieties of animals, only a part of which were perhaps amphibious, which made them their promenade. Almost every trace, however, of such organisms as mollusca, annelids, crustacea, and plants, have disappeared.

The second part of the memoir, relating to the fossil fishes, occupies about two-thirds of the volume, and commences with some preliminary remarks on the gradual discovery of the fauna under consideration. Some of these fishes were among the first fossils to attract the attention of American geologists, and two species were figured and described by Agassiz in his "Poissons Fossiles." To Messrs. W. C. and J. H. Redfield, however, palæontology is mainly indebted for the knowledge of the fauna previous to the researches of Dr. Newberry; and their original collection, now in the Yale College Museum, has furnished much of the material for the present memoir. Dr. Newberry himself undertook excavations at Boonton, N.J., in 1866, thus obtaining a large series of specimens for the Columbia College, New York; and numerous discoveries have been more recently made in other localities, by various investigators, to whom the author expresses indebtedness.

The detailed descriptions of the genera and species, illustrated by no less than twenty plates, form the first satisfactory account of the American Triassic fish-fauna; and this will prove of great value for comparison with the corresponding assemblages of fishes met with elsewhere. To the Lepidosteoid family of Lepidotidæ are assigned *Ischypterus*, with eighteen species, *Catopterus*, with six species, *Acentrophorus*, *Dictyopyge*, and *Ptycholepis*, each with one species; while the Crossopterygian family of Cœlacanthidæ is represented by a peculiar genus and species—*Diplurus longicaudatus*. Some interesting general remarks upon each genus precede the more detailed discussion of the various species; and, in the case at least of *Ischypterus*, we are inclined to agree with the author when he suggests that future researches may tend to reduce the number of specific types he at present feels justified in recognizing. In such cases as the present,

it is most difficult to distinguish the results of crushing and disintegration from actual specific characters; and even in a formation so little disturbed as the black Triassic shale in which these fishes are entombed, the apparent form of the head and trunk cannot always be relied upon in specific diagnoses.

Ischypterus is undoubtedly identical with *Semionotus*, as Dr. Newberry suspects, and is thus represented both in America, Europe, and South Africa. Ichthyologists will doubtless also agree with the systematic position in which the author places the genus. To us, however, it appears that this determination was more conclusively proved by the researches of Dr. Traquair in 1877, when he offered to the Geological Society some detailed remarks on the osteology of the fish; and on that occasion the intimate connection between *Ischypterus* and *Semionotus* was equally pointed out. Each of the species is illustrated by at least one figure, and good reason is given for assigning to one of the larger forms the supposed fragment of a *Tetragonolepis*, brought from Virginia by Lyell.

Catopterus and *Dictyopyge* are retained as distinct genera, in accordance with the usual custom; and then follow two interesting types which the author himself has added to the list. A very distinct species of *Ptycholepis* is described from Durham, Conn., and an equally peculiar species of *Acentrophorus* is made known from the Chicopee Falls, Mass. Of these genera, the first has hitherto been known chiefly from the English and German Lias, though also rarely obtained from the Austrian Keuper; while the second has previously been found only in the Permian magnesian limestone and marl slate of Durham, England.

A preliminary definition of the Cœlacanth fish *Diplurus longicaudatus* was given by Dr. Newberry several years ago; and the detailed description and figure now published are a welcome addition to our knowledge of the group to which the fish belongs. The finest specimen is nearly complete, and is only disappointing in the matter of cranial osteology. The largest specimen discovered measured about three feet in length, thus exceeding in size any Cœlacanth hitherto met with below the Jurassic.

The third portion of the memoir deals with the fossil plants, only seventeen species of which have been brought together. These confirm the views of Saporta as to the infra-lias, or, at most, Keuper, age of the formation, arrived at from a study of the far more important series described by Fontaine in the sixth monograph of the Survey, issued in 1883; a series procured from the coal-bearing outliers of the same age in Virginia and Carolina.

Of plants common to the Rhætic of Europe we have *Clathropteris platyphylla*, *Cheirolepis Münsteri*, *Otozamites latior*, *O. brevifolius*, two species of *Pachyphyllum* hardly separable from *P. peregrinum*, *Equisetum Rogersi*, claimed by Saporta to be identical with *E. arenaceum*, and the doubtful stems well known in many Triassic rocks, sometimes referred to Calamites, but here referred to *Equisetum Meriani*, Brong., and *Schizoneura*.

Among the novelties is *Dendrophycus triassicus*, a supposed algoid with a cabbage-like leaf destitute of transverse nerves. From the fact that there is in the British Museum an identical structure, from a gritty Tertiary limestone of Mull, which can hardly be organic, we should question the vegetable nature of this fossil, without, however, being able to suggest any other plausible origin. There is a new Cycadinocarpus, founded on a compressed cycas-looking nut, possibly the fruit of one of the Otozamites; and the obscure plant, referred to by Fontaine under the misleading name of *Bambusium Carolinense*, now called *Zoperia simplex*. Whether, as suggested by the author, this may prove to be an aquatic Monocotyledon—"a kind of gigantic Schollera"—there are no sufficient materials for discussing. It is somewhat surprising to find *Baiera Münsteriana* located among the Cryptogams, as there are so many forms connecting it with Ginkgo, all possessing the remarkable twin fibro-vascular bundles in the petiole which result in the symmetrically cleft leaf, that its position is scarcely doubtful. We prefer that the Cycads should precede the Conifers, but in so small an assemblage of species, their want of arrangement is of no great importance.

A. S. W. AND J. S. G.

SEA ANEMONES OF THE NORTH ATLANTIC.

Den Norske Nordhavs-Expedition, 1876–1878. *XIX. Zoologi: Actinida.* Ved D. C. Danielssen. Med 25 Plader og 1 Kart. (Christiania: Grondahl and Son's Bogtrykkerl, 1890.)

ANOTHER part of the General Report of the Norwegian North Atlantic Expedition has just been published, containing a memoir on the Actinida of the North Atlantic, by D. C. Danielssen. It will be remembered that this fine series of memoirs is published under the sanction of the Norwegian Government, and with some assistance from their Treasury. They have been distributed to very many of the Academies and learned Societies of the world, and reflect immense credit on the zeal and intelligence of the Norwegian naturalists.

All the specimens described by Dr. D. C. Danielssen in this memoir were collected from deep water, and most of them from the "cold area." These anemones, for the greater part, proved capable of accommodating themselves to changes of habit and temperature, and it was therefore possible to keep them alive for a considerable period, during which their external characteristics were observed and their portraits taken. That, despite the heavy rolling sea so generally met with in the North Atlantic, the artist has done his part well, is proved by a glance at the first five plates which accompany this memoir, which have been printed in colours by Werner and Winter, of Frankfort-on-the-Main.

This memoir represents the first serious attempt, since the publication of Richard Hertwig's Report on the *Challenger* Actinaria, to describe the sea anemones of an extended area, taking their anatomical features as the basis of their classification; and it seems to us to justify the remark that a very much larger series of facts must be noted before an even fairly plausible scheme of classification of this group can be formulated. No doubt the systems of Gosse and Andrès, based for the most part on mere external characteristics, have had their day; but no new scheme to take their place has yet been properly developed; a wider and closer anatomical investigation of even well-known species must be undertaken ere this can be looked for.

Perhaps this will in some measure account for the fact that of the Actiniæ collected during the expedition, thirty-nine out of forty-one are described as new species, for which eighteen new genera are diagnosed, and five new families are formed. The large majority of these new forms belong, as might be expected, to the Hexactiniæ of Hertwig, but some belong to the Edwardsiæ, Zoantheæ, and Ceriantheæ; while a new tribe has been provisionally made to receive two forms (Fenja and Ægir), not at first sight clearly appertaining to the Actinaria. These forms have elongated, cylindrical, vermiform bodies, with an apparently complete body cavity; the oral disk is surrounded with tentacles, and opens into an œsophagus, which is continued into a closed intestine, which opens at the aboral end of the body. There are twelve septal chambers, complete in themselves, with twelve pore openings around the anal opening.

In *Fenja mirabilis* the body is 70 mm. in length and 15 mm. in breadth at the anterior extremity, whilst the posterior part is rather narrower; the surface of the body is smooth and shining.

In *Ægir frigidus* the animal is surrounded with a mucous investment, and the body is but 30 mm. in length; from 8 to 10 mm. in breadth at the anterior extremity, to 4 to 5 mm. in breadth at the somewhat rounded posterior extremity. While in Fenja the ovaries do not materially differ from the type in the Actinida, those in Ægir greatly approach the form generally met with in the Alcyonida.

It would seem useless to speculate as to the position these strange forms must occupy until something more is known of their structure and something of their development. Dr. Danielssen writes that, if the cœlom is to be regarded as the distinctive feature, then it is evident they cannot be placed among the Cœlenterata; but he adds that perhaps too much stress has been laid on the so-called gastro-vascular apparatus as a systematic feature in this group, and that what is called the œsophagus in Actinida is possibly a rudimentary intestinal formation.

We have alluded to these two forms in some detail as being of very special interest, but an almost equal interest attaches to others which are also to be found described, but which our space forbids us to do more than thus generally refer to. In addition to the coloured plates representing the new species, there are nineteen with the various anatomical details, making this memoir one of the best illustrated of the series. It follows so closely on the memoir on the Alcyonida by the same distinguished author, that we cannot but express our admiration for the energy he displays in working out the natural history of the Norwegian coast, which is now better known than that of our own shores.

OUR BOOK SHELF.

Smithsonian Report, 1887. (Washington: Government Printing Office.)

THIS important publication is increasing year by year in value, in consequence of the pains taken to increase the quantity and quality of the records of progress in the various sciences. In the present volume it brings the records of the Institution down to June 30, 1887. We find the proceedings of the Board of Regents and of the Executive Committee, the Report of the Secretary on the general work of the establishment, the National Museum, and the Bureau of Ethnology. But there is very much more than this, although these matters are by no means of merely local interest. The progress of astronomy, North American geology and palæontology, vulcanology and seismology, geography and exploration, physics, chemistry, mineralogy, zoology, and anthropology, take up no less than 500 pages, and are admirably done. We should add that the record of each branch of science is accompanied by a full bibliography, which largely increases its usefulness. The miscellaneous papers this year deal chiefly with the Western mounds and Indian archæology.

Travels and Discoveries in North and Central Africa. By Henry Barth. (London: Ward, Lock, and Co., 1890.)

FORTY years ago Barth was invited to join a mission which the British Government was about to despatch to Central Africa. He accepted the invitation, and was absent from Europe nearly six years, in the course of which he travelled from Tripoli to Bórnu, and from Bórnu to Timbúktu. The account of his explorations, published in 1857 in German and English, was immediately recognized as one of the most important and fascinating of modern books of travel; and even now, after so long an interval, it has lost but little of its interest. In the present volume, which belongs to the Minerva Library, the first half of the great traveller's elaborate work is reproduced with many of the original illustrations. The books of travel by Darwin and Wallace, which have been reissued in the same series, differ considerably from that of Barth, who was not a naturalist; but, as Mr. Bettany, the editor, says, "to make up for this he is extremely rich in topographical, historical, and anthropological details." Mr. Bettany contributes to the volume a short introduction, in which he brings together some of the leading facts relating to Barth's career.

Weather Forecasting for the British Islands. By Captain Henry Toynbee, F.R.A.S., &c. (London: Edward Stanford, 1890.)

THIS is a most interesting and useful little book, and should be in the possession of all those who take any interest whatever in weather forecasting. It is written with the intention of showing what a single observer can do as regards this subject, supposing him to have a barometer, means for observing roughly the direction and force of the wind, and power to recognize cirrus clouds and the direction from which they are coming. To make the book more complete, the author has added some daily weather charts to illustrate the application of the principles and variations which occur in practice, and to show what can be learnt from them.

The Encyclopædia of Photography. By Walter E. Woodbury. (London: Iliffe and Son, 1890.)

THIS is the second part of the work we noticed before, to be completed in about twelve parts issued monthly. The ground covered is from B to Coffee Process, between which entries will be found information useful to all classes of photographers. Bromide paper, camera-bellows making, carbon process, may be mentioned as among the subjects most fully treated of. When completed, the work will contain over 1000 references, and be illustrated by about 200 explanatory sketches and diagrams by the author.

Japan and the Pacific. By Manjiro Inagaki, B.A. (Cantab.). (London: T. Fisher Unwin, 1890.)

THIS book, so far as it has any elements of interest, appeals rather to politicians than to students of science.

The subject with which the author deals is the relation of Japan to the Eastern Question, and therefore to England and Russia. It is a striking fact that such matters should be discussed in an English work by a native of Japan. Mr. Manjiro Inagaki cannot, however, be congratulated on the way in which he sets forth his ideas. His facts are thrown together so loosely that it is sometimes difficult to make out the propositions which he wishes to prove or to illustrate.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Indiscriminate Separation, under the Same Environment, a Cause of Divergence.

I HAVE accumulated a large body of facts indicating that separated fragments of a species, though exposed to the same environment, will in time become divergent. I find that, wherever a species possessing very low powers of migration is for many generations divided into a series of fragments by barriers that do not obstruct the distribution of surrounding species, more or less divergence arises in the separated portions of the species, though, in the same areas, there is no divergence in the environing species whose distribution is not obstructed. I still further find that, whenever the distances intervening between the different fragments are an approximate measure of the time and degree of separate breeding (as is frequently the case, as long as the divergence does not involve any physiological and psychological segregation), these distances are also an approximate measure of the degree of divergence.

The validity of this conclusion is called in question because it is inconsistent with the theory that all divergence is due to diversity of natural selection, and that all diversity of natural selection is due to exposure to different environments. The divergences in the cases above referred to, it is said, are probably due to differences in the environment that are not easily recognized. This was the explanation suggested by Darwin when the facts were reported to him in 1872. The division of a species into isolated portions did not seem to him to furnish any factor that could produce divergence unless it was aided by exposure to different external conditions. The same view is expressed in his "Origin of Species," sixth edition, p. 319.

My reply is twofold. (1) The theory that all the divergences in Sandwich Island land mollusks are due to differences in the environment requires us to believe that there are occult influences increasing in difference with each additional mile of separation, and that these influences control the natural selection of the mollusks, but have no influence on any of the other species occupying the same areas. A theory that involves so heavy an assumption cannot be received when a simpler theory is open to us. (2) I believe I can entirely remove this objection, urged against my conclusion on these purely theoretical grounds, by showing that there are certain causes of divergence, not depending on exposure to different environments, that are necessarily introduced by the division of a species into isolated groups; and that, under the influence of these causes, diversity of habits may arise producing diversity of natural selection, even while the fragments are exposed to the same environment.

I have elsewhere called attention to the fact that the independent breeding of separated groups, so far as we can judge, always tends to produce divergence; and I have shown that, when a species is indiscriminately broken into independent fragments, the tendency to divergence will, on the average, vary in direct proportion to the instability of the species, and in inverse proportion to the size of the fragments, for on these factors depends the probable degree of departure of the average characters of the fragment from the average character of the species previous to its being broken into fragments, and, therefore, the degree of segregation.

I wish now to show that the maintenance of certain classes of characters always belonging to an unbroken species is due to a form of selection that can continue only so long, and so far, as free crossing continues. Reflex selection is a formative principle, depending on the relations in which the members of an inter-generating group of organisms stand to each other, while they continue to inter-generate; but when two portions of an original species have become so divergent as to compete with each other in the same area without crossing, they form incipient species, and each belongs to the environment of the other. While they are members of the same inter-generating group, their mutual influence results in reflex selection, which maintains the correspondence with each other by which power to cross is preserved; while they are members of groups that do not cross, their mutual influence results in mutual selection that inevitably tends toward the preservation of variations that, through greater divergence, best escape from competition. I have elsewhere defined reflex selection as being the exclusive propagation of those better fitted to the relations in which the members of the same species stand to each other, resulting from the failure to propagate of those less fitted. Among those that are equally fitted to the environment of the species, and therefore equally preserved by natural selection, there is often great difference in the degrees of fitness for sustaining such relations to the rest of the species as will secure an opportunity to propagate. To this class of influences belong the different forms of sexual selection through which the sexual instincts and other sexual characters of the different sexes are kept in full co-ordination. In like manner we must believe that the pollen of any species is kept up to its full degree of potency by the constant selection which results from the failure to propagate of the individuals whose pollen is less potent or whose germs are more difficult to fertilize than the average. We cannot call this sexual selection; but we have to class it as the form of reflex selection through which the physiological co-ordination of the male and female elements with each other is so maintained as to secure full fertility. Again, there is a constant selection of animals that are suitably endowed with the recognition marks and calls by which the different members of the species know each other, and that have the corresponding instincts that lead them to associate with their own kind who are thus endowed. I have elsewhere called this principle of social co-ordination "social selection," and have classed it as a form of reflex selection.

There are several other forms of reflex selection. One of these secures harmony in the habits and modes of life of the different members of a freely inter-generating group of organisms; for individuals, whose habits are not sufficiently co-ordinated with those of the mass of the species to allow of their interbreeding with them, will fail of propagating. This we may call co-ordinative industrial selection. We are now prepared to understand one reason why independent breeding resulting from indiscriminate separation is in time transformed into segregation. Independent breeding is in its very nature the suspension, not only of one form, but of all forms of reflex selection between the separated portions of the species. The importance of the cessation of natural selection in producing the different stages of the degeneration of organs that are disappearing has been fully discussed by Prof. Romanes (see NATURE, vol. xli. p. 437, and previous communications there referred to), who points out that, as the power of the special form of heredity by which any organ is produced has been built up by the many generations of natural selection that have acted in favour of the organ, so the gradual weakening of that power follows the cessation of the natural selection. Prof. Weismann seems to appeal to the same principle when he attributes the disappearance of "rudimentary organs" to the action of "panmixia." Now, in the cessation of reflex selection which follows independent breeding, a similar principle is introduced, and the inevitable result must be the weakening of the power of heredity by which the portions of the species were held in correspondence with each other before their separation. I have elsewhere shown that separate breeding necessarily disturbs unstable adjustments~~; and~~ we here see that the most stable of the adjustments by which each part of a species is kept in correspondence with every other part gradually becomes unstable under the continued influence of separation. Whenever a species is divided into two portions that do not interbreed, the four forms of reflex selection above described will cease to act between the two portions, and they will continue in sexual, social, physiological, and industrial harmony with each other, only in so far as the force of the old heredity holds them to the old standards. But the power of heredity in these respects will in time fail, if reflex selection is entirely removed. If the

separate breeding is long continued, incompatibility in all these respects tends gradually to arise; but it is manifest that incompatibility of industrial habits implies diversity in the forms of natural selection that shape each portion. I therefore maintain that separation which necessarily includes the cessation of reflex selection between the portions separated is a cause of segregation and divergence, and that it introduces diversity of natural selection, which is a still further cause of divergence.

Unless the separated portions of a species possess exactly the same average character (which we must believe is seldom, if ever, the case), separation must, from the first, be more or less segregative; and even in cases where the portions completely correspond in character (if there are any such cases), the cessation of reflex selection which is involved in the separate breeding, must result in segregration as soon as the power of heredity begins to weaken; and this is in due time followed by other forms of intensive segregation. I therefore conclude that indiscriminate separation may be regarded as a preliminary form of segregation (*i.e.* discriminate separation), and that in the nomenclature we ordinarily use both principles may be called "segregation" without confusion.

26, Concession, Osaka, Japan. JOHN T. GULICK.

The Affinities of *Heliopora cœrulea*.

IN Prof. Moseley's admirable account of the structure and affinities of *Heliopora*, published in the Transactions of the Royal Society, 1876, and afterwards in the *Challenger* Reports, there occurs the following passage: ". . . directly the coral (*i.e. Heliopora*) was left at rest a swarm of a species of *Leucodora*, closely resembling *Leucodora nasuta*, which infests the coral and perforates it all over, expanded themselves at once."

This will probably explain the cause of the curious mistake that Mr. Saville Kent has made, in his letter published in last week's NATURE (p. 340), in supposing that *Heliopora* is a tubicolous annelid. *Heliopora* is *not* a tubicolous annelid, nor does it belong to the "Hydrozoic division of the Cœlenterata," but it is, without a shadow of doubt, as Moseley described it to be, an Alcyonarian.

When I was preparing my paper on the "Siphonoglyphe in the stomodæum of Alcyonarians" in 1883, Prof. Moseley kindly placed at my disposal his preparations of *Heliopora*, and I was able then fully to confirm his conclusions as to the Alcyonarian nature of this interesting coral.

During my visits to the coral reefs on the coasts of North Celebes and the adjacent islands, I came across many large and beautiful specimens of *Heliopora*, some of which I carefully preserved for further investigation at home. I never found the polyps fully expanded with the eight pinnate tentacles standing out from the disk like the petals of a flower, but in the few instances when I saw the polyps protruded ⅛ inch or thereabouts from the surface of the coral the tentacles were partially withdrawn, so that their characteristic features were hidden.

Since my return from Celebes I have made a large number of sections of the material I brought back with me with a view to the publication of a short paper on some further details of its anatomy, and I have recently been able to supplement this by a series of preparations I have made from the excellent material given to me by Prof. Haddon, who found *Heliopora* in abundance in Torres Straits.

I will not venture, in the present state of my investigation, to state my opinion as to the position that *Heliopora* should occupy in the group to which it undoubtedly belongs; I merely wish to call the attention of the readers of NATURE to the fact that its Alcyonarian characters are beyond dispute.

Downing College, August 9. SYDNEY J. HICKSON.

Meteors.

LAST night, between 11.12 and 11.52, I and another observer saw altogether eighty-three meteors, eighty of which were Perseids. Some of them were very brilliant, especially those near the neighbourhood of Aquila.

The remaining three meteors had different paths, one having a direction exactly opposite to that of the Perseids.

The other observer was watching the radiant point and the region around it, while I observed the south-west quadrant.

More observations would have been made, but were interrupted by clouds. W. J. LOCKYER.

Observatory House, Westgate-on-Sea, August 12.

A LIQUID COMPOUND OF NICKEL AND CARBON MONOXIDE.

IN the August number of the Journal of the Chemical Society a full account is given of the remarkable new compound described by Mr. Mond, in conjunction with Drs. Langer and Quincke, at the last meeting of the Chemical Society. The following is an outline of the main facts described in their communication.

Carbon monoxide is found to be affected in a very curious manner when passed over finely divided metallic nickel heated to a temperature of 350°–450° C. The metal becomes converted into a black amorphous powder containing nickel and carbon, the carbon monoxide becoming at the same time changed into the dioxide owing to the loss of carbon. A comparatively small amount of nickel is capable of decomposing a very large quantity of carbon monoxide, and at the commencement of the operation the gas may be passed over at a very rapid rate without any escaping decomposition. As the operation continues, the change becomes less and less complete, but even after numerous repetitions of the experiment carbonic anhydride continues to be formed. The solid product of the reaction appears to vary in composition somewhat widely according to the temperature and the time during which the operation is carried on. The highest proportion of carbon found was 85 per cent. Some time ago MM. Gautier and Hallopeau obtained a similar product, containing 80 per cent. of carbon, by the action of carbon bisulphide upon metallic nickel. The nickel is only partially removed by acids, for even after repeated extraction the whole of the nickel is not found in solution.

When this black substance was allowed to cool in the current of carbon monoxide another change was found to occur, with production of some volatile substance, whose vapour rendered a non-luminous Bunsen gas flame placed in its path highly luminous. Further, on heating a portion of the tube near the exit a mirror of metallic nickel was obtained mixed with a little carbon. Evidently a gaseous substance containing nickel was contained in the issuing gas, a circumstance of considerable importance in view of the non-volatility of the ordinary known compounds of nickel. Experiments were then made with the idea of obtaining larger quantities of the new substance and isolating it from the other gaseous products. It was eventually found that when finely divided nickel, obtained by reducing nickel oxide in a current of hydrogen at a temperature of about 400°, is allowed to cool in a slow stream of carbonic oxide, the latter gas is very readily absorbed as soon as the temperature has fallen to about 100°. If the current of carbon monoxide is continued, or if that gas is replaced by an inert gas, such as carbon dioxide, nitrogen, or even hydrogen or air, the issuing gas carries away with it large quantities of the new nickel-containing vapour. After about an hour the quantity of this vapour evolved becomes less, and finally its evolution ceases. The property of the nickel to produce it is restored by heating it to 400° again and allowing once more to cool; indeed, up to a certain limit it forms the compound more abundantly after repeated use. If the issuing gas is collected and heated to 150°, its volume is found to largely increase, nickel more or less contaminated with carbon being deposited. At a temperature of 180° the nickel deposited was found to be quite free from carbon.

The new volatile compound was eventually isolated by leading the mixed issuing gases through condensers placed in a freezing mixture of ice and salt, in which the vapours condensed to a *colourless* mobile liquid of very high refractive power.

The final arrangement adopted for the preparation of the liquid is as follows. A quantity of nickel oxide is placed in a combustion tube, and reduced at about 400° by the passage of a current of hydrogen gas. The tube and contents are then cooled down to about 30°, and pure dry carbon monoxide instead of hydrogen passed through the tube without further heating it. The issuing gas is caused to pass through a Y tube surrounded by ice and salt. The lower end of the Y tube projects through the vessel containing the freezing mixture into a small flask in which the liquid collects. The gas leaving the Y tube still retains about 5 per cent. of the new body, and is therefore collected, dried, and again passed over the nickel until no more liquid condenses. The tube containing the nickel is then re-heated to 400° in a slow current of hydrogen, again cooled, and the operation recommenced. In this manner it is easy to obtain ten to fifteen grams of the liquid in each operation.

The liquid boils at 43° under a pressure of 751 mm. Its specific gravity is 1·3185 at 17°. At −25° it solidifies to a mass of needle-shaped crystals. The liquid is soluble in alcohol, and even more readily in benzene and chloroform. It is perfectly indifferent to dilute acids and alkalies, and is not attacked by concentrated hydrochloric acid. Strong nitric acid oxidizes it readily. As regards its composition, the nickel was estimated by weighing the nickel deposited on passing repeatedly through a heated tube, and the carbon by passing the vapour mixed with air over copper oxide, and absorbing and weighing the carbon dioxide produced. The following numbers were obtained:—

	I.		II.		Calculated for $Ni(CO)_4$.
Nickel ...	33·35	...	33·37	...	34·34
CO ...	66·60	...	65·99	...	65·66

Its composition, therefore, appears to be represented by the formula $Ni(CO)_4$. Its vapour density, the first density determination of a nickel compound, was determined by Victor Meyer's method at 50°. The value obtained was 6·01. $Ni(CO)_4$ corresponds to the density 5·9. At 60° the vapour was found to explode with considerable violence.

Vapour of nickel-carbon oxide, as its discoverers term it, reduces an ammoniacal solution of cuprous chloride, first decolorizing it and subsequently precipitating from it metallic copper. It also precipitates metallic silver from ammoniacal solutions of silver chloride. Chlorine decomposes it with production of nickelous chloride, $NiCl_2$, and carbon oxychloride, $COCl_2$. Bromine reacts in a precisely similar manner. The electric spark decomposes it slowly into nickel and carbon monoxide.

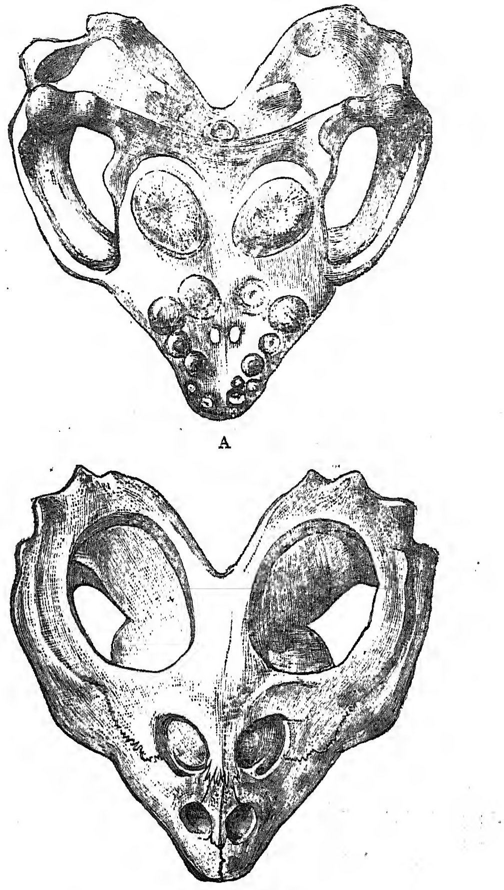
Experiments have also been made to ascertain the possibility or otherwise of preparing a similar compound of cobalt and carbon monoxide. It was found, however, that cobalt does not form such a compound; indeed, it is quite possible to separate nickel from cobalt by reacting with carbon monoxide in the above manner, the nickel only being removed. The metallic mirrors obtained by the decomposition of nickel-carbon oxide by heat were found to consist of unusually pure nickel, containing no traces of cobalt. They consisted of a grey metallic powder of specific gravity 8·2834 at 15°·4.

It became interesting, therefore, to ascertain the atomic weight of this pure nickel, especially in view of the recent work of Drs. Krüss and Schmidt. Accordingly, a series of three determinations were made, with the following results:—If O = 16, Ni = 58·58, 58·64, and 58·52. These numbers are sufficiently close to the value 58·74, long ago obtained by Dr. Russell, to justify the conclusion that nickel, as we have known it, is indeed a simple substance, whose atomic weight lies very near to the figure hitherto accepted—a conclusion which is further supported by the determination of the vapour density of this remarkable new compound, nickel-carbon oxide.

A. E. Tutton.

BRITISH MUSEUM NATURAL HISTORY PUBLICATIONS.[1]

The present Part (IV.) concludes Mr. Lydekker's "Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History)," the four volumes making together a work of 1247 pages. In Part I. the author records the Ornithosauria, the Crocodilia, the Dinosauria, the Squamata, the Rhynchocephalia, and the Proterosauria; Part II. contains the Ichthyopterygia and Sauropterygia; Part III. embraces the Chelonia; and Part IV. the anomalous group of the Placodontia, the Anomodontia, and the class Amphibia,



A

B

Fig. 1.—*Cyamodus* (*Placodus*) *laticeps*, Owen. A, palatal aspect; B, frontal aspect of cranium; from the Muschelkalk of Baireuth, Germany.

including the Ecaudata, the Caudata, and the Labyrinthodontia, with supplementary notes and additions to the preceding orders. The earlier parts having been already noticed in Nature, we shall confine our attention to Part IV.

Amongst the rare remains of Reptilia met with in the Muschelkalk of Baireuth, Bavaria, none are of more interest than those belonging to the anomalous group of the Placodontia, the ordinal position of which is still uncertain. The skull and teeth of one of these reptiles

[1] "Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History), Cromwell Road, S.W." Part IV., containing the Orders Anomodontia, Ecaudata, Caudata, and Labyrinthodontia; and Supplement. By Richard Lydekker, B.A., F.G.S., &c. Pp. 295 and xxiv. With Index to the entire Work. Illustrated by 66 Woodcuts. (London: Printed by Order of the Trustees; and sold by Longmans and Co.; B. Quaritch; Asher and Co.; Kegan Paul, Trench, Trübner, and Co., &c., 1890).

was originally referred by Count Münster, and afterwards by Agassiz, to the class of fishes, under the genus *Placodus;* but more perfect specimens enabled Prof. Owen, in 1858, to show that this animal was really a reptile which probably fed upon shell-bearing mollusks and used its flat, broad, palate-like teeth, so thickly-coated with enamel, for pounding and crushing their shells (see Phil. Trans., 1858, p. 169).

Two genera, *Placodus* and *Cyamodus*, are referred to this group, at present known only by the skull and teeth, no vertebræ or bones of the pectoral or pelvic girdles, or limbs, having been as yet discovered. Owen originally referred this singular form to the Sauropterygia, but subsequently he regarded it as belonging to the Anomodontia, in which order Seeley also places the Placodontia. The present author, however, assigns the Placodonts to no ordinal position, a course which, we think, is to be regretted. If not Anomodont reptiles, why not give them the value of an order? Surely they have as good a claim to such a position as *Proterosaurus?*

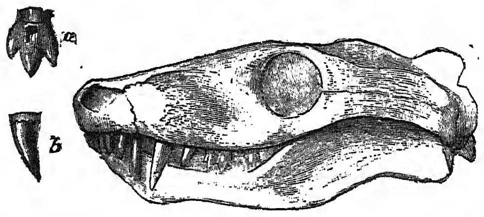


FIG. 2.—Left lateral aspect of skull of *Galesaurus planiceps*, Owen; from the Karoo beds (Triassic), South Africa. (⅓ nat. size.) *a*, an upper cheek-tooth; *b*, an incisive tooth.

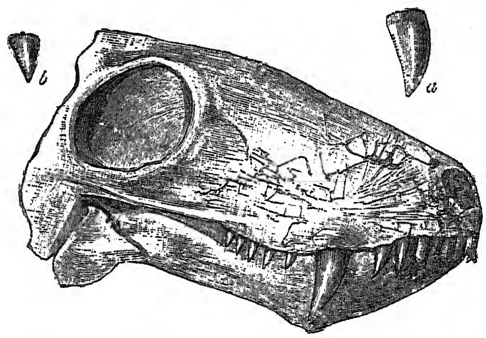


FIG. 3.—Right lateral aspect of imperfect cranium of *Ælurosaurus felinus* Owen; from the Karoo beds (Triassic), Beaufort West, South Africa. (⅔ nat. size.) *a*, upper incisive tooth; *b*, upper cheek-tooth, enlarged.

The Anomodontia, which follow next in order, are a truly Triassic group, and have been met with in Russia, India, North America, and in South Africa. It is especially from this last-named region that the British Museum collection has been most largely recruited, the majority of the specimens having been procured by Messrs. A. G. and T. Bain, Dr. Atherstone, and Sir George Grey. Quite recently, Prof. H. G. Seeley, F.R.S. (assisted by the Government Grant Committee of the Royal Society), visited the Cape, where he was most successful in obtaining a large series of reptilian remains, not yet fully worked out, but of which sufficient is already known to justify us in believing it will prove one of the most valuable additions made for years past to our National Museum.

The interest attaching to these South African Triassic rocks (if Triassic they be) lies in the fact that they have yielded evidence of one of the earliest mammals known—*Tritylodon*—represented by a most remarkable although imperfect cranium, with dentition similar to Cope's genus *Polymastodon*, from the Eocene of North America.

In the group of Anomodont reptiles are included several forms having a well-differentiated series of cheek-teeth, canines, and incisors, a character of dentition considered at one time to be peculiar to the Mammalia. Good examples of such dentition may be seen in the skulls of *Galesaurus*, *Ælurosaurus*, *Lycosaurus*, &c.

Another no less singular family, placed in this order, is that of the Dicynodontidæ, in which the surface of the

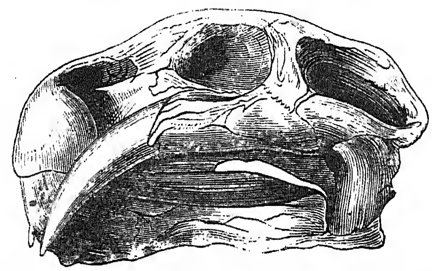


FIG. 4.—Lateral view of the skull of *Dicynodon lacerticeps*, Owen; from the Karoo series, South Africa.

palate and mandible are without teeth, the skull being provided with a pair of tusk-like maxillary teeth, growing from persistent pulps; the alveolar margins of the jaws being trenchant, and probably encased in a horny beak-like sheath, as in *Hyperodapedon*.

Another remarkable form of Anomodont, from these Reptiliferous beds of South Africa, has been referred to the genus *Pariasaurus* by Owen. In the form of its

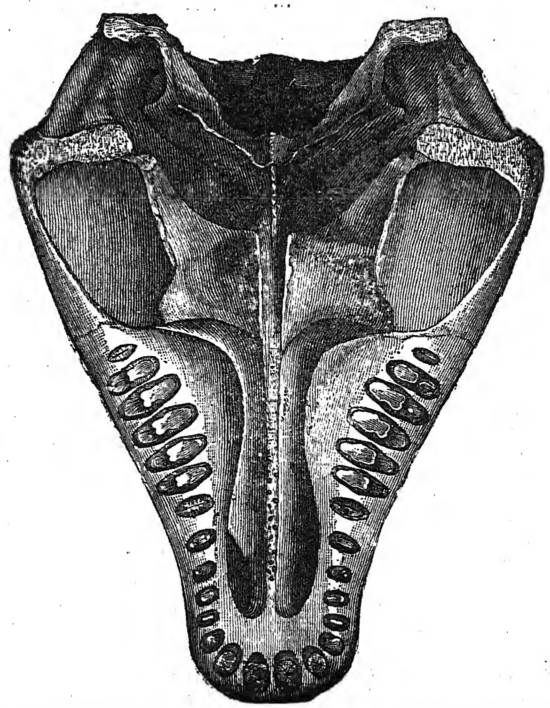


FIG. 5.—Palatal aspect of cranium of *Empedias molaris*, Cope; from the Permian of Texas, North America. (¼ nat. size.)

head it is very like a huge Salamander, 8 to 10 feet in length, having a numerous and uniform series of moderately tall marginal teeth in its jaws, with swollen and narrow crowns, ornamented with a few deeply-marked flutings descending from the cutting edge, and with numerous small conical teeth on the palate. The

skull is deeply channelled on its surface, as in the Crocodilia and the Labyrinthodontia.

From the flattened wearing away of the crowns of the teeth, Prof. Owen has suggested it was a vegetable-feeding reptile. The vertebræ of *Pariasaurus* are notochordal, frequently having intercentra present, and there are not more than two vertebræ united to form the sacrum.

The Permian rocks of Texas have yielded to Prof. Cope a most remarkable genus of Anomodont reptiles, named by its describer *Empedias molaris*. The dentition forms an uninterrupted series without a distinct tusk, the incisors differing but little from the cheek-teeth in form, each tooth having a more or less distinct transverse edge. The teeth are about fifty-six in number.

The genus *Naosaurus*, also from the Trias of Texas, makes us acquainted with a very curious reptile, in which the neural spines of the vertebræ are of most enormous height, and each spine has often as many as six paired horizontal processes at intervals produced from its sides.

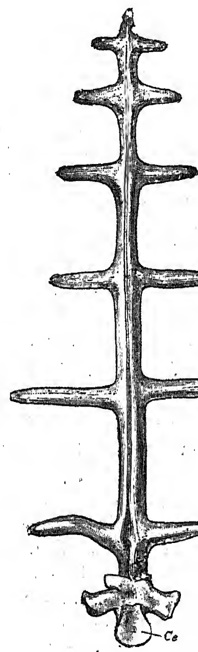


FIG. 6.—Anterior view of a dorsal vertebra of *Naosaurus claviger*, Cope; from the Permian of Texas. (¼ nat. size.) *Ce*, centrum.

This reptile when living must have had an enormous dorsal crest, like some monstrous newt, rising from its back, but it is difficult to conceive any advantage which its owner could possibly derive from such an unwieldy appendage.

Turning to the Amphibia, one cannot fail to be struck by the similarity in the form of the cranium, and its external ornamentation, in the Labyrinthodontia and the Crocodilia. The body is also long, usually lacertiform, and the feet pentadactyle; a bony thoracic buckler and bony scutes are frequently present on the ventral aspect of the body. Doubtless these old Triassic reptiles were ancestrally related to the later Crocodilia, as well as to other and higher forms of Vertebrata.

Teeth in the Labyrinthodonts are usually present on the palatines and vomers, and more rarely on the pterygoids; and there is very generally an ossified sclerotic ring to the orbit. The vertebræ exhibit considerable variation in condition, being amphicœlous, and fully ossified in some instances, or with a notochordal canal, or with large intercentra and the centra represented by paired lateral pieces (pleurocentra) in others.

The parietal foramen is always present in the cranium, and in the Mastodonsauridæ the occipital condyles are well ossified.

The Trias of Würtemberg has yielded the finest known examples of these Labyrinthodont reptiles, quite recently described and figured by Dr. Fraas; but the most complete skeletal remains of Amphibia have been obtained from the Gaskohle (Lower Permian) of Bohemia by Dr. Anton Fritsch, of Prague; others from Germany by Prof. Credner, of Leipzig; and by Prof. Gaudry from the Lower Permian of Autun. The best examples in the collection are those of *Archægosaurus* from the Lower Permian of Saarbrücken, and of *Loxomma* from the Coal-measures of Coalbrookdale and Scotland.

To the Ecaudata (frogs and toads) little interest attaches in a palæontological sense, as no tailless forms of Amphibia are known earlier than the Miocene period; but good examples of these have been obtained from the Brown-Coal of Rott, near Bonn, and from the Fresh-water Tertiary Limestone of Oeningen, which also yields the remains of *Cryptobranchus scheuchzeri*, closely related to the giant salamander now living in the fresh-waters of Japan.

Contrasting for a moment the MAMMALIA with the REPTILIA, while many genera of the former, such as *Dinotherium*, *Mastodon*, *Machairodus*, *Phenacodus*, *Palæotherium*, *Anthracotherium*, *Taxodon*, *Sivatherium*, *Dinoceras*, and others, have died out, *eight entire orders* of REPTILIA and AMPHIBIA, embracing more than 200 genera, have all disappeared. This is the more readily understood when we consider the comparative periods of geological time during which the Mammalia and Reptilia have respectively flourished; for whilst it is true that the earliest known forms of Mammalia made their appearance as far back as the termination of the Triassic period, yet during the whole of the succeeding Jurassic and Cretaceous periods their numbers were few and their forms quite insignificant; and it is not until we arrive at Eocene times that the Mammalia commence to occupy anything like a prominent position in the animal kingdom. On the other hand, the Amphibia began to be abundant as early as the Coal period; and the Reptilia (ushered in by *Proterosaurus*) in the Permian attained a maximum development both in size and numbers in the Lias and Oolites, whilst the Mammalia were yet only in the incipient stage of their development.

Great credit is due to Mr. Lydekker for the manner in which he has performed the task of preparing these Catalogues for the National Collection, a work which will doubtless prove of extreme value to students of comparative anatomy and to workers at a distance who desire to know what objects in any particular family or genus the Museum possesses.

We could wish that greater distinctness had been given in printing these Catalogues to the important fact of particular specimens being the ones which are known as "types," and which are the actual ones that have been figured and described; where this is mentioned it does not catch the eye at once, as it should do. We would advocate the placing of such information in a separate line; and, if possible, they should be marked prominently by the use of *special type*. Perhaps the word "type" or "figd." could be inserted in black letter and begin a separate line.

Again, the formation and locality are hardly prominent enough, and under each genus we would like to see the "range in time" and also the "geographical distribution" given as a separate paragraph.

We hope this series of Catalogues, so helpful to all real workers, may be continued and completed for every group in the Geological Department. The Trustees could not perform a more useful service to science than by urging forward the issue of these works in every Department of the Museum.

THE AUSTRALASIAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE third annual meeting of this Association will be held, as we stated last week, at Christchurch, New Zealand. On January 15, the President-Elect, Sir James Hector, F.R.S., will hold a reception in the grounds of Christ's College, and the first general meeting will take place in the evening, when Baron F. von Mueller, F.R.S., will resign the chair, and an address will be delivered by his successor.

The Council of the Australasian Association invite the members of the British Association to attend this meeting, and a circular relating to the matter, signed by Profs. A. Liversidge, F.R.S., W. Baldwin Spencer, and F. W. Hutton, the general secretaries, will be distributed at the Leeds meeting. The cheapest way of reaching New Zealand is by the direct steamers which leave Plymouth every fortnight for Wellington. These steamers call at Teneriffe, Cape Town, and Hobart on their way out, and at Rio de Janeiro and Teneriffe on their way back. In the circular to which we have referred, it is stated that return tickets will be issued to members of the British Association proceeding to New Zealand to attend the Christchurch meeting for £84, which is 20 per cent. below the ordinary return fares. These tickets will be issued by the New Zealand Shipping Company and by the Shaw, Savill, and Albion Company, and holders may return by either line. In addition to this advantage, members of the British Association will be allowed to travel over the New Zealand Government railways (1770 miles) at half fares during January and February.

Visits to places of interest in the immediate neighbourhood of Christchurch will be made during the meeting. After the meeting is over, excursions will be made to the West Coast Sounds; to the top of Ruapehu; and, if possible, to the Upper Rakaia. The trip to the top of Ruapehu will start from Napier, and will be accompanied by Mr. H. Hill. The trip to the Sounds will start from Port Chalmers, and will be accompanied by Prof. Hutton.

Members of the British Association have thus a splendid chance of visiting New Zealand, and of seeing for themselves what is being done in science by our kinsfolk in Australasia; and no doubt a good many will avail themselves of the opportunity. Those who decide to accept the invitation are requested to notify their intention, as well as the name of the steamer by which they propose to go, to Prof. F. W. Hutton, the general secretary in Christchurch, in order that arrangements may be made for their reception. The following steamers will leave Plymouth in time for the meeting:—

Company.	Steamer.	Leave Plymouth.	Arrive at New Zealand.
N.Z. Shipping Co.	S.S. *Kaikoura*.	Nov. 15, 1890	Dec. 28, 1890
Shaw, Savill, and Albion Co.	S.S. *Doric*.	Nov. 29, 1890	Jan. 11, 1891

Members going by the *Kaikoura* could visit the Hot Springs district of the North Island before attending the meeting.

NOTES.

NOTHING of scientific value can be extracted from the ghastly descriptions of the recent electric execution. These graphic horrors are too evidently manufactured for sensational or political purposes to be trusted, even had the writers been spectators of the scene. But we may at least gather from them that an entire absence of physiological knowledge, and a very scant acquaintance with elementary physical principles, were exhibited alike by the contrivers of the operation and by the actual operators. With our present physiological knowledge, electric *currents*, whether steady, interrupted, or alternating, are not qualified primarily to produce death, but torture—which, of course, may lead ultimately to death. They have been recommended, in the interests of humanity, as efficient and (if the expression be permitted) healthy substitutes for the "cat." But Nature's own operations, in a thunderstorm, suggest the true substitute for the axe or the cord, viz. the discharge of a condenser of sufficient capacity, charged with so-called "statical" electricity.

THE French Association for the Advancement of Science has been holding its annual meeting at Limoges. The meeting began on August 7, and will come to an end to-day. M. A. Cornu, the President, chose as the subject for his address "the part played by physics in the recent progress of the sciences."

A CONSIDERABLE impetus to scientific study ought to be given by the science scholarships which the Royal Commission for the Exhibition of 1851 is about to establish. They amount in the aggregate to £5000 a year, and are to be used for the benefit of English provincial colleges, and of colleges in Wales, Scotland, Ireland, and the colonies. In accordance with the recommendations of a scientific committee, each of the scholarships will be £150 a year in value, and will be tenable for two years—in rare cases for three; and they are to be restricted to those branches of science (such as physics, mechanics, and chemistry), the extension of which is specially important for our national industries. A series of seventeen scholarships will be allotted to various institutions annually. The first allotment, as the Commissioners explain in a paper they have issued, is to be considered experimental and temporary. "The selection now made of institutions to which nominations are offered will be subject to modification in the future, having regard not only to the manner in which the nominations are exercised, but also to the claims of other universities and colleges which may from time to time be brought under the consideration of the Commissioners."

THE Reale Istituto di Scienze e Lettere of Milan offers prizes as follows:—(1) A historico-critical investigation of works on the variations of climate in geological times (with estimation of hypotheses as to the causes of those changes). Prize, 1200 lire (the lira equals $9\frac{1}{2}d.$). (2) A monography of the Protista of spring water in Milan. Cagnola Prize of 2500 lire, and a gold medal of 500 lire. (3) Elucidation, by personal observations, of some points in the physiology of the nervous system, especially the brain. Fossati Prize of 2000 lire. (4) Elucidation of the physiology, or the macro- or microscopic anatomy, of the brain. Fossati Prize of 2000 lire. (5) Draper's theory of the progressive development of the light-rays of a body whose temperature is gradually raised having been attacked by Prof. Weber, a thorough investigation of the phenomena is desired, so as to establish their laws, to exclusion of the ordinary influence of the observer on the meaning of the phenomena. Secco-Commeno Prize of 864 lire. Papers to be written in Italian, French, or Latin, and sent in, with motto, to the Secretary, Palazzo di Brera, Milan. The dates are—for No. 1, April 30, 1891; for Nos. 2 and 3, May 1, 1891; for No. 4, April 30, 1892; and for No. 5, May 1, 1893.

THE Berlin Academy of Sciences has recently granted £60 (each) to Prof. Dames, of the Mineralogical Museum, for a geological investigation of Dalecarlia and the island of Gotland; to Prof. Urban, of the Botanical Garden, for a visit to Paris, to study the specimens of West Indian flora there; and to Dr. Rinne, for study of the Central German basalts. Further, £75 has been granted to Prof. Nussbaum for publication of his studies on Californian Cirrhipedia, and £27 for printing of Dr. Schumann's researches on the union of races. £75 is granted to the Anatomical Society, to further the publication of Prof. His's uniform anatomical terminology.

DR. ST. GEORGE MIVART, F.R.S., has been appointed Professor of the Philosophy of Natural History at the University of Louvain. The professorship is one of those included in the Faculty of Philosophy and Letters.

THIS week the Royal Archæological Institute has been holding its annual Congress at Gloucester. The first meeting took place on Monday, when the chair was taken by Sir John Dorington, in succession to Lord Percy. In his presidential address, Sir John described the neighbourhood of Gleva as it was under Roman civilization in contrast with its later condition in the time of the Saxon invasion.

ON Tuesday the Royal Horticultural Society held a meeting and show in the Drill Hall, and certificates were distributed by the Committee. A paper by Mr. Badger, on fruit-drying by evaporation, as practised in America, was read by Mr. Wilkes, the Secretary. Little fruit, it is said, will be preserved in England this year by the processes described, a worse season generally for plums and apples having seldom been known.

ON Monday the Fellows of the Royal Botanic Society held their fifty-first anniversary meeting. The Council, in their report, congratulated the Fellows upon the firm position held by the Society in the year of its jubilee, and thanked them for their action in response to which 109 new names were added to the list. The result was a permanent growth of prosperity, as shown by the total subscriptions for the year—£3568—which had not been reached since 1885.

THE *Kew Bulletin* for August opens with some interesting notes on Natal aloes, by Mr. J. Medley Wood, the curator of the Natal Botanic Gardens. There are also sections on Gambia mahogany, Ceylon cacao, chestnut flour, wine production in France, and ramie as food for silkworms. The number closes with a list of the staffs of the Royal Gardens, Kew, and of botanical departments and establishments at home, and in India and the colonies, in correspondence with Kew.

IN the new number of the *Internationales Archiv für Ethnographie* (Band iii., Heft 3), there is an article (in German) by Dr. Richard Andree, of Heidelberg, on the Stone Age of Africa. Dr. J. D. E. Schmeltz contributes a finely-illustrated and valuable study (also in German) of decorated weapons used in the East Indies. There is also a short English paper on Zuñi fetiches, by Dr. H. Ten Kate, of the Hague.

THE Japanese collections of Heinrich von Siebold were lately presented to the Hofmuseum of Vienna. They consist of about 5200 specimens, many of which are of great value. In recognition of the donor's generosity, the Austrian Emperor has raised him to the rank of Freiherr.

IN the museum of the Industrial Society of Mühlhausen, there is an interesting ethnographical collection, including a number of fine American antiquities. The objects are being rearranged by Herr E. Grosse.

A WORK on Hindoo folk-lore has just been issued from the London Printing Press at Lucknow, the author being Rai Bahadur Mal Manucha, chairman of the Fyzabad Municipal Board, and well known in Oudh as a legal practitioner. In the preface he says that while he was enjoying the vacation at Hardwar on the Ganges, it occurred to him that if a few notes on religious beliefs, social customs, superstition and folk-lore, proverbs and sayings, puns, riddles, aphorisms, and other miscellaneous matters in common vogue among the Hindoo community generally, and among the country people especially, were brought together, they would "aid a great deal in throwing light upon the hitherto partially explored regions of the mode of life led by the common people." In a lengthy article on the little book the *Times of India* says the author has gathered together a little of everything that his preface promises. We learn, for instance, that if a person is drowned, struck by lightning, bitten by a snake, or poisoned, or loses his life by any kind of accident, or by suicide, then he goes usually to hell. If he die naturally on a bed or a roof, he becomes a "Bhut," or evil spirit, and with this belief care is taken on the approach of death to move the person carefully on to the floor. The earth is believed to be resting on the horn of a cow and the raised trunks of eight elephants, called "Diggai," or "elephants supporting the regions," and each of the cardinal and sub-cardinal points of the compass has its appropriate guardian. An eclipse is produced by the occasional swallowing up of the sun or moon by the severed head of Ráhu, son of a demon family, who was decapitated by Vishnu for disguising himself as a god and drinking nectar.

IN the thirteenth of his "Res Ligusticæ," recently published, Count Salvadori announces the occurrence of *Cypselus affinis* in Liguria on May 14 last. The Count gives full synonymy of the species, and an interesting account of the species on this its first visit to Europe.

IT has been known for some time that Dr. Loria was engaged in prosecuting zoological researches in the Papuan sub-region, and now two instalments of his collection have been described by Count Salvadori. The localities visited by the Loria expedition have been Pulo Penang, Timor Cupang, Pulo Semau, Port Darwin, and Port Moresby in South-Eastern New Guinea. Three new species have been discovered in the latter locality, and have been named by the author, *Ægotheles loriæ*, *Arses orientalis*, and *Pitta loriæ*, the last-named being the only species collected on the island of Su-a-u, a small islet near South Cape.

PROF. GIGLIOLI has just issued the second part of his "Primo Resoconto dei risultati della inchiesta ornitologica in Italia," the first portion of which we noticed last year. This second instalment is in the form of a goodly octavo volume of nearly seven hundred pages, and is entitled "Avifauna Locale." It consists of reports from the various provinces of Italy, furnished by different observers, with remarks as to the nidification, distribution, and migration of the various species. As to the value of these local lists there can be no question, and Prof. Giglioli may be trusted to choose men with a thorough knowledge of local ornithology to record the observations. As far as we can judge, Prof. Giglioli has been fortunate in his coadjutors.

THE problem as to the origin of the nephrite of which the tombstone of Tamerlane, at Samarcand, is made—a question which has interested a good many mineralogists—seems to have been definitely solved by M. Grombchevsky's visit to the nephrite-mines on the Raskem-daria, on the eastern slope of the Pamir. M. Grombchevsky found there a big dyke of nephrite, of extreme hardness, embedded in the rocky banks of the Raskem-daria, which consist in that place of white jadite. The Chinese used to extract the nephrite by lighting great fires on the rock, and afterwards throwing water on it when it was heated. They stopped these operations in the course of the present century, when the heir to the throne, after having slept in a bed made of Raskem nephrite, fell ill. A large piece of the stone, so much liked by the Chinese, which was on its way to Peking, was put in chains (like Yakoob-Beg's guns, which are still kept in chains at Yanghi-ghissar) and thrown on the road-side at Kutcha, where it remains. After a careful analysis of the samples brought by M. Grombchevsky, Prof. Mushketoff (in the *Izvestia* of the Russian Geographical Society, xxv., 6) comes to the conclusion that the Raskem nephrite and that of Tamerlane's tombstone are identical.

As to the white jadite in which the dark nephrite is embedded on the Raskem-daria, and which was extracted by the Chinese on the Tunga River, it is like the jade obtained in Burmah on the tributaries of the Irawadi, and described in a recent issue of the *Scottish Geographical Magazine*.

SOME curious results have appeared in an examination, by Herren Geisler and Ulitzsch, of school children in the (Saxon) Freiberg district, with reference to growth (*Humboldt*). Twenty-one thousand children (of both sexes) were measured. The boys, up to the eleventh year, were found to be about 0·6 to 0·9 cm. taller than the girls; but they were then overtaken by the girls; and this superiority of the girls continued till the sixteenth year, when the boys again grew more than the girls. This is against Quetelet's opinion, that boys are throughout bigger than girls.

THE Liverpool Geological Society has issued Part II. of the sixth volume of its Proceedings. Among the contents is an address by the President, Mr. H. C. Beasley, on the life of the English Trias. Mr. T. Mellard Reade contributes geological notes on an excursion to Anglesey; a note on a boulder met with in driving a sewer heading in Addison Street, Liverpool; and a note on some mammalian bones found in the blue clay below the peat-and-forest bed at the Alt mouth.

AT the meeting of the Linnean Society of New South Wales on June 25, Mr. Fletcher exhibited one living and several spirit specimens of *Notaden Bennettii*, Gthr., from three different localities—namely, Dandaloo, on the Bogan River (collected by Mr. A. Fletcher), Warren, on the Macquarie (collected by Mr. Thacher), and Narrabri (collected by Mr. Henry Deane). He remarked that though this toad has hitherto been rare in collections, it is at times not uncommon in its native haunts. In two of the localities above named he had been informed that during April and May of this year considerable numbers had appeared, though possibly the recently prevalent floods may have been concerned in bringing them prominently under notice. From what he had seen of living specimens in captivity, the animals were expert burrowers; and from what he had heard as to their avoidance of water, their comparatively sudden appearance, followed shortly afterwards by a noticeable diminution in numbers, he was inclined to think that the species perhaps resembled the American spade-foot toad (*Scaphiopus*) in keeping generally out of sight except during a short breeding period. Mr. Ogilby remarked that Mr. Helms, who is away on a collecting expedition for the Australian Museum, had recently sent down specimens of the same species from Bourke.

SOME habits of crocodiles have been lately described by M. Voeltzkow. Travelling in Wituland, he obtained in January last 79 new-laid eggs of the animal, from a nest which was five or six paces from the bank of the Wagogona, a tributary of the Ooi. The spot had been cleared of plants in a circle of about six paces diameter; apparently by the crocodile having wheeled round several times. Here and there a few branches had been laid, but there was no nest-building proper. The so-called nest lay almost quite open to the sun (only a couple of poor bushes at one part). The eggs lay in four pits, dug in the hard, dry ground, about two feet obliquely down. Including eggs broken in digging out, the total seems to have been 85 to 90. According to the natives, the crocodile, having selected and prepared a spot, makes a pit in it that day, and lays about 20 to 25 eggs in it, which it covers with earth. Next day it makes a second pit, and so on. From the commencement it remains in the nest, and it sleeps there till the hatching of the young, which appear in about two months, when the heavy rain period sets in. The egg-laying occurs only once in the year, about the end of January or beginning of February. The animal, which M. Voeltzkow disturbed, and saw drop into the water, seemed to be the *Crocodilus vulgaris* so common in East Africa.

IN the last official report from Gambia, the Colonial Surgeon has an interesting paragraph on native diseases. The natives of Africa, he says, who are world-renowned for their superstition, attribute all diseases to one of two causes: either they have been "witched," or some enemy has made "greegree" against them. Of the latter there are two forms: (*a*) the "greegree" that is administered to a person, and most usually consists of an infusion made of roots, leaves, or bark from trees supposed to have the desired properties; (*b*) the "greegree" that is prepared against a person. This is done with much ceremony, and the process is accompanied by incantations, recalling the scene of the "witches' cauldron" in "Macbeth." The treatment relied upon for cure, and much practised in the country, is to call in a man who is supposed to be a "doctor," who, after looking at the patient, sits down at his bedside and writes in Arabic characters on a wooden slate a long rigmarole, generally consisting of extracts from the Koran. The slate is then washed, and the dirty infusion is drunk by the patient. As a result of this state of ignorance and superstition, unqualified practice of every description is openly carried on, and drugs and poisons are daily sold by persons who are wholly ignorant of their properties, but who have acquired sufficient influence over ignorant patients to extort money.

THE *Deutsche Seewarte* has just published in a tabular form the results of the meteorological observations made on German and Dutch ships for the ten-degree square, lat. 20°–30° N., long. 30°–40° W., situated in the centre of the North Atlantic. This is, in fact, the eighth such square which has been similarly published in the last few years; the results for each month are grouped in one-degree squares, of which there are one hundred in each ten-degree square, and the observations for any part of such sub-square are so grouped as to be readily available for combination with the materials collected by other institutions. The winds are recorded under 16 points, with additional columns for variable winds, calms, and storms. Other columns include the means of the various data, the duration of rainfall, and remarks of special interest extracted from the logs used in the discussion. The volume contains xxvi. + 193 large quarto pages.

IN the third number of the *Sammlung von Vorträgen und Abhandlungen*, Prof. Foerster, the Director of the Berlin Observatory, has brought together seven lectures delivered by him in recent years to scientific societies and artisan audiences in Berlin and Hamburg, and various papers reprinted from *Himmel und Erde*, the Prussian *Normal Kalender*, and other sources. Four of the lectures have relation to standard time, the universal meridian, and the Washington Conference; others are included in the prediction of earthquakes and meteorological phenomena, luminous night clouds, the red skies which followed the Krakatão eruption, and Karl Braun's cosmogony. In a paper on the zodiacal light, it is held that, although the constitution of the light is still a matter of doubt, the evidence gained by means of the spectroscope and polariscope indicates that it consists not merely of sunlight reflected from bodies of a meteoritic nature, but also of innate light, due, probably, to electrical effects in a gaseous medium. A paper from the *Kalender* for 1891 contains an account of recent work done at Potsdam on the motion of stars in the line of sight, the instruments employed in the investigation being fully described. Prof. Foerster enjoys some renown in Germany as a popular exponent of scientific questions, and numerous reprints of his papers and discourses have appeared.

THE demand for techical education in New South Wales is rapidly increasing both in Sydney and in the principal centres

of population throughout the country. At present there are between 3000 and 4000 students enrolled, as against a total of 2200 this time last year. A tender has been accepted for the erection of a new technical college in Sydney, to cost £19,537, and the building is to be completed by March next.

THE American journal *Bradstreet's* in a recent article describes a school of manual training at Baltimore, which claims to be the pioneer public manual training school, as well as the only absolutely free school of the sort in the world. The school was opened in 1884 with sixty pupils and four instructors; now it has 549 pupils and fifteen instructors. It has had manual training for its chief object. The ordinary work of advanced public school grades is here but a department called the literary department. This is an essential difference from the manual training in so many schools, where it is one feature of many, and not the chief. It is not meant to teach trades, but rather the use of the tools used in all common trades, and the rudiments of mechanical industry. The regular course of the school takes three years, but there is a preparatory course of two years for the benefit of those who could not continue the ordinary public school course. All the students have their daily work in the shops, drawing-rooms, physical laboratory, and literary department. Each class has its own recitation-room, and only leaves it for drawing, shop, and laboratory work. In shop work the classes are limited to twenty-four boys, while in drawing and other studies double that number are instructed at a time. In the first year fifteen weeks are devoted to carpentry, five to wood-turning, and twenty to forging. In the second year fifteen weeks are devoted to pattern-making, five to moulding, fifteen to vise-work, and five to soldering and brazing. In all the shops instruction is given as to the care and use of tools, laying off and designing work, and the composition of the material used. Each class makes some special design for graduation, and the class this year is engaged on a ten horse-power dynamo, thirteen lathes, and a Gordon printing press. The dynamo will be set up, and is expected to furnish electricity enough to light both buildings with incandescent lights. The boys also do all the plating required, and make all repairs on the machinery in use.

THE additions to the Zoological Society's Gardens during the past week include an Ashy-black Macaque (*Macacus ocreatus*) from the East Indies, presented by Mr. W. J. Bosworth; a Two-banded Monitor (*Varanus salvator*) from the East Indies, presented by Captain W. J. Rule; a Wapiti Deer (*Cervus canadensis* ♀) from North America, an Aard Wolf (*Proteles cristatus*) from South Africa, two Patagonian Conures (*Conurus patagonus*) from La Plata, purchased; two Ariel Toucans (*Ramphastos ariel*) from Brazil, received in exchange; a Barbary Wild Sheep (*Ovis tragelaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on August 14 = 19h. 33m. 5s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4510	—	Pale blue.	19 37 45	+14 25
(2) G.C. 4514	—	Greenish-blue.	19 41 56	+50 15
(3) δ Sagittæ...	4	Yellowish-red.	19 42 29	+18 16
(4) β Aquilæ...	4	Yellow.	19 49 54	+ 6 8
(5) δ Aquilæ...	3·4	White.	19 19 54	+ 2 54
(6) 483 Birm.	7	Very red.	18 58 32	− 5 49
(7) S Scorpii	Var.	—	16 11 7	−22 37

Remarks.

(1) This is a small planetary nebula which gives the usual spectrum of three bright lines, in addition to a comparatively distinct continuous spectrum of considerable length. In further observations special attention should be directed to the character of the chief line, and maxima of brightness in the continuous spectrum should be looked for. It is not improbable also that many faint lines may be found with the improved instruments now in use. In the General Catalogue the nebula is thus described: "A planetary nebula; bright; very small; round."

(2) This is one of the so-called "nebulous stars" appearing in ordinary instruments as a star out of focus. The central nucleus gives a continuous spectrum, but the surrounding atmosphere gives a spectrum consisting of three bright lines. It would be a considerable advance in our knowledge if the spectrum of the nucleus could be determined. It may be that we are simply in presence of a star like those of the Pleiades, produced by the intersection of streams of meteorites, or it may be an ordinary case of condensation of a nebula. In the former case the spectrum would probably be that of a hot star, whilst in the latter case it would be one of an early group, possibly consisting of bright lines. Further observations are obviously required. The General Catalogue description is as follows: "A nebulous star; bright; pretty large; round; star of 11th magnitude in the middle."

(3) A bright star, with a well-marked spectrum of Group II. The bands 2, 3, 7, and 8 are strong, and 1, 4, 5 are well seen (Dunér). The usual observations for bright carbon flutings and absorption lines are required.

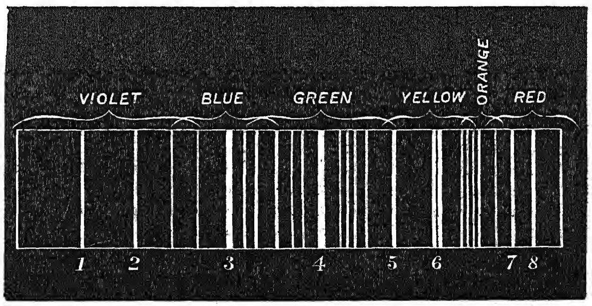
(4) and (5) Gothard states that these are stars of the solar type and of Group IV. respectively, the former being confirmed by Vogel. The usual more detailed observations are required in each case.

(6) This is a comparatively bright star of the rare type of Group VI., and offers a good opportunity for a detailed study of this kind of spectrum. In addition to the three usual carbon bands, it shows the secondary bands 4 and 5 (Dunér). The intensity of band 6 (λ 564), as compared with the other carbon bands, is not recorded by Dunér. The presence or absence of line absorptions should be particularly noted.

(7) The spectrum of this variable has not yet been recorded. According to Gore's catalogue, the period is about 177 days, and the magnitude ranges from 9·1–10·5 at maximum to < 12·5 at minimum. There will be a maximum about August 19.

A. FOWLER.

LIGHTNING SPECTRA.—Mr. W. E. Wood, in the current number of *The Sidereal Messenger*, gives the results of some observations of lightning spectra made on June 22, 1890. The results were obtained with a Browning direct-vision spectroscope of small dispersion, and having no scale, so that the lines mapped in the accompanying diagram are eye estimates.



With respect to the 25 bright lines shown it is remarked, "two moderately bright lines lie in the violet, one heavy bright line in the blue, and which I estimate to be the familiar F line, one brilliant line in the green (the coronal or auroral line?), one brilliant line on the yellowish-green, a double line in the yellow—very brilliant (the sodium line?), a fainter but fairly broad line on the edge of the red, and two very bright lines in the centre of the red, one of which I think is a hydrogen line. The fainter bright lines lie approximately as shown in the diagram. The intense flashes, those which usually do the damage during a storm, gave *exceptionally* faint, continuous spectra, and rarely more than the lines number 3, 4 and 5. Heat-lightning flashes gave the principal bright lines 1 to 8, and the spaces between were occupied by a multitude of finer bright lines. An absorption band in the violet occurred in all *bright* flashes of heat-lightning, and in some cases I saw two

such bands in the red, lying on either side of the pair 7 and 8. . . . It might be well to state that the line, which I judge to be the auroral line, was in all cases the most noticeable, and especially so in discharges of heat electricity, which seemed to occur in the upper and more rarefied strata of the air."

SOLAR ACTIVITY.—Prof. Tacchini gives the following results of solar observations during the second quarter of this year (*Comptes rendus*, August 4):—

	No. of days of observation.	Relative frequence of spots.	Relative frequence of days without spots.	Comparative area of spots.	Comparative area of faculæ.	No. of groups of spots per day.
April	19	2·08	0·75	1·40	10·40	0·44
May	20	2·55	0·54	2·58	25·83	0·71
June	26	1·35	0·76	0·86	8·10	0·25

A comparison of these figures with those of the first quarter of this year shows that the spots are slowly increasing in magnitude, and that the number of days without spots is diminishing.

The following results have been obtained for the prominence:—

	No. of days of observation.	Mean number.	Mean height. ″	Mean extent. ″
April	19	1·90	35·2	1·5
May	20	1·55	37·9	0·9
June	26	2·42	27·7	1·3

DENNING'S COMET (*c* 1890).—Dr. A. Berberich has computed the following orbit of the comet discovered by Mr. W. F. Denning at Bristol on the 23rd ult., from observations made at Nice on the 24th and 25th, and at Strasburg on the 27th (*Astronomische Nachrichten*, No. 2982):—

$$T = 1890 \text{ Sept. } 24{\cdot}7573 \text{ Berlin Mean Time.}$$

$$\left.\begin{aligned} \omega &= 158^\circ\ 26'\ 64'' \\ \Omega &= 96\ 35\ 42 \\ \iota &= 99\ 37\ 67 \end{aligned}\right\} \text{Mean Eq. } 1890{\cdot}0.$$

$$\log q = 0{\cdot}12288$$

$$\Delta\lambda \cos\beta = +0'{\cdot}08;\quad \Delta\beta + 0'{\cdot}06.$$

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl. ° ′
Aug. 14	15 22 56	+52 45·7
15	23 42	51 22·0
16	24 29	49 57·4
17	25 16	48 32·0
18	26 4	47 5·9
19	26 52	45 39·2
20	27 41	44 11·9
21	28 30	42 44·3

Brightness = 1·82 on August 17, and = 1·95 on August 21, that at discovery being taken as unity.

The comet will pass perihelion about September 25, at a distance of 1·33 the mean distance of the earth from the sun.

From the ephemeris given it will be seen that the comet is between β Boötis and θ Draconis on August 15.

GEOGRAPHICAL NOTES.

THE Russian *Official Messenger* of August 1 gives the following news about the work done by M. Grombchevsky during last spring. On March 13 the expedition left Khotan for Niya. After having passed through the oasis of Keria, the travellers crossed the desert, where they met with a succession of *barkhans* (downs), reaching to the unusual height of 200 feet. From Niya they visited the Sougrak gold-mines, which are worked by nearly 3000 families living in caverns excavated in the loess and conglomerates on the slopes of the hills. Lumps of gold 2 lbs. in weight are sometimes found in these mines. Leaving Niya, the expedition crossed the border-ridge, which consists of several chains—the passes across them attaining heights of from 10,500 to 11,000 feet—and reached Polu, whence it returned to Keria. There M. Grombchevsky received the good news that the expedition would be allowed to continue its work till January 1, 1891, and that £200 had been granted for that purpose; so that M. Grombchevsky made arrangements to start for Rudok, in Tibet, in the first half of May.

THE following telegram about M. Grombchevsky's expedition, dated Marghilan, July 19, has appeared in the Russian *Official Messenger*. The expedition had reached Polu, but had been stopped there by the Chinese authorities, who insisted upon the immediate return of the expedition to Kashgar, and ordered the population to leave their settlements and to camp in the mountains. Brought to despair, M. Grombchevsky spent his last money in bribing some inhabitants, and, without a guide, left Polu in the night of May 17, going further south into the depth of the unexplored wilderness.

THE last number of the *Izvestia* of the Russian Geographical Society is of unusual interest, especially on account of its maps. It contains three reduced photographic copies of the hypsometric map of Russia, by General Tillo, and it is impossible not to admire the distinctness with which the two chief lines of upheaval, the south-west to north-east direction, and the north-west to south-east direction, appear on this map, even amidst the plateaus and the depressions of middle Russia. Another interesting map renders, on a scale of 7 miles to the inch, the surveys of M. Grombchevsky, made during his recent attempts to reach Tibet from the north. The map is accompanied by two letters from the explorer, written in December 1889, at the sources of the Khotan-daria and the Kara-kosh. The same issue contains a letter from the chief of the Tibet expedition, M. Roborovski, dated from Niya, December 11, 1889; a paper on the geodetical surveys in Russia; and a most interesting summary, by M. Kuznetsoff, of his several years' study of the flora of the Caucasus.

IN a communication to the Société de Géographie of Paris, M. G. Marcel, who is one of the librarians of the Bibliothèque Nationale, has given some particulars of Louis Boulanger, an astronomer, geometer, and geographer of the sixteenth century. In 1511 he published at Lyons a work, "Equatorii Cœlestis Motus," of which only one copy is known. It is in the Bibliothèque Mazarin, and is described by M. Marcel as hitherto ignored by bibliographers. In 1514 he brought out a piracy of Muller's "Cosmographiæ Introductio." The globe accompanying this is regarded as the first on which the word "America" is found. Another globe has been found by M. Marcel at the Bibliothèque Nationale, which he regards as having been made by one of the school of Schoener between 1513 and 1518, and on it the then new name of the New World occurs four times. It is therefore either the first or the second cartographic document in which America is mentioned.

THE SCIENTIFIC PRINCIPLES INVOLVED IN MAKING BIG GUNS.[1]

III.

PART III.—WIRE GUN CONSTRUCTION.

AN inspection of Fig. 5 (p. 307), and of the serrated edge of the curve of circumferential tension, *t*, shows that only the inner fibre of each coil is doing its full share of resistance when the gun is fired.

Great economy of material can be effected if we can make all the circumferential fibres take up a full uniform working tension (say of 18 tons per square inch) when the gun is fired; but to secure this condition only approximately, the number of coils would have to be largely increased, and the cost, complication, and time of manufacture of a gun would be enormous.

But, by adopting Mr. J. A. Longridge's plan of strengthening the inner tube A by steel wire, wound round with appropriately varying tension, we are theoretically able to make the curve of circumferential firing tension, *t*, a straight line for a determinate powder pressure; and now all parts of the wire coil are equally strained, and take an equal share in the resistance.

The subject has been investigated theoretically by Mr. Longridge, assisted by Mr. C. H. Brooks, beginning in 1855; and his theories are set forth in papers in the Proceedings of the Institution of Civil Engineers in 1860, 1879, 1884, em-

[1] Continued from p. 334.

bodied in Mr. Longridge's "Treatise on the Application of Wire to the Construction of Ordnance," 1884 (Spon); and again in a paper in 1887, "Further Investigations regarding Wire Gun Construction."

Dr. Woodbridge, of America, claims to have originated the system of strengthening guns with wire, in 1850; but to Mr. Longridge belongs the credit of pointing out the proper mode of winding on the wire with initial tension so adjusted as to make the firing tension of the wire uniform for the maximum proof powder pressure.

Mr. Longridge's principle is applicable not only to engines of destruction, but also to peaceful purposes, such as strengthening the cylinders of hydraulic presses and lifts, and the copper pipes of steam-engines; for which a great, and, we hope, a profitable future is in store.

Returning to the application of the principle to artillery, the great object attained is the notable reduction in weight of the gun—a matter of importance in siege artillery, where the weight of the largest single piece of metal, the gun itself, is limited by the difficulty of transport over bad roads and rough country. By the use of Mr. Longridge's principle, the weight of a howitzer can be reduced from five tons to three and a half—quite sufficient to make all the difference between getting the gun into position, or being compelled to leave it behind.

It is also claimed as an advantage of the wire gun that the construction will be found cheaper and more expeditious, when once the appropriate machinery is erected; and that this machinery need not be nearly so elaborate and expensive as that required with the present system of construction with steel coils shrunk on over each other.

As we have seen in Part II., the appropriate initial state of stress is, in the coil gun, dependent on such delicate fitting as thousandths of an inch, and a slight irregularity in the texture of the metal may be sufficient to completely modify the initial stresses as designed. With the wire gun, on the other hand, the wire can be coiled on to the inner tube from an equal parallel coil of wire, and the appropriate tension given by means of a certain weight running on the free part of the wire, and incidentally testing the strength of the wire. Certain practical difficulties exist in securing the ends of the wire, and in providing for longitudinal strength, which experience will doubtless soon overcome.

Besides Mr. Longridge's "Treatise," the most important is a long article in the *Revue d'Artillerie*, on "Steel Wire Guns," by Lieutenant G. Moch, since published as a separate book, and also translated in the American "Notes on the Construction of Ordnance," No. 48, 1888.

Lieutenant Moch resumes Longridge's and Brooks's calculations, and presents the mathematical work in a more concise and elegant form; he applies his formulas to the design of the wire guns, proposed in 1871 by Captain Schultz, who was unaware of Mr. Longridge's previous work.

We shall attempt here to present the essence of Lieutenant Moch's article in a concise and geometrical form, depending on the method and formulas of Parts I. and II., and illustrated by the design of one of Schultz's guns; referring the reader who wishes to pursue the subject in all its practical details to Moch's original article, and to Longridge's "Treatise."

(44) Taking the cross-section of the gun across the powder-chamber, as composed of the inner tube, A, the wire coil, B,

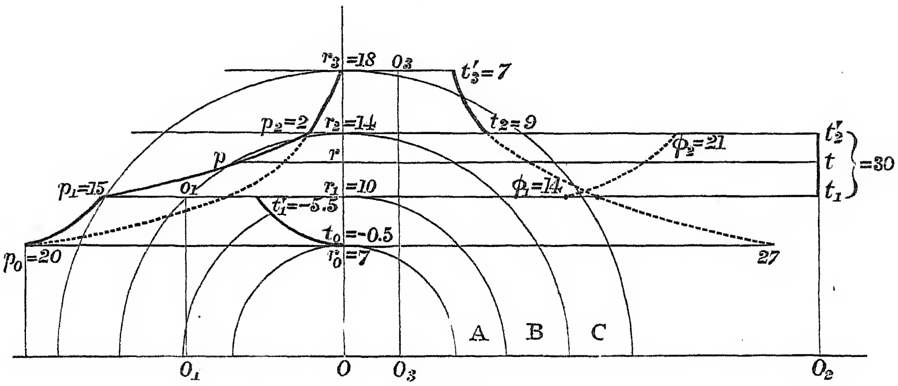


FIG. 9.

and an outer jacket, C, then in the ideal state, the firing stresses will be represented in Fig. 9, where the curve of circumferential tension, $t_1tt'_2$ is a straight line in the wire coil B.

The outer jacket, C, is merely required for protection of the wire from damage by shot, so that it may be supposed fitted over the wire without any appreciable shrinkage; when the gun is at rest, the jacket C will then be in a state of repose free from stress; but when the gun is fired, we may suppose the stresses in C to be the powder-stresses (§ 12, p. 306), on the assumption that the gun behaves as if homogeneous.

We denote by r_0 the internal radius of the tube, by r_1 and r_2 the internal and external radii of the wire coil, and by r_3 the external radius of the jacket, all measured in inches in our units.

Then in the jacket C the curves t'_3t_2 of circumferential tension and r_3p_2 of radial pressure, representing firing stresses, will be Barlow curves, the reflexions of each other in their medial axis O_3o_3.

(45) The continuation of the Barlow curve r_3p_2 in the dotted line up to p_0 will give graphically the powder pressure p_0; but now the curve of firing radial pressure between r_2 and r_0 will be the broken curve $p_2p_1p_0$, of which p_1p_0 in the tube A is the portion of another Barlow curve, but of which p_2p_1 in the coil B is easily seen to be a portion of a hyperbola.

For the curve of firing circumferential tension in the wire being the straight line tO_2, the condition of equilibrium of any cylindrical portion of the wire coil, bounded internally by the radius r, requires that the rectangle r_2t of circumferential resistance should be equal to the rectangle Op – rectangle Op_2; or, in other words,

$$\text{the rectangle } p_2t = \text{rectangle } Op,$$

or

$$\text{the rectangle } O_2p_2 = \text{rectangle } O_2p;$$

which proves that the curve p_2p is a hyperbola, with O_2O and O_2t as asymptotes.

(46) The tangent at any point of this hyperbola—say at p_2—is drawn by joining the point p_2 with points on O_2O or O_2t at double the distance of p_2 from O_2t or O_2O, by a well-known property of the hyperbola.

But to draw the tangent at p_2 of the Barlow curve r_3p_2, we must join p_2 with a point on O_3O at a distance from O_3 treble the distance of p_2 from O_3o_3.

Similarly we can draw at p_1 the tangent to the hyperbola p_2p_1, and the tangent to the Barlow curve p_1p_0, when we know the position of O_1o_1, the axis of this Barlow curve, p_1p_0.

(47) The position of O_1o_1 is fixed by the condition that the curve of circumferential tension in the tube A is the reflexion of the curve p_1p_0 in O_1o_1; and the position of this curve of circumferential tension, $t_0t'_1$, is settled by the condition that the rectangle Op_0 is equal to the sum of the areas of circumferential resistance, bounded by $t_2t'_3$ in the jacket C, by the straight line $t_1t'_2$ in the wire coil B, and by the curve $t_0t'_1$ in the tube A.

(48) It will be noticed in the diagram that, with the numbers given there, the curve $t_0t'_1$ lies to the left of the line r_0r_1, showing

that when the gun is fired the interior of the tube is still in a slight state of compression, so that the circumferential firing stresses of the tube are insignificant pressures, the chief stress being thrown upon the wire.

This theoretical result appears to be of great practical advantage in prolonging the life of the gun, as it is found that the tube of the wire gun has hitherto shown an unexpected vitality; a very gratifying result, when it is considered how short the life of our large guns is, in consequence of the erosion of the bore.

An empirical formula, N = 2400 ÷ c − 50, given by General Maitland (Proc. I.C.E., vol. lxxxix. p. 205) for the life of a gun, where c denotes the calibre in inches, and N the number of full charges that can be fired before the gun requires relining, will illustrate forcibly the comparative longevity of large and small guns: thus, if $c = 16$, N = 100; if $c = 12$, N = 150; but if $c = 0{\cdot}3$, as in the new magazine rifle, N = 7950.

We have now determined graphically the firing stresses in the wire gun, where the powder pressure, p_0, is exactly adjusted, so as to produce uniform t in the wire; a less powder pressure would obviously strain the inner fibres less, and less than the outer fibres; *vice versâ*, a powder pressure greater than p_0.

(49) But now the gun-maker has to determine the initial stresses in his gun from the above state of firing stress, by the operation of stripping off the powder stresses, assuming the gun to behave as if homogeneous.

As a first consequence, the initial stresses in the jacket C will be reduced to zero, as they should be; because we have supposed the jacket C slipped on with merely a mechanical fit.

Secondly, in the wire coil B, the state of initial circumferential tension will be obtained by subtracting the ordinates of the prolongation of the Barlow curve t'_3t_2 from the ordinates of the straight line t'_2t_1; whence we obtain the symmetrical Barlow curve $\phi_2\phi_1$, by reflexion of the Barlow curve t'_3t_2 . . ., produced.

The curve of radial pressure $r_2\varpi_1$ in the wire coil B, obtained by subtracting the ordinates of the Barlow curve p_2p_0 from the hyperbola p_2p_1, is now easily plotted, but is of a more complicated analytical character.

Finally, we come to the state of initial stress in the tube A, obtained also by stripping off the powder stresses from the firing stresses; and consisting of the curve of initial radial pressure ϖ_1r_0, a Barlow curve, and its reflexion, $\tau_1\tau_0$, the curve of circumferential pressure in the tube A; the position of $\tau_1\tau_0$ being settled so as to make the area $r_1\tau_1\tau_0r_0$ equal to the area $r_1\phi_1\phi_2r_2$; and now the state of initial stress is represented in Fig. 10.

(50) We notice that τ_0 is considerable, and may with imperfect design become dangerously near the crushing pressure of the material of the tube A; practically, however, the great crushing pressure τ_0 is considered advantageous, as tending to improve the resisting power of the material against the great enemy, erosion.

In the Severn tunnel, as a different exemplification of these principles, the crushing effect in the brick tube, due to the head of water of the land springs, was not allowed for sufficiently; if the land water around the tunnel is not kept down by pumping, the head of water soon becomes sufficient to cause the bricks on the interior of the tunnel to crush and splinter; and until the interior is strengthened considerably with steel or cast-iron curbs, the expense of pumping cannot be avoided.

(51) There is considerable divergence of opinion as to the proportions to be given to the tube A and the wire coil B; Longridge preferring a comparatively thin tube, A, of some softer material, like cast-iron, while Schultz made his tube of steel, and considerably thicker in proportion, with the advantage of throwing the longitudinal strength into the tube.

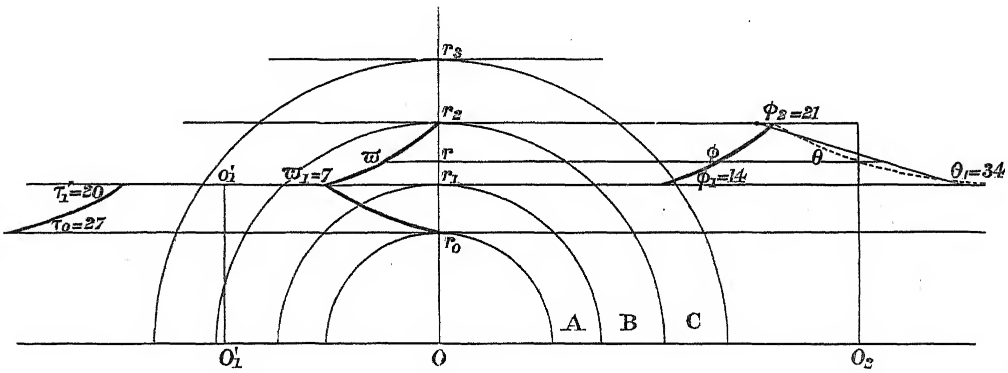


FIG. 10.

As the theory is considerably simplified if we take the tube A and the wire of the coil B of the same elasticity, we shall make Fig. 9 represent the design of one of the Schultz guns, as described by Moch, altering the dimensions and stresses to round numbers in inches and tons.

Now Figs. 9 and 10 represent the section across the chamber of the Schultz 34-centimetre (13½-inch) gun, in which we have made $r_0 = 7$, $r_1 = 10$, $r_2 = 14$, $r_3 = 18$, in inches, to the nearest integer.

(52) We assume that, under a powder-pressure, p_0, of 20 tons on the square inch, the wire coil is under a uniform circumferential tension of 30 tons on the square inch; a very moderate estimate for what steel wire is capable of sustaining, as 60 would not be excessive.

Numerical calculation by means of the formulas of Part I. gives the following values of the stresses, in round numbers:—
$p_2 = 2$, $p_1 = 15$; $t'_3 = 7$, $t_2 = 9$, $t'_2 = t_1 = 30$, whence $\phi_2 = 21$, $\phi_1 = 14$; $t'_1 = -5{\cdot}5$, $t_0 = -0{\cdot}5$, all in tons per square inch.

In Fig. 10 the initial stresses are represented; and we find, as before, $\phi_2 = 21$, $\phi_1 = 14$, $\varpi_1 = 7$, $\tau'_1 = 20$, $\tau_0 = 27$, while the initial stresses in the jacket C are *nil*.

(53) There still remains an important practical detail to be settled theoretically—the formula for the varying tension with which the wire must be wound on the tube A, in order that when the coil is complete the curve of initial tension of the wire should become finally $\phi_1\phi_2$.

The formula has been investigated in all its generality by Mr. Brooks in Longridge's "Treatise," but we shall follow Moch in his article in considering the very much simplified case of uniform modulus of elasticity.

As we have already used the word *initial* to distinguish the stresses in a gun in a state of repose when finished, we shall call the varying tension with which the wire is wound on the gun the *winding tension*, and denote it by θ, in tons per square inch.

(54) Now, to determine θ for any radius, r, of the coil B, Moch assumes that the winding tension of the wire is equal to the initial tension, ϕ, increased by the circumferential tension (pressure) due to the initial radial pressure, ϖ, at the radius r, acting on the partly finished tube and coil between the radii r_0 and r; and thus

$$\theta = \phi + \varpi\frac{r^2 + r_0^2}{r^2 - r_0^2}.$$

In other words, it is assumed that the tension of repose, ϕ, is less than the winding tension, θ, by the amount due to the pressure ϖ at a radius r, and zero pressure at the radius r_0, treating the material as homogeneous.

Now, by the formulas of § 7 (p. 305),

$$\phi = t - p_0 \frac{r^{-2} + r_3^{-2}}{r_0^{-2} - r_3^{-2}},$$

$$\varpi = p - p_0 \frac{r^{-2} - r_3^{-2}}{r_0^{-2} - r_3^{-2}}$$

$$= (p_2 + t)r_2 r^{-1} - t - p_0 \frac{r^{-2} - r_3^{-2}}{r_0^{-2} - r_3^{-2}},$$

where

$$p_2 = p_0 \frac{r_2^{-2} - r_3^{-2}}{r_0^{-2} - r_3^{-2}},$$

so that

$$\varpi = t(r_2 - r)/r - p_0 \frac{r^{-2} - r_2^{-2}}{r_0^{-2} - r_3^{-2}};$$

and the expression for the winding tension, θ, finally reduces to the form—

$$\theta = A + \frac{L}{r} + \frac{M}{r - r_0} + \frac{N}{r + r_0},$$

where

$$A = \frac{p_0 r_0^2 (r_3^2 - r_2^2)}{r_2^2 (r_3^2 - r_0^2)} = p_0 \frac{r_2^{-2} - r_3^{-2}}{r_0^{-2} - r_3^{-2}} = p_2,$$

$$L = -tr_2,$$

$$M = t(r_2 - r_0) - p_0 r_0 \frac{r_0^{-2} - r_2^{-2}}{r_0^{-2} - r_3^{-2}},$$

$$N = t(r_2 + r_0) + p_0 r_0 \frac{r_0^{-2} - r_2^{-2}}{r_0^{-2} - r_3^{-2}},$$

after considerable algebraical reduction.

(55) A great simplification is introduced if we put $r_3 = r_2$, equivalent to supposing that the jacket C fits loosely over the coil B, so that the firing stresses do not extend into the jacket C, which, therefore, now contributes nothing to the strength of the gun; and now $A = 0$, $L = -tr_2$, $M = tr_2 - (t + p_0)r_0$, $N = tr_2 + (t + p_0)r_0$; and we thus obtain the formula (51) of Longridge's "Treatise," or formula (50) of Moch's article.

With the numbers of Fig. 10, we find $\theta_1 = 34$, while obviously we always have $\theta_2 = \phi_2$, as the winding tension of the last layer of wire is the same as the tension in repose.

Having plotted out by points the curve $\theta_1\theta_2$ for the winding tension θ, a curve of the fourth degree, it will be found practically correct enough to replace it by the most approximate straight line; and now in winding the coil, the difference of the tension weights destined for two consecutive layers of wire remains constant.

(56) We have now finished the theory of the wire gun, so far as the circumferential strength is concerned; and for its experimental verification, an interesting article in Note No. 38, on the Construction of Ordnance, "On Winding and Dismantling an Experimental Wire-wound Gun Cylinder," by Lieutenant W. Crozier (Washington, June 1886), may be consulted; and according to recent reports a 10-inch gun has been recently constructed in America by Captain Crozier, on designs based upon his experimental results.

The theory of the longitudinal stresses in the wire gun has not been touched upon, because it is still a point of dispute as to whether the tube alone should provide the longitudinal strength, or whether it should be partly borne by the outside jacket, the wire coil being obviously unable, except in Canet's double coning system, of giving any assistance in this direction.

Mr. Longridge's principle of strengthening a tube with wire, wound with appropriately varying tension, will be found useful in peace and in war: he can claim credit that a gun strengthened on this principle, the 9·2-inch wire gun, was chosen from its great strength to test the extreme range of modern artillery in 1888, with what were called the "Jubilee rounds"; when, with an elevation of about 40°, a range of 21,000 yards, or 12 miles, was attained, the projectile weighing 380 pounds, and the muzzle velocity being about 2360 f.s.

The dimensions in the diagrams have been purposely taken in round numbers, so as not to represent invidiously any particular gun; in some cases, inappropriate stresses have made their appearance; and now it is the art of the gun-designer to modify slightly the dimensions of the parts of his first rough sketch, so as to attain to more uniformity of strength and a better theoretical result.

There is no claim to originality in the theory that has been given above, and we fear that due credit has not always been properly assigned to the right investigator; but the attempt has been made to present the essential points of the theory in as simple a form as possible, with a minimum recourse to algebraical formulas. The subject has been written about so much of late years that the reader is apt to be confused with the variety of notation and treatment; and it is hoped that the graphical method presented here will enable the theorist to present his results to the practical gun-maker in a more intelligible and convincing form.

A. G. GREENHILL.

ON PUTREFACTIVE ORGANISMS.[1]

THE author said his difficulty was to decide in which way to treat his subject. He might summarize the investigations of twenty years, and endeavour to show the original motives which led to their being undertaken, and then contrast this with the new meaning which has been derived from the investigations founded on recent methods and instruments; or, secondly, he might show the results of a series of continuous observations on certain saprophytic organisms placed under increasingly adverse environments, so as to endeavour to discover their behaviour in regard to the great Darwinian law. He inclined to this last as the view of his work that might have the broadest interest to a Society like that he was addressing; but the value of the improvements in recent lenses led him to give the priority to the results so obtained. In the case of larger animals, it was well known that a change of environment produced changes in the organism; but that these changes were hard to follow up, owing to the few generations that come under the notice of the student or observer. But in the case of micro-organisms the generations succeed each other so rapidly that it is easy to follow the changes produced by environment. He could show the effect on certain micro-organisms of a gradual change of temperature, and how in from seven to eight years an organism arose which lived and multiplied at a temperature of 157° F., whose ancestors had lived at a temperature of 65° F., and would have died if exposed to temperatures above 100°. He said there was nothing harder than to carry an audience to a just appreciation of the lower forms of life, but nevertheless he hoped to point out some of the practical results due to the improvements in modern microscopes. If they took a glass of drinking-water and put in it some shreds of fish, or any other organic substance, it soon became turbid and charged with the minutest organisms. To illustrate the number of these organisms, Dr. Dallinger said that visible to the human eye in the heavens there were in all probability with our most powerful modern telescopes 100,000,000 stars; and if they supposed that each of these, like our sun, was attended by eight primary bodies and twenty secondary planets, there would be two thousand eight hundred millions of bodies in space accessible to human research. The same number of these minute organisms to which he had referred would lie in a space equal to one ten-thousandth of a cubic inch. Any such a molecule of even dead matter must arrest the attention of the human mind; but when we remembered that these were complex *vital* forms, they had a significance of a high order, and their inconceivably rapid multiplication would make the mind pause and think. A decomposing mass of matter was a mass of beings endowed with life, and producing definite products. The life of the organism was not even an incidental product, the organisms were there for a purpose. They break up the decomposing organic matter into its elements, and so make it ready again for the purposes of life. Dr. Dallinger went on to describe some of the organisms which he has observed and examined. He said, that if they took some putrescent fluid from different putrefactive material, and mixed them, then put a very minute quantity of sterilized fluid on the microscope slide, and put into this the point of a needle which had been inserted into the mixture of putrefaction, and examined it with a sufficiently powerful microscope, the field of view in the microscope became, as it were, charged with life in an instant. There were many kinds of organisms, and they had many movements. There were rod-shaped organisms, spiral forms, a perfect oval form with two flagella, or whips. Another would be like the calyx of a papilionaceous flower, and have four flagella. Another would have a delicate egg-shape, and another be shaped like a double-convex lens, and move with a beautiful wave motion. The fluid speck seen under the microscope was densely peopled. What were these organisms, and what their functions amid the denizens

[1] Abstract of an Address delivered before the Bristol Naturalists' Society, by the Rev. W. H. Dallinger, F.R.S.

of earth? They were extremely small, and the largest of them so small that one hundred millions could be packed within a cube whose side was equal to the diameter of a coarse human hair, and there were from ten to twenty less than this. This group were amenable only to the most powerful microscopes. It was known long ago that they carried on putrefaction; now they knew that the process was a fermentation. Dr. Dallinger then went on to contrast ordinary saccharine fermentation, like that of yeast, producing carbonic acid and alcohol, with the fermentation produced by these saprophytic organisms, and showed that both could be prevented by taking care to keep away any of the germs of the fermentation, that both could be arrested by the action of heat, and that both tended to break up the organic matter into simpler forms. In the case of the saprophytes, water and carbonic acid were produced eventually from the decaying mass on which they dwell, and thus by the vital functions of these organisms the chemical elements in the animal body were restored to nature, to become once more part of the protoplasm of living things. There were, however, two things in which these saprophytic ferments were different from ordinary ferments; in the latter a special organism produces a special product, whereas in the former there was no such definite product, and in the saprophytic ferment the final process was produced, not by one definite organism, but by a series of organisms. He did not think that these ferments destroyed one another; but between the beginning and the end of the putrefaction there was a definite incoming and disappearance of many forms. In from 50° to 60° north latitude, he believed these organisms were limited to ten forms, and of these eight were definitely determined, and their life-history made out. There were some present everywhere, and they acted at once. Dr. Dallinger said the object of his study was biological, and not pathological. Some of the results he discovered some time ago, but the large progress of recent years was due to the great improvements in our instruments. These organisms were all different, no two of them behaved alike. He said that if they added a very small quantity of putrescent fluid to a speck of water on a slide kept at 65° F., it was very easy to find some of these organisms almost directly, using a lens magnifying 1000 diameters; and they would be found to increase with a rapidity that no description could suggest. He then showed on the screen the first kind of organism that appears, and mentioned that when seen in reality, they were in a constant state of movement, and that the saprophytic ferment begins to split up and break down the organic tissues. This first organism, *Bacterium termo*, would produce profound changes in the putrefying tissues, and prepare the way for other organisms. It would be seen that this organism would be densest round the mass that was being broken up, forming a felt-like covering or garment to it; soon a new organism of a spiral form would make its appearance (this was shown on the screen), while *Bacterium termo* would become less abundant, and be diffused over the entire fluid. The new one, like *Bacterium termo*, would be densest next to the putrid matter, and would form a covering to it. The decaying tissue would now rapidly change, and would give off noxious gases. This form would continue for an indefinite time, and be succeeded by one or two new forms. (These were shown on the screen.) One of these new forms would have a single flagellum, and the other would have two; and they would move rapidly about and glide continuously over the decomposing matter. They increased very rapidly, one method of increase being by a process of division. In another method two bodies would unite together, and an amœboid condition ending in the *fusion* of two forms resulting in a sac from whence spore was produced, giving rise to new generations. Their rate of increase was inconceivably rapid, and it was not surprising that the putrid tissue was surrounded by a garment of these organisms. They had in all probability their food and suitable conditions for their life produced by the functions of their predecessors. Then a time came when this form died out, and a very remarkable organism appeared which also invested the putrid matter with a garment of living organisms; they stuck to the mass and waved to and fro. These were shown on the screen as they would be seen in the microscope, clustering round the matter. With this was shown the next organism—a most wonderful one. It has a rigid flagellum armed with a hook and a long trailing flagellum. The animal swims about, and when it comes to a piece of decaying tissue, it often anchors itself by the trailing flagellum, which is coiled into a spiral; then it darts up and down upon the decaying matter. The action of this was shown by a mechanical slide, the up-and-down motion and the coiling and uncoiling of the flagellum being seen. These were succeeded by a group which had a free flagellum without any hook, and which fastens itself down by means of its trailing flagellum, and hammers the decomposing tissue by throwing itself against it. This process was also shown on the screen by means of a mechanical slide. Dr. Dallinger said that this occurred at about the middle of the putrefactive action, the greater part of which is accomplished by this. The mass now gradually broke up. The next kind, which was also shown on the screen, and its process explained by a mechanical slide, has two trailing flagella by which it anchors itself; it then springs up and darts down, and further promotes the decomposition. At the close of this stage there is little left of the original tissue but some water charged with carbonic acid, and a slight deposit of fragments. Dr. Dallinger said that four years ago he found a new organism which acts as a gleaner, and gathers up the fragments of the *débris* left by the others. It is armed with six flagella, and swims about in the liquid, and when it comes within a certain distance of the solid remnants twists its middle flagella together, and springs up and down on the *débris*, removing entirely tiny particles. They move in a most beautifully rhythmic manner up and down. He showed a picture of these on the screen, and also a mechanical slide of a group of three, with their pretty rhythmic action. And thus the organic tissues were broken up into their ultimate elements. Dr. Dallinger mentioned that the last form was comparatively rare, and was more frequent in warm countries. It was clear, he said, that different climates had somewhat different forms. In conclusion, he said that twenty years ago, when in a state of ill-health, he took to this research, and found all these beauties and a thousand times more; and he urged those present to take up some field of microscopical research, and seek for the hidden beauties of Nature. They would find much pleasure in the doing of it. They need not be appalled by the high powers he had used; there were many facts to be found by the help of far lower powers. If they did this they would find that life would have a pleasure it had never known before.

HIGHLAND PLANTS FROM NEW GUINEA.

As we have already noted, Baron von Mueller contributes to the Transactions of the Royal Society of Victoria (vol. i., Part 2) some important records of observations on Sir William MacGregor's highland plants from New Guinea. The following are his general conclusions:—

"The memorable expedition, so valiantly and circumspectly carried out by His Excellency Sir William MacGregor, the Governor of British New Guinea, for the ascent and exploration of the Owen Stanley's Ranges, has for the first time brought also the flora of the temperate and the sub-Alpine zone of that great island within the reach of elucidation. In a brief preliminary report, written in July last, attention was drawn to the extraordinary commigration, by which plants of Asiatic, of far southern and even of sub-Antarctic types had mingled together in the Papuan highlands. From the material thus brought together only a commencement could be made to study the vegetation of the higher mountains regarding geographic points of view; in order to obtain a full insight into the Papuan Alpine flora, it would require to explore the hitherto inaccessible more central culminations in the island, where on tiers still some few or perhaps several thousand feet higher in yonder latitudes, according to varied physical conditions, a glacier flora would be more fully reached. To form extensive conclusions on the nature of the Papuan Alpine flora would at present be premature; but from what we have now seen, it promises to be eminently interesting. On this occasion I shall merely group these highland plants on geographic principles, with a hope that it may yet fall to my own share to carry on these comparisons more amply at some future time from fuller material, the total sub-Alpine and Alpine flora of New Guinea in all likelihood comprising several hundred species of vascular plants. Such future researches will be to myself all the more fascinating, as from 1853 to 1855 the whole flora of the Australian Alps became elucidated by field-work of my own, it being utterly unknown before. In these pages is alluded only to those plants, which Sir William MacGregor gathered in altitudes between 8000 and 13,000 feet, therefore in the region above the mountain zone, involved in almost permanent clouds.

"Of the 80 plants, specifically and distinctly recorded in these

pages as emanating from the most elevated regions, nearly half the number seems endemic, so far as hitherto can be judged, while not yet all the highlands of South-Eastern Asia are explored, and while we yet remain in uncertainty about the constancy of some of the characteristics on which the adopted new specific forms are systematically established. Of these restricted Papuan plants, two—namely, *Ischnea elachoglossa* and *Decatoca Spencerii* represent new genera, the one allied to the exclusively Italian Nananthea, the other to the Australian and chiefly Alpine Trochocarpa. Of the other endemic plants 17 are of Himalayan types—namely, *Hypericum Macgregorii*, *Sagina donatioides*, *Rubus Macgregorii*, *Anaphalis Mariæ*, *Myriactis bellidiformis*, *Vaccinium parvulifolium*, *V. amblyandrum*, *V. Helenæ*, *V. Macbainii*, *Gaultiera mundula*, *Rhododendron gracilentum*, *R. spondylophyllum*, *R. culminicolum*, *R. phæochiton*, *Gentiana Ettingshausenii*, *Trigonotis Haackei*, and *T. oblita*, though some of these show also a touch of the Sundaic vegetative element; and here at once may be alluded to the extensive display of Ericaceous (inclusive of Vaccinaceous) plants, which forms of vegetation are in Australia so very scantily developed, and then only in Alpine regions. Contrarily, however, we now perceive otherwise almost a preponderance of upland Australian or New Zealandian or sub-Antarctic types in the highlands vegetation of New Guinea, so far as already revealed; this is demonstrated by the endemic occurrence of *Ranunculus amerophyllus*, *Metrosideros Regelii*, *Rubus diclinis*, *Olearia Kernotii*, *Vittadinia Alinæ*, *V. macra*, *Veronica Lendenfeldii*, *Libocedrus Papuana*, *Phyllocladus hypophyllus*, *Schœnus curvulus*, and *Festuca oreobaloides*; furthermore this repetition of the features of the southern flora so far north is rendered still more expressive and significant by the occurrence of numerous plants absolutely identical with our southern species—namely, *Epilobium pedunculare*, *Galium australe*, *Lagenophora Billardièrii*, *Styphelia montana*, *Euphrasia Brownii*, *Myosotis australis*, *Sisyrinchium pulchellum*, *Astelia alpina*, *Carpha alpina*, *Carex fissilis*, *Uncinia riparia*, *U. Hookerii*, *Agrostis montana*, *Danthonia penicillata*, *Festuca pusilla*, *Lycopodium scariosum*, *Gleichenia dicarpa*, and *Dawsonia superba*—most of these being now shown for the first time to approach so near to the equator. Four Borneo plants, hitherto only known from lofty altitudes of Kini-Balu, have now been traced to the Papuan highlands also, viz. *Drimys piperita*, *Drapetes ericoides*, *Rhododendron Lowii*, *Phyllocladus hypophyllus*, three being of far southern type. Even a few of such British plants, not almost universally cosmopolitan, have now come like messengers from home before us from New Guinea as there also indigenous; thus, *Taraxacum officinale* and *Scirpus cæspitosus*, these being wanting even in the Malayan islands and in continental Australia, irrespective of the widely distributed *Aira cæspitosa*, *Festuca ovina*, *Lycopodium clavatum*, *L. Selago*, and perhaps *L. alpinum*, as well as *Hymenophyllum Tunbridgense* and *Aspidium aculeatum*. For the familiar northern genus Potentilla a truly indigenous position in the southern hemisphere has been gained now for phytogeography, as well as for Myriactis and Trigonotis, while Astelia, Uncinia, and Dawsonia are now seen to enter equinoctial regions in the eastern hemisphere. The *Styphelia montana*, the Astelia, and the Carpha mentioned indicate the commencement of a truly Alpine flora.

"On the Finisterre Range, the ascent of which was accomplished by Mr. Zoeller and his party during 1888 (this enterprise being inspired by myself in a lengthened interview with the leader), tree vegetation exists to the summit, therefore up to 11,000 feet, as indeed already telescopically ascertained by M. Mikluho Maclay. I can, however, furnish no data, which might assist our present purpose, on the nature of the vegetation there, as—against my expectation—no botanic specimens whatever, resulting from that courageous exploit, came to me as one who since many years has been engaged occasionally on connected elucidations of the Papuan flora. Sir William MacGregor found the arboreous vegetation to cease on the Owen Stanley's Ranges at 11,500 feet (despatch, July 1889, p. 10), and this cessation was not due to a change of geologic formation. The limits of tree vegetation may, however, on some other Papuan culminations under altered physical conditions be somewhat higher so near to the equator, in comparison to zones of vegetation in the Himalayas at and near the verge of the tropics.

"As regards prospective utilitarian gain from the world of plants likely to emanate from this expedition, we may look forward to the acquisition of the 'cypress' (*Libocedrus Papuana*), which constitutes the principal forests on the summit of Mount Douglas and Winter's Height, for arboreta even of countries of the cool temperate zone, and with this cypress-like tree could doubtless be associated in parks far outside of the tropics also the tall 'bamboo' (see Sir William MacGregor's despatch, p. 8), with which the dry region above the nebular zone begins at (about 8500 feet). The several hardy and gaudy rhododendrons could aptly be consociated by dissemination with the many Sikkin species, now so frequent as garden favourites. The dwarf raspberry would give us an additional table-fruit. How far the Korthalsia palm would bear actual frigour, remains to be ascertained. The species of Papuan highland grasses are rather gregarious than numerous.

"Why so many plants from cold southern latitudes suddenly reappear on the Papuan and perhaps also on the Bornean highlands in evidently coæval forms of common origin; why the highest regions, and these almost only, should, like in New Zealand, reiterate plant-life, otherwise typical of Tasmania, of continental Australia, of islands in the Southern Ocean, and also of Fuegia and Patagonia; whether this indicates a continuity of portions of the Papuan Island with a once vastly extending southern land, now mostly submerged; what clues can be obtained for all this from the study of glacial drifts occurring during former enormous telluric changes, such as geologic science endeavours to explain; what part possibly could have been taken by any migratory birds in effecting so wide a dispersion of some of these plants even into so exceptional isolations; all this and other momentous considerations involved in these questions must be reserved for future discussions and generalizations in a special essay, perhaps under the advantage of access to ampler working material, and at not too distant a day."

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for July contains an article by Prof. H. A. Newton on the late Prof. E. Loomis, of Yale College, U.S. (see NATURE, vol. xl. p. 401). In early life he paid much attention to terrestrial magnetism, and published the first magnetic charts of the United States; but his most important contributions were to meteorology. In a discussion of the storms of 1842, he adopted the use of synchronous charts very much like those now generally employed. The later years of his life were spent in discussing the materials collected by the Signal Service, and he published twenty-three memoirs upon them, entitled "Contributions to Meteorology." A large portion of his estate was bequeathed to the endowment of an astronomical observatory.—Prof. H. A. Hazen has an article setting forth the observations most needed in the study of tornadoes. He points out that, after fifty years' observations, our knowledge of this subject is very unsatisfactory.—Lieut. Finley gives tornado statistics for the States of Florida and South Carolina. The observations for the latter extend over 128 years. The month of greatest frequency in Florida is September, and in South Carolina, March.—M. H. Faye continues his articles on trombes and tornadoes, dealing especially with their action upon forests, and the carrying of heavy *débris* to great distances.—Prof. W. A. Rogers continues his article concerning thermometers, dealing principally with the pulsatory movements of a mercurial column found to exist in nearly all the thermometers investigated.—The last article is devoted to American opinions on the relation of the influenza epidemic to meteorological conditions, being abstracts of papers read at the meeting of the American Medical Association in May last.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, August 6.—Captain H. J. Elwes, Vice-President, in the chair.—Prof. Meldola, F.R.S., exhibited a male specimen of *Polyommatus dorilis*, Hufn., a common European and Asiatic species, which had been taken at Lee, near Ilfracombe, in August 1887, by Mr. Latter. At the time of its capture Mr. Latter supposed the specimen to be a hybrid between *Polyommatus phlæas* and one of the "Blues," and had only recently identified it as belonging

to a well-known species. Mr. Stainton, F.R.S., Mr. Jenner-Weir, and Colonel Swinhoe made some remarks on the specimen, and commented on the additions to the list of butterflies captured in the United Kingdom which had been made of late years.—Mr. W. F. H. Blandford exhibited, and made remarks on, five specimens of *Athous rhombeus*, Ol., recently collected by himself in the New Forest.—The Rev. Dr. Walker exhibited a large collection of Coleoptera which he had recently made in Iceland. The following genera, amongst others, were represented, viz. *Patrobus*, *Nebria*, *Byrrhus*, *Aphodius*, *Philonthus*, *Barynotus*, *Chrysomela*, *Agabus*, *Creophilus*, and *Carabus*. Mr. Champion, Dr. Sharp, F.R.S., and the Chairman made some remarks on the collection.—Captain Elwes exhbited three species of the genus *Atossa*, Moore, three of the genus *Elcysma*, Butl., and three of the genus *Campylotes*, West.,—all from the Himalayas and North-Eastern Asia. The object of the exhibition was to illustrate the remarkable differences of venation in these closely-allied forms of the same family. Colonel Swinhoe, Mr. Warren, and Mr. Moore took part in the discussion which ensued.—Mr. P. Crowley read a paper entitled "Descriptions of Two New Species of Butterflies from the West Coast of Africa," and exhibited the specimens, which he proposed to name respectively *Charaxes gabonica* and *Cymothoe marginata*. He also exhibited several other new species from Sierra Leone, which had been recently described in the *Annals and Mag. of Nat. Hist.*

PARIS.

Academy of Sciences, August 4.—On the exhaustion of land by culture without manure; study of drainage waters, by M. P. P. Dehérain.—Observations of Coggia's comet (July 18, 1890) made with the Brunner equatorial of Toulouse Observatory, by M. E. Cosserat. Observations of position were made on July 21 and 22.—Elements and ephemeris of Denning's comet (July 23, 1890), by M. Charlois. The elements have been calculated from observations made at Nice on July 24, 28, and 30.—*Résumé* of solar observations made at the Royal Observatory of the College of Rome during the second quarter of 1890, by M. P. Tacchini. (See Our Astronomical Column.)—On the density of nitrogen and oxygen according to Regnault, and the composition of air according to Dumas and Boussingault, by M. A. Leduc. The author draws attention to a difference between the results obtained by Regnault and by Dumas and by Boussingault. If x = the proportion of oxygen in 100 volumes of air, d and d' the densities of oxygen and nitrogen, then

$$dx + d(100 - x) = 100, \text{ and } x = \frac{100(1 - d)}{d - d'}.$$

Replacing d and d' by Regnault's values ($d = 1\cdot10563$ and $d' = 0\cdot97137$), we get

$$x = 21\cdot324,$$

and for the percentage composition of air by weight,

$$\text{Oxygen} = 23\cdot58, \text{ and Nitrogen} = 76\cdot42.$$

Dumas's mean value was $23\cdot0 \pm 0\cdot1$, and the author throws out several suggestions as to the probable cause of the discordance. He has also made some determinations of the density of nitrogen, and obtained values comprised between 0·972 and 0·973.—Electrical resistances of gases in a magnetic field, by M. A. Witz. The author has previously communicated his researches on the action of magnetic fields on Geissler tubes (May 12, 1890), and has studied the effects produced by variations in the intensity of the magnetic field and the position of the tube with respect to the lines of force; he has now determined the influence exercised by changes in the pressure of the gas in the tube. The experiments have led to the conclusion that the action of magnets upon Geissler tubes is due to a variation in the capacity of the tubes, so that they constitute true condensers, and their illumination is the result of an oscillatory discharge of the same order as that of a Leyden jar, of which the period T is a function of the capacity C of the jar, and of the coefficient L of self-induction of the conductor of small resistance, and $T = \pi\sqrt{CL}$. A variation of the capacity C would thus modify the vibratory state of the gas and would be the cause of the differences observed in the luminous phenomena in intense magnetic fields.—Reactions of alkaloid salts, by M. Albert Colson. Some investigations on heats of formation are given.—On the division of sulphuretted hydrogen between the metals of two dissolved salts, by M. G. Chesneau.—On some derivatives from acetylacetone, by M. A. Combes.—Experimental researches on thermic sensibility, by M. Charles Henry.—Experimental researches on the affected nerves of chronic lead poisoning, and on the causes determining their appearance, by MM. Combemale and François.—On the combinations of hæmoglobin with carbonic acid, and with a mixture of carbonic acid and oxygen, by M. Christian Bohr.—On the colouring of the silkworm by feeding, by M. Louis Blanc. From the investigations it would appear that very soluble and diffusible substances, such as fuchsin, are absorbed by the epithelium intestinal of the silkworm, and colour the cells of the secretory organs, but not the product of secretion.—On the cellular division of *Spirogyra orthospira*, and on the rearrangement of the colouring matters driven to the ends of the spindle, by M. Degagny.—The treatment of black rot, by M. A. de l'Écluse.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

La Photographie Judiciaire: A. Bertillon (Paris, Gauthier-Villars).—British Cage Birds, Part 4: R. L. Wallace (U. Gill).—The Canary Book, Part 4: R. L. Wallace (U. Gill).—The Elements of Solid Geometry: R. B. Hayward (Macmillan).—Les Facultés Mentales des Animaux: Dr. F. de Courmelles (Paris, J. B. Baillière).—English-Eskimo and Eskimo-English Vocabularies: R. Wells and J. W. Kelly (Washington).—Photogravure: W. T. Wilkinson (Iliffe).—Bulletin from the Laboratories of Natural History of the State University of Iowa, Vol. i., Nos. 3 and 4 (Iowa).—Journal of Physiology, Vol. xi., Nos. 4 and 5 (Cambridge).

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THURSDAY, AUGUST 21, 1890.

FRESHWATER ALGÆ.

Introduction to Freshwater Algæ, with an Enumeration of all the British Species. By M. C. Cooke, M.A., LL.D., A.L.S. With 13 Plates. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1890.)

DR. COOKE is justified in saying, in his preface, that no apology is needed for the production of this volume, the latest addition to the "International Scientific Series." Notwithstanding the increased attention which has been paid of recent years in this country to this interesting and beautiful class of plants, we have had hitherto no popular hand-book devoted to their structure and their classification; the only existing works on British freshwater Algæ, by Hassall, and by Dr. Cooke himself, having been published at a price which places them out of the reach of the great majority of collectors of Algæ.

More than one-half of the present volume is occupied by a general account of freshwater Algæ, the main points connected with their structure and modes of multiplication, and useful instructions as to their collection and preservation. The greater part of this introductory portion is very good, and will serve admirably to interest and to instruct those who are turning their attention to the collection and determination of the plant-denizens of our streams and pools. In the chapters headed "Polymorphism," "Spontaneous Movements," and "Notable Phenomena," a large mass of interesting information is brought together, and the views of the leading authorities well and clearly presented. In the chapter on "Conjugation," although the present writer naturally dissents from Dr. Cooke's conclusion as to the nature of the process in the Zygnemaceæ, no objection can be taken (except on one minor point) to the way in which both sides of the controversy are presented.

The last two chapters of the Introduction, "The Dual Hypothesis," and "Classification," are much less satisfactory. By the "dual hypothesis" is meant the theory that Lichens are compound organisms made up of a fungal and an algal constituent. Dr. Cooke is, of course, perfectly at liberty to come to a different conclusion on this subject from that of nearly all biologists who have investigated it experimentally; but, at least, if the hypothesis is discussed, the arguments on both sides should be fairly stated. The synthetical construction of a Lichen out of its constituent elements has been affirmed by authorities so worthy of respect as Stahl and Bonnier—a fact which, if established, settles the question in a sense opposite to that accepted by Dr. Cooke; and yet these observations are not even alluded to, much less controverted, by him.[1]

The chapter on Classification is chiefly occupied by animadversions on a system different from that adopted by the author, which has been proposed by other writers on the same subject, "the most pretentious of philosophical systems," which he treats with a certain amount of unphilosophical scorn. Any attempt to classify according to their genetic affinities a class of plants about which so much still remains to be learnt as our freshwater Algæ, must necessarily be to a large extent tentative; but we do not think that practical algologists will be grateful to Dr. Cooke for perpetuating, as the basis of his classification, the obsolete system of Rabenhorst's "Flora Europæa Algarum aquæ dulcis et submarinæ," published in 1864, placing together, for example, *Palmella* and *Apiocystis* in one family, *Protococcus* and *Pediastrum* in another family, and retaining, as a primary group, the "Nematophyceæ." From the sentence on p. 187, the reader would suppose that the separation of the Protophyta as a distinct class was a fad of the "constructors of paper systems," for whom he expresses so great a contempt, instead of having the sanction of such authorities as Luerssen and Sachs long before these "paper systems" were published.

The latter and smaller part of the work is occupied by a description of the families and genera, and of all the known British species, of freshwater Algæ. Though not so stated on the title-page, the two largest families, the Desmids and Diatoms, are not included; but this was inevitable, to bring the work within moderate compass. The Characeæ, which also form a part of Hassall's "Freshwater Algæ," are likewise, and, as we think, rightly, excluded. The plates include a drawing of one species of each genus; they are copied from the plates in the author's "British Freshwater Algæ," and are not improved in the process. It is no fault of Dr. Cooke's that with Algæ, even more than with flowering plants, it is often almost impossible to distinguish the species from verbal descriptions only; those given here are mostly taken from the best writers, and could, on the whole, scarcely be improved. Small inexactnesses in spelling or in expression occur with irritating frequency—such as "immovable" for "motionless" in describing the spores of *Chantransia*, "Kutzing" (and Kutz.) throughout for "Kützing," "*Bulbochoete*" for "*Bulbochaete*," "*Glaeocystis*" for "*Gloeocystis*," "cytioderm" for "cytoderm," &c.; but of more serious inaccuracies we have noticed very few. We must, however, enter a word of protest against the Glossary, taken, apparently, almost *verbatim* from the author's "British Freshwater Algæ." A good glossary is an excellent thing; a bad glossary is useless, or worse. What are we to make of such definitions as the following: "ANTHERIDIA, certain reproductive organs supposed (*sic*) to be analogous to anthers, or fecundative"; "CARPOSPORE, spores produced by conjugation (*sic*) in a sporocarpium"? Under the Chroococcaceæ we find a constant reference to a mucous, gelatinous, or crustaceous "thallus"; turning to the glossary, we find a thallus to be "an expansion somewhat resembling a leaf"! "Trichogonia" are "the female reproductive organs in Batrachosperms"; under the Batrachospermeæ we find no reference to any sexual organs of reproduction.

The work, as it stands, will be in the hands of every collector and lover of Algæ. If, in preparing a second edition, Dr. Cooke will consent, in deference to the views of other algologists, to re-write the two chapters in his "Introduction" to which we have called attention,

[1] Martelli has quite recently recorded a complementary process in the dissociation of a lichen (*Lecanora subfusca*) into its algal and fungal elements.

and will bring his "Glossary" into harmony with the present condition of science, his "Freshwater Algæ" will have a still higher claim to a permanent place in botanical literature. ALFRED W. BENNETT.

APHASIA, OR LOSS OF SPEECH.

On Aphasia, or Loss of Speech, and the Localization of the Faculty of Articulate Language. By Frederic Bateman, M.D., F.R.C.P., Senior Physician to the Norfolk and Norwich Hospital, &c. Second Edition, greatly enlarged. (London: J. and A. Churchill, 1890.)

THE subject of aphasia has always been, and still is, not only of the greatest interest, but also of the greatest difficulty. Its interest is, of course, largely due to the fact that a study of partial or total loss of language may not only help in an analysis of language itself, but also may throw light on the exact anatomical situation of that function which has been said to set up an insurmountable barrier between man and the lower animals. Its difficulty is greatly increased by the fact that each investigator seems to define it in a different way. For instance, in the book whose title is given above, which is a second and greatly enlarged edition of Dr. Bateman's "Aphasia," first published twenty years ago, two entirely different definitions are accepted as correct. In the opening chapter aphasia is defined as "the term which has recently been given to the loss of faculty of language, and of the power of giving expression to thought, the organs of phonation and of articulation, as well as the intelligence, being unimpaired." On p. 154, however, Dr. Bateman states that he will "employ the term as a title for the whole group of disorders of speech, thus embracing not only the loss, but all the various degrees of impairment, of that faculty." This latter definition will, of course, denote an enormous number of affections, such as all the losses or alterations of speech due to gross cerebral lesions, to insanity, diseases of the medulla, cretinism, deaf-mutism, chorea, and so forth, many of which have hardly been touched upon in this work. The former definition also, in spite of its greater connotation, would include such diseases as deaf-mutism, which is hardly a form of true aphasia.

It is, perhaps, better to limit the term aphasia much more than either of the above definitions would allow. As Ross has so well shown in his small but highly philosophical work, "Aphasia," the mechanisms of speech must include (*a*) the "receptive organs"—that is, the eye, the ear, and the touch (as in reading from raised letters); (*b*) the apperceptive centres in the brain, where the various sensory phenomena are appreciated as language—that is, the angular gyrus and supra-marginal convolutions for written language, and the first temporo-sphenoidal convolution for spoken language; (*c*) the emissive motor centres in the brain at the posterior end of the third left portal convolution (Broca's convolution), from which discharges are sent through the internal capsule to the various organs of phonation; and (*d*) the executive organs, including the nerve nuclei in the medulla, the peripheral nerves from these, and the various muscles of the larynx, pharynx, and tongue. Now of these four sets of organs the first and last should not be included in considering true aphasia so that we should not include as aphasia such disorders of speech as arise from deafness, blindness, bulbar paralysis, or paralysis or other diseases of the larynx or tongue. The two remaining groups, (*b*) and (*c*), alone remain, and the organs of both are situated entirely in the brain, so that we can shortly define aphasia as a disorder of speech due to cerebral disease, the intellect being unaffected. It can, moreover, be seen to be roughly of two kinds—sensory when the apperceptive organs are affected, and motor when the emissive organs are affected. Now on these points Dr. Bateman has not been sufficiently explicit, with the result that some subjects have been included in his work which might well have been left out.

Aphasia may be divided, moreover, as regards causation, into organic where there is a distinct gross lesion, and functional where no known lesion is present. Of the functional causes, hysteria is by far the most common, and is generally the cause of those apparently anomalous cases where speech suddenly vanishes, only to return in as sudden a manner.

Nearly the first hundred pages of the book contain a most excellent account of the history of aphasia from the earliest times; but we regret to notice that while great stress is laid on cases reported more than fifty years ago, when nervous diseases were so little understood and when the examination of aphasic patients was most incomplete, yet the later and most thorough accounts of aphasia are noticed very slightly or not at all. Thus, although we find some slight references to Kussmaul's classical work, and also a slighter account of the works of Ross and Broadbent, Wernicke and Lichtheim are altogether neglected. And be it remembered that these names stand out pre-eminently as writers on this difficult subject.

All through this work there is an undercurrent of disbelief in any possibility of localizing the cerebral situation for speech, and after considering all the various views of localization, from that of Schroeder van der Kolk, who localized it in the corpora olivaria of the medulla oblongata, to that of Broca, with his localization in the posterior part of the third left frontal convolution, the author finally agrees with Kussmaul in saying that "a simple centre of language or seat of speech does not exist in the brain, any more than a seat of the soul exists in a single centre." Indeed, as the author's cases (to which I shall refer immediately) show, he seems to take extraordinary pains to satisfy his readers that Broca's convolution is not the seat of language, as in many cases of aphasia it is entirely unaffected. Nowadays, however, a neurologist does not attempt to fix on Broca's convolution alone as the seat of language, but says that either the supra-marginal or angular gyri, or the first temporo-sphenoidal convolution (all on the left side), or their connections with the motor region, are affected, as a rule, in sensory aphasia; or Broca's convolution, or its connections through the internal capsule with the medulla, are affected in motor aphasia. We know now sufficiently well that a lesion at the front of the anterior lobes, or in the motor area proper, would not be accompanied by aphasia, just as we know that a lesion in the other regions mentioned would almost certainly be accompanied by aphasia.

The author's own cases are ten in number, and are of much interest. Cases I. and IV., however, seem to us to have probably been general paralysis of the insane, and therefore not true aphasia, no typical lesion being found in the brain. There is no mention as to whether the pia mater was adherent to the cortex, which would have been probably the case in both instances. Case V. is given as one of disease of the spinal cord producing paraplegia with aphasia, but as no *post-mortem* examination was held, it is impossible to say that there was not a lesion on each side of the brain which would explain the symptoms more simply. In Case VIII., again, no *post-mortem* examination was made, and it appears to us to be a disease of the medulla or pons and not of the brain proper. Cases VI. and X. are evidently both hysterical aphasia, one occurring in a woman at the climacteric period, the other in a man. In the latter case, Dr. Bateman does not consider that it was hysterical, but the whole history of the man appears to us to prove conclusively that it was a typical case of hystero-epilepsy, which is much commoner, even in England, than is supposed. In Cases III. and VII. we have motor aphasia, but without lesion of the anterior lobes, but, inasmuch as the disease was found in the internal capsule, the fibres from Broca's convolution would, of course, be easily affected. Case VII. is again one of motor aphasia, but is rare, as it is accompanied by left hemiplegia instead of the usual right hemiplegia. This, however, is a coincidence which has occasionally been observed, and in which the speech centres seem to be localized on the right side of the brain instead of the left. There appears to have been no *post-mortem* examination in this case. It is much to be regretted that Dr. Bateman has not given better examples from his own experience of the various classes of aphasia, particularly of the sensory variety.

In mentioning the various kinds of aphasia Dr. Bateman describes fifteen different classes. On analyzing these, however, it can be seen that many of these are mere degrees of a larger class, and it would have been much better to have made fewer varieties. Perhaps the most useful division is as follows:—Motor aphasia, including agraphia and aphemia. Sensory aphasia, including word blindness, or inability to understand written language, and word deafness, or inability to understand spoken language. Finally, rather as a result of sensory aphasia, and, as it were, merely a symptom of it, verbal amnesia, in which a patient either constantly uses wrong words, as in paraphasia, or cannot remember the names of things, as in the aphasia of recollection.

Perhaps these cases of verbal aphasia are the most difficult of all to fathom, and they have been the cause of various neurologists assuming that in the brain there is a a definite centre for the understanding and remembrance of nouns, and another for the understanding of propositions. Ross, however, has pointed out that such an assumption is altogether unnecessary, and shows, by a careful analysis of the evolution of language, that in a lesion of any of the various auditory or visual centres such a dissolution of language would occur as would exactly cause inability of understanding propositions or nouns; and, moreover, that highly abstract nouns, such as *virtue*, would disappear first, and, if the injury were greater, then an inability to understand even concrete nouns would occur. In fact, the way in which Ross looks upon all forms of aphasia as mere paralyses, either sensory or motor, is to us the most satisfactory view yet mooted. These highly philosophical explanations have been entirely unnoticed by Dr. Bateman.

A most useful part of this work is a chapter on the medical jurisprudence of aphasia. This is a subject which we believe has not been touched upon in any previous English text-book, and it is of the greatest importance. Undoubtedly in former times many pure aphasics have been considered insane, and so incapacitated for various legal functions. Now, however, the distinction between insanity and aphasia is clear, and although certain cases of aphasia could not be made to understand legal documents, still other cases would have slight difficulty in this respect, and each case would have to be decided on its merits.

The treatment of aphasia is intensely interesting, for, although apparently it is a hopeless task to attempt to form, as it were, new speech centres in the brain, yet it is really wonderful how much may be done in this way by systematic and painstaking efforts.

To summarize briefly, we may say that Dr. Bateman's work is one that should be read by everyone interested in the faculty of language, or in diseases of the nervous system. It contains an enormous amount of valuable material, which has been put together by great labour, and is written by one who has devoted many long years to his subject.

ERNEST S. REYNOLDS.

CHEMICAL CRYSTALLOGRAPHY.

Einleitung in die chemische Krystallographie. Von Dr. A. Fock. (Leipzig: Verlag von Wilhelm Engelmann, 1888.)

IN contradistinction to works on systematic and physical crystallography, this little volume is devoted to crystallography in its far more fascinating relations to chemical constitution. It has been a most noticeable fact that while pure crystallography in its geometrical and physical aspects has been brought to a state of great perfection, our knowledge of the essentially intimate connection between crystallographic form and chemical constitution has until recently been almost at a standstill, and our information upon this branch of the subject is confined to a few isolated facts, many of which even are greatly in need of more complete investigation. As to whether chemists will ever be able to predict with tolerable certainty the crystalline form of a new substance of given composition, opinions among crystallographers are divided, and it may with reason be advanced that, in view of the meagre collection of facts before us, opinions cannot claim to have any real value at all. Since crystallography has commenced to be studied a little more from the side of the chemist, almost every number of the crystallographer's journal, the *Zeitschrift für Krystallographie*, edited by Prof. Groth, contains contributions to our knowledge of such relations. And whether it be ever possible or not to attain the great generalization, if chemists will only more generally tackle the study of crystallography, the subject will at least be raised from its present position of doubt and uncertainty. British chemists

in particular are somewhat behind in this respect, for the dearth of crystallographers in this country, the home of Miller, one of the greatest names in crystallography, is a subject of general remark among Continental workers in this domain of science. What is required is, first, that chemists shall make practical crystallography, the difficulty of becoming skilled in which has been greatly overestimated, one of the essential accessories of their main subject; and secondly, that special care be taken never to permit a series of well-crystallizing bodies, differing chemically from each other in an ascertained manner, to escape being thoroughly investigated crystallographically, with the object of discovering what geometrical differences accompany the constitutional ones.

To those who take up the subject from this standpoint, Dr. Fock's work will be of great assistance in placing before them in a succinct, concise, and very complete manner, the present state of our knowledge. The earlier chapters deal with the history of the growth of the views now entertained as to the nature of the architecture of crystals. Then follow a series of chapters upon the modes of formation of crystals, by resolidification of the fused substance, sublimation and separation from saturated solutions; upon the complicated influence of water of crystallization upon the geometrical form, and the various theories that have been put forward as to the condition of the water in crystals containing it. The nature of double salts, and the evidence of thermochemistry as to the mode of union of the simple salts in the double molecule, are very fully discussed, and form a most interesting chapter. Then follow a series of chapters upon the ultimate structure of crystals, as evidenced by the mode of formation of crystallites, and the order of growth in larger crystals.

By far, however, the most interesting portion of the book is that which deals with the relations between the crystalline form and chemical composition of crystals. The development of the theory of isomorphism is very clearly traced from the first observation of De l'Isle, in 1772, that the sulphates of copper and iron separated in mixed crystals from a solution of the two, to the latest definition of the theory given by Sohncke with reference to his 65 systems of points. The subject of mixed crystals, and the rules which govern their formation, are entered into at length, and their relations to true isomorphism clearly defined. A very suggestive term, that of "physical isomerism," is given to polymorphism, reminding one forcibly of the similarity between the various forms of the same compound or allotropic forms of elementary substances on the one hand, and the isomerism so characteristic of many of the compounds of carbon on the other. The last few chapters of the book are devoted to a *résumé* of all the more important researches upon isogonism or morphotropy—that is, partial or particular-zone isomorphism. The researches of Groth upon the crystallographical relations between the derivatives of benzene naturally take a prominent place in such a description, being, as they were, the first which were instituted in a systematic manner. And here, in spite of many additions which have recently been made in other branches of organic chemistry to our knowledge of such morphotropic relationships, the subject must perforce end for the present, until more facts have been accumulated and observations multiplied. It is not, however, the mere accumulation of records of measurements of isolated compounds which is so much needed, it is the systematic crystallographical investigation of series of compounds whose chemical relationships are indubitably established that will be calculated to throw most light upon the subject. To this end it is earnestly to be desired that British chemists will not merely content themselves, in describing well-crystallizing new compounds, with attaching to them the meaningless terms "prisms" or "tables," but will have their crystallographical characters thoroughly investigated, and their relationships to other compounds of the same or related series definitely made out.

A. E. TUTTON.

OUR BOOK SHELF.

British Rainfall, 1889. By G. J. Symons, F.R.S. (London: Edward Stanford, 1890.)

THIS work deals with the distribution of rain over the British Isles during the year 1889, as observed at nearly 3000 stations in Great Britain and Ireland. The author begins with his usual report for the year, in which he points out rather particularly, the list only dealing with the years 1884–88, that heavy falls of 3, 4 or 5 inches per diem may occur in all parts of the country. Then follows an interesting article on the amount of evaporation, including illustrations of evaporators and numerous tables.

Under the heading "Staff of Observers" the volume contains returns from 299 stations which sent no perfect record in 1888, the losses being 181, resulting in a net gain of 118. The author informs us that this is the largest increase since the year 1882; and we are glad to see that Scotland, which has been retrograding ever since 1883, has at last improved considerably.

Coming now to the rainfall and meteorology of the year 1889, we have, first, notes of some of the principal phenomena, amongst which we may mention the following:—January 6, at Nottingham, an extraordinary thickness (an inch at least) of rime on all the trees, &c.; June 6, at Cambridge Observatory, severest thunderstorm ever remembered.

The observers' notes for the months and for the year contain some interesting information:—In July, at Finchely, Etchingham Park, there were 22 days of absolute drought followed by 15 wet out of the 20 following days; in the same month the traffic for a distance of ten miles was suspended on the upper level of the Caledonian Canal owing to the scarcity of water.

Of the heavy rains in short periods recorded, the highest was that of 3·37 inches per hour, lasting for 12½ minutes, at Petersfield, Compton; but following this, in Warwickshire, 3·64 inches fell in 1 hour 5 minutes—a quantity unequalled at any station in the British Isles for at least ten years. Of the extremes of rainfall for the year, the largest fell at Stye, in Cumberland (152·85 inches), the least at Dingwall, East Ross (14·51 inches).

Among the absolute and partial droughts, of the former the longest was at Cargen in Kirkcudbright, where no rain fell between June 8 and July 9, or for 30 clear days; and of the latter the longest lasted for 45 days from June 3, at Portland, Co. Waterford. The definitions of these two kinds of drought are respectively:—Periods of more than 14 consecutive days absolutely without rain; periods of more than 28 consecutive days, the aggregate rainfall of which does not exceed 0·01 inch per diem.

With regard to the relation of the total rainfall in 1889 to the average, we find that it is 8 per cent. below the true average as well as 13 per cent. below that of 1870–79.

The general tables of the total rainfall for the year are given, with an explanation of their arrangement.

The result of this systematic and laborious task of gathering all these records and observations reflects great credit on the editor, who seems to have spared no pains to insure the accuracy of the information recorded.

Photogravure. By W. T. Wilkinson. (London: Iliffe and Son, 1890.)

THE aim of photographers has long been to produce prints, permanent and artistic in effect, with the delicacy and truthfulness of a photograph from nature. The process of photogravure seems to fulfil these requirements, and for purposes of book illustration should form a most important factor from the commercial point of view. The process is both simple and interesting, and requires little apparatus or material which is not already found even in most amateur's photographic dark rooms.

Mr. Wilkinson describes in this little book a method employed in obtaining a finished plate, the process being divided into six stages. The first is the production of a transparency upon a special (transparency) carbon tissue from the negative; in the second, from the transparency a negative in ordinary carbon tissue is made; the third consists in laying the etching ground upon a polished copper plate; in the fourth the carbon image (second stage) is mounted and developed upon the prepared copper plate; the fifth stage deals with the protection of the margin, and etching and burnishing; and the sixth and last stage gives us the print from the plate, done in much the same manner as copper-plate etchings and mezzotint engravings. The frontispiece, by W. L. Colls, affords a good illustration of a result of this process.

Elements of Euclid. Book I. By Horace Deighton, M.A. New Edition, Revised. (London: George Bell and Sons, 1890.)

THE present edition of Mr. Deighton's book is a great improvement on many of the works on this subject. In addition to the ordinary propositions, the solutions of a large number of important propositions are incorporated in the text with riders attached to them, which will be found useful, since in examinations nowadays more is required than is contained in Euclid.

Abbreviations and other symbols are used throughout, with the exception of the first fifteen propositions, and great clearness is obtained in the propositions and problems by making the construction lines thin, and also by printing the letters referring to the figures in a larger and more conspicuous type.

At the end of Book I. a series of examples is given on the propositions in it, and a short chapter on plane loci is added. This book will be especially instructive to beginners, the author having smoothed the path for those who wish to acquire facility in solving geometrical questions.

Camping Voyages on German Rivers. By Arthur A. Macdonell. (London: Edward Stanford, 1890.)

IN this book Mr. Macdonell gives an account of boating expeditions on German rivers. Some of the streams he describes have already been dealt with in English books, but he may fairly claim that no previous work of the kind is so nearly complete as his own. Every German river—with the exception of the Lahn—which an Englishman would care to see, he has navigated; and his experiences with regard to each are carefully recorded. We need scarcely say that for young and vigorous travellers there is no more delightful way of visiting a beautiful country. It not only provides them with healthy physical exercise, but takes them into the midst of enchanting scenery, and gives them opportunities of becoming intimately acquainted with interesting towns and villages. Mr. Macdonell has thoroughly appreciated the happiness which has thus come in his way; and in this book he contrives to communicate to his readers a good deal of the pleasure with which he recalls his adventures, and depicts what he has seen. The work is based on notes taken down each day in the course of the various voyages, and to some extent this no doubt accounts for the brightness and freshness of the narative. The value of the book is much increased by good maps, of which no fewer than twenty are given.

Epping Forest. By E. N. Buxton. Third Edition. (London: Edward Stanford, 1890.)

WE are glad to welcome a new edition of this excellent little Guide. Mr. Buxton says the idea of writing it occurred to him when he observed how small a percentage of the summer visitors to Epping Forest ever ventured far from the point at which they were set down by train or vehicle. No one to whom this Guide is known will be content to go to Epping Forest without trying to see as much of it as can be conveniently visited. Mr. Buxton has lived all his life in one or other of the Forest parishes, and knows exactly what parts of the subject are most worthy of being fully dealt with. He knows also how to express concisely and clearly all that he wishes to say. For visitors who are interested in natural history he has added some chapters on "the different forms of life which they may expect to find in the course of their rambles."

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The "Barking Sands" of the Hawaiian Islands.

ABOUT a year ago NATURE printed my letter from Cairo giving a condensed account of an examination of the Mountain of the Bell (*Jebel Nagous*) on the Gulf of Suez, and of the acoustic phenomenon from which it is named. In continuation of my researches on sonorous sand, which are conducted jointly with Dr. Alexis A. Julien, of New York, I have now visited the so-called "barking sands" on the island of Kauai. These are mentioned in the works of several travellers (Bates, Frink, Bird, Nordhoff, and others), and have a world-wide fame as a natural curiosity; but the printed accounts are rather meagre in details and show their authors to have been unacquainted with similar phenomena elsewhere.

On the south coast of Kauai, in the district of Mana, sand-dunes attaining a height of over one hundred feet extend for a mile or more nearly parallel to the sea, and cover hundreds of acres with the water-worn and wind-blown fragments of shells and coral. The dunes are terminated on the west by bold cliffs (*Pali*) whose base is washed by the sea; at the east end the range terminates in a dune more symmetrical in shape than the majority, having on the land side the appearance of a broadened truncated cone. The sands on the top and on the landward slope of this dune (being about 100 yards from the sea) possess remarkable acoustic properties, likened to the bark of a dog. The dune has a maximum height of 108 feet, but the slope of sonorous sand is only 60 feet above the level field on which it is encroaching. At its steepest part, the angle being quite uniformly 31°, the sand has a notable mobility when perfectly dry, and on disturbing its equilibrium, it rolls in wavelets down the incline, emitting at the same time a deep bass note of a tremulous character. My companion thought the sound resembled the hum of a buzz saw in a planing mill. A vibration is sometimes perceived in the hands or feet of the person moving the sand. The magnitude of the sound is dependent upon the quantity of sand moved, and probably to a certain extent upon the temperature. The drier the sand the greater the amount possessing mobility, and the louder the sound. At the time of my visit the sand was dry to the depth of four or five inches; its temperature three inches beneath the surface was 87° F., that of the air being 83° in the shade (4.30 p.m.).

When a large mass of sand was moved downward I heard the sound at a distance of 105 feet from the base, a light wind

blowing at right angles to the direction. On one occasion horses standing close to the base were disturbed by the rumbling sound. When the sand is clapped between the hands a slight hoot-like sound is heard; but a louder sound is produced by confining it in a bag, dividing the contents into two parts and bringing them together violently. This I had found to be the best way of testing sea-shore sand as to its sonorousness. The sand on the top of the dune is wind-furrowed, and generally coarser than that of the slope of 31°, but this also yielded a sound of unmistakable character when so tested. A bag full of sand will preserve its power for some time, especially if not too frequently manipulated. A creeping vine with a blue or purple blossom (*kolokolo*) thrives on these dunes, and interrupts the sounding slope. I found the main slope 120 feet long at its base; but the places not covered by this vine gave sounds at intervals 160 paces westward. At 94 paces further the sand was non-sonorous.

The native Hawaiians call this place *Nohili*, a word of no specific meaning, and attribute the sound caused by the sand to the spirits of the dead, *uhane*, who grumble at being disturbed; sand-dunes being commonly used for burial-places, especially in early times, as bleached skeletons and well-preserved skulls at several places abundantly show.

Sand of similar properties is reported to occur at *Haula*, about three miles east of Koloa, Kauai; this I did not visit, but, prompted by information communicated by the Hon. Vladimar Knudsen, of Waiawa, I crossed the channel to the little-visited island of Niihau. On the western coast of this islet, at a place called *Kaluakahua*, sonorous sand occurs on the land side of a dune about 100 feet high, and at several points for 600 to 800 feet along the coast. On the chief slope, 36 feet high, the sand has the same mobility, lies at the same angle, and gives when disturbed the same note as the sand of Kauai, but less strong, the slope being so much lower. This locality has been known to the residents of the island for many years, but has never been before announced in print. This range of dunes, driven before the high winds, is advancing southward, and has already covered the road formerly skirting the coast.

The observations made at these places are of especial interest, because they confirm views already advanced by Dr. Julien and myself with regard to the identity of the phenomena on sea-beaches and on hill-sides in arid regions (*Jebel Nagous*, *Rig-i-Rawan*, &c.). The sand of the Hawaiian Islands possesses the acoustic properties of both classes of places; it gives out the same note as that of Jebel Nagous when rolling down the slope, and it yields a peculiar hoot-like sound when struck together in a bag, like the sands of Eigg, of Manchester (Mass.), and other sea-beaches—a property that the sand of Jebel Nagous does not possess. These Hawaiian sands also show how completely independent of material is the acoustic quality, for they are wholly carbonate of lime, whereas sonorous sands of all other localities known to us (now over one hundred in number) are siliceous, being either pure silex or a mixture of the same with silicates, as feldspar.

The theory proposed by Dr. Julien and myself to explain the sonorousness has been editorially noticed in NATURE, but may properly be briefly stated in this connection. We believe the sonorousness in sands of sea-beaches and of deserts to be connected with thin pellicles or films of air, or of gases thence derived, deposited and condensed upon the surface of the sand grains during gradual evaporation after wetting by the seas, lakes, or rains. By virtue of these films the sand grains become separated by elastic cushions of condensed gases, capable of considerable vibration, and whose thickness we have approximately determined. The extent of the vibrations, and the volume and pitch of the sounds thereby produced after any quick disturbance of the sand, we also find to be largely dependent upon the forms, structures, and surfaces of the sand grains, and especially upon their purity, or freedom from fine silt or dust (Proceedings Am. Assoc. Adv. Sci., 38, 1889).

I should be lacking in courtesy if I closed this letter without expressing my great obligations to Mr. H. P. Faye, of Mana, and to Mr. Geo. S. Gay, of Niihau, for both a generous hospitality and a sympathetic assistance in carrying out my investigations. H. CARRINGTON BOLTON.

Honolulu, H.I., May 26.

Relative Growth of Boys and Girls.

A "NOTE" in NATURE of August 14 (p. 376) referring to some measurements made by Herren Geisler and Ulitzsch on school children at Freiberg (Saxon), speaks of the fact that between the ages of 11 and 16 years girls are taller than boys as if it had not been previously observed and recorded. The fact has been well known for many years, and was first observed by Dr. Bowditch, of Harvard. In a private letter to me dated March 13, 1876, Dr. Bowditch wrote:—"A comparison of the rates of growth of boys and girls shows that in this community girls about 13 years of age are taller and heavier than boys of the same age, and I wish to see how far this is the case in other countries. Quetelet's observations seem to show that it is not the case in Belgium, but some English observations quoted by him indicate that among certain classes of the population in England the same thing is found." Again, on June 10, 1876, he wrote:—"I am exceedingly obliged to you for your letter of April 27, and for the statistics which it contains. It is very interesting to get a confirmation of my observation on the difference between the two sexes about 13 years of age. I shall endeavour to verify your conjecture as to the cause of it. . . . You refer to Quetelet's measurements as being based on only ten observations for each age. Do you understand that the elaborate tables given in his 'Anthropometrie,' p. 417, rest entirely on this small number of observations?" In his paper on "The Growth of Children," published the following year (1877), Prof. Bowditch demonstrated the fact by both tables and diagrams; and in my "Manual of Anthropometry" (1878), and in the Reports of the Anthropometric Committee of the British Association (1880–83), I have given similar evidence of the difference of the sexes in this country. The English tables published by Quetelet mentioned by Dr. Bowditch refer to factory children, and were collected by Stanway, and published in a Report of the Factory Commission so long ago as 1833. Quetelet makes his curves of growth of boys and girls meet at the age of 12 years, but at all other ages the girls are shorter than the boys, but his ten observations (on selected individuals?) at each age were not sufficient to bring out the true difference. The only novelty in the German observations is that the boys do not "catch up" the girls quite so early as they do in England and America. This point, however, can only be decided by a comparison of the actual measurements of the German with those of the English and American children.

Curzon Street, Mayfair, August 16. CHARLES ROBERTS.

The Perseid Meteors.

As I merely expressed a wish that the Perseid shower should be closely watched on the present occasion, in order to ascertain whether the apparent shifting of the radiant was not really due to other causes, I do not think I need enter into any controversy with Mr. Denning on the subject. I will therefore only say that I think his "Catalogue of Radiants," recently published by the Royal Astronomical Society (the most valuable catalogue I think which has yet appeared), seems to me susceptible (as regards the Perseids) of a different interpretation from that which he places on it in his preliminary remarks.

Dublin, August 8. W. H. S. MONCK.

IN reply to Mr. Monck I need only say that I desired no controversy on this subject, but simply to uphold one of the most conclusive facts of my meteoric observations. I arranged my radiants with the utmost care, and on the basis of a practical acquaintance with the facts; and if Mr. Monck considers my results in regard to the Perseids will bear another interpretation, I must be content to wait for the corroboration which future observers will certainly give.

It is singular that so important a feature as the shifting radiant of the Perseids (which I first announced in NATURE, vol. xvi. p. 362) has not yet been adequately investigated. Mr. D. Booth, of Leeds, has, however, effected some observations in recent years (and especially in August 1887), and his results confirm my own. W. F. DENNING.

Bristol.

The Eclipse of Thales.

MR. PAGE, the author of a work entitled "New Light from Old Eclipses," which was noticed in NATURE last April, has forwarded the following communication on the subject of the eclipse of Thales. The views which Mr. Page entertains on the subject of these ancient eclipses are not those generally accepted; but he believes a crucial test of the superiority of his system is afforded by this particular eclipse;

and the author of the notice referred to is therefore invited to furnish a parallel calculation, based upon the theories which have hitherto received general support. This invitation the writer must decline, simply because two far abler hands than his have already investigated this problem on the lines which he would have pursued; and he could add nothing to the authority that accompanies the utterances of Dr. J. R. Hind, the Superintendent of the English "Nautical Almanac," and Prof. Simon Newcomb, the Superintendent of the American.

The following is Mr. Page's communication:—

"Herodotus speaks of the eclipse of Thales as follows:—'A war commenced between the Lydians and the Medes, . . . which continued five years; and it is remarkable that one of their engagements took place in the night. In the sixth year, when they were carrying on the war with nearly equal success, on the occasion of an engagement, it happened that in the heat of battle day was suddenly turned into night' (Herodotus, b. i., s. 74).

"This battle was fought on the morning of the (Julian) July 8, or 9 days after the solstice; consequently in the time of longest days and hottest weather. It would seem from the above account that it commenced in the night, and was not ended until the time of the eclipse, or 5.24 a.m.; when the armies ceased fighting on account of their fears.

"From Ptolemy's canon we learn that Cyaxares, King of Media, began to reign B.C. 634, and reigned 40 years, during 28 of which the Scythians ruled over Asia. In B.C. 606 the Scythian power was broken, and the Medes and Babylonians conquered Assyria. Soon afterwards (*i.e.* in B.C. 603) that war broke out between Lydia and Media which was terminated by mutual fears of this eclipse. As the King of Media reigned 40 years from B.C. 634, he must have died B.C. 594, which is the latest date that can be fixed for the eclipse; and as he was 28 years subject to the Scythians, he must have reigned 12 years after the defeat of the Scythians in B.C. 606; and as his war with the Lydians could not have taken place for several years after this, and as the eclipse was in the sixth year of the war, the date of the eclipse cannot possibly be placed earlier than B.C. 600: consequently we are compelled to look for it some time between B.C. 600 and B.C. 594."

Appended to this communication is a calculation by Mr. Page of the time of new moon in B.C. 597. This calculation is founded upon Ferguson's tables, to which some corrections have been applied by the computer. The calculation cannot be given here in detail; but the result to which Mr. Page is led is July 8, 5h. 24m. 11s., as that at which the so-called eclipse of Thales occurred. This date differs some twelve years from that which has been assigned by the two authorities just mentioned, viz. B.C. 585—a date, too, which accords with that mentioned by Pliny, reckoned by Olympiads. But those who find Mr. Page's arguments sufficient will agree with him; my regret is rather that he has chosen to build his theory on absolute tables, and to ignore all that the ablest astronomers and mathematicians have recently been able to accomplish in this direction. WILLIAM E. PLUMMER.

The Rotation of Mercury.

IN your issue for January 16 (xli. p. 257), Schiaparelli's observations on the planet Mercury are stated to lead that astronomer to the conclusion that "Mercury revolves around the sun in the same manner that the moon revolves round the earth, always presenting to it the same hemisphere."

Permit me to recall the fact that, as a matter of deductive reasoning, I recorded this opinion in 1883: "The powerful tidal action experienced by Mercury has greatly retarded its primitive axial motion, and increased its distance from the sun. No surprise would be occasioned by the proof that the planet has already attained to synchronistic motions" ("World-Life," p. 425). This opinion was accompanied by calculations of the solar tidal efficiency on Mercury.

ALEXANDER WINCHELL.

Ann Arbor, Michigan, U.S.A., August 4.

Wet and Dry Bulb Thermometers.

IT may, perhaps, interest you to know that on Friday last the difference between the wet and dry bulb thermometers, on board this ship in Grimsby roads, amounted to $12\frac{1}{2}°$; the dry bulb showing 66°, and the wet bulb 53°·5. Wind west; force, 7 to 8 by Beaufort's scale. This is the greatest difference I have recorded in this country for ten years. T. H. TIZARD.

H.M.S. *Triton*, Grimsby, August 17.

Experiment in Subjective Colours.

THE following experiment does not seem to be widely known: it is not easy to make a clear explanation of the lenses.

Take a number of the *Graphic* and a piece of thin paper, which, if put upon the ordinary print, allows it to be seen through, as black. Now put the paper over some of the large black letters on the apple-green outer cover: seen through the paper, they appear as bright red. W. B. CROFT.

Winchester College, August 18.

THE SCIENCE AND ART MUSEUM, DUBLIN, AND THE NATIONAL LIBRARY OF IRELAND.

IN the year 1877 the Natural History Museum and the Library of the Royal Dublin Society, which, though mainly supported for many years by Parliamentary grants, had been directly managed by the Society, were, by Act of Parliament, transferred to the Science and Art Department, a large sum of money having been at the same time paid by Government to the Society for ceding its rights and property.

Soon afterwards steps were taken by the Science and Art Department for providing suitable accommodation for an art and industrial addition to the Museum. Into a consideration of the various causes which delayed the carrying out of this project we need not enter here; they will be found described in the Reports of the Science and Art Department.

At length, in 1884, a final competition between rival architects' designs for the new buildings was arrived at, and those by Messrs. Deane and Son, of Dublin, were chosen by the representative committee, which was specially appointed for the purpose of selection.

The sites for these buildings, which were adopted after much discussion, are at right angles to Leinster House on its Kildare Street or western side. The *façades* of both buildings, which face one another, are about 200 feet long, and are similar, consisting of two rotundas with colonnades, and pavilions at the sides. In the centre of the Museum building is a large court about 125 feet by 75 feet. Opening from it there are in all 24 galleries or rooms, which are devoted to exhibiting purposes.

The foundations were laid by His Royal Highness the Prince of Wales on April 10, 1885, the ceremony connected therewith being the most important presided over by His Royal Highness during his last visit to Ireland.

The tender for the erection of the buildings by Messrs. Beckett Brothers, of Dublin, was accepted on November 3, and by the 17th operations had commenced. In four years, or by November 1889, the Museum building was completed, and was handed over to the Science and Art Department, and the transfer to the new galleries of the collections which had accumulated in the temporary premises during twelve years was at once proceeded with.

It was not until June of the present year that the sister building, for the reception of the National Library of Ireland, was completed. During the month of July the transfer of the books, consisting of about 100,000 volumes, from the old Library in Leinster House, has been satisfactorily accomplished.

Both institutions are about to be opened on the 29th of the present month by His Excellency the Lord-Lieutenant, after which they will continue to remain open and free to the public.

It may be of interest to add some details as to the principal contents and system of arrangement in the two institutions respectively. In order to describe the Museum

effectively, it is necessary to include here an account of the collections in the Natural History Department, which, however, remain in their old quarters—a very suitable building on Leinster Lawn. It has for a pendant a similar building, the National Gallery of Painting, which is, however, under different management. The relative positions of the five buildings may be compared to a capital H, in which the cross bar represents Leinster House, and the other portions of the letter the four buildings which have been referred to, with Leinster Lawn and the courtyard of Leinster House between them. Close by there is another group of buildings, which contains the class-rooms of the Metropolitan School of Art.

Leinster House, which is comparable in a sense to Burlington House, affords accommodation to the officers of the Science and Art Department in administrative charge of the various institutions, and also to the Royal Dublin Society, which, since it has been relieved of its mangement of the several institutions, has considerably developed its various functions in science and agriculture; for the due carrying out of the latter it has provided itself with extensive show yards at Ball's Bridge in the suburbs of Dublin, where the cattle shows, &c., have acquired an importance and success never attained while they continued to be held in the City premises.

The Society is possessed, moreover, of a large private library, and its members enjoy various privileges, such as the use of a general reading-room, free admission to meetings, lectures, &c. The Society has recently entered upon several new lines for the development and encouragement of the arts and industries of Ireland.

Returning to the Museum—the building which contains the natural history collection consists of two large rooms or halls, one on the basement and the other on the first floor, the latter having two galleries. The basement room contains the systematic collections of fish, reptiles, a number of large recent and fossil skeletons, and groups illustrative of the geographical distribution of animals.

In the lobby of the first-floor room there is a special collection of the mammals of Great Britain and Ireland, and in the room itself the main systematic collection of mammals is arranged in a row of large central cases, and the invertebrate collections in table and wall cases at the sides. As in some of the other groups, there are special collections of Irish invertebrates.

The first gallery contains a general collection of stuffed birds, and a special collection of Irish birds. In the second gallery there are general collections of insects, and of birds' nests and eggs. A room off this gallery contains a large collection of birds' skins arranged in glazed drawers, for study. Besides the above, there are considerable collections of invertebrates—especially of insects—in the curators' rooms.

A large annex, which was formerly occupied temporarily by a portion of the art collections, has been made use of for the display of the palæontological collections, which are of some extent and importance. The specimens of plant and animal fossils are for the most part arranged systematically; they include large numbers of fossils from the Sivalik Hills in India, and many well-known casts of generalized types of animals. The special collections, not incorporated, include an extensive one of Irish mammals, Sir Richard Griffith's collection of Irish Carboniferous and Silurian fossils, and several collections of Arctic fossils made by Sir F. Leopold McClintock and others.

From this annex a passage affords access to the new building which is about to be opened. In the first two rooms the collections of fossils, rocks, and minerals, which have been made by the Geological Survey of Ireland, are exhibited. In an adjoining room there is the general Museum collection of minerals, with a small one of meteorites. The next room is devoted to a large relief map of Ireland, coloured geologically. It is on the horizontal scale of one inch to a mile, the vertical scale, as is usual, being considerably exaggerated. In this room there are also a number of photographs representing natural phenomena, including some large transparencies which were presented by the United States Government. The corresponding rooms on the other side of the building are devoted respectively to (1) Greece and Rome; (2) Egypt and Assyria; (3) Ethnography, a very extensive and important collection; (4) Musical instruments; (5) India and Persia.

The Central Court, from which these rooms open off, is devoted mainly to casts of antique and mediæval sculptures, and to a large number of models of statues and busts by the late J. H. Foley. Close by is the rotunda, a hall which has been compared to Napoleon's tomb. It contains casts of antique sculptures, and in the centre a group of three bronze guns, with their carriages, &c., which were taken at Sobraon and Maharajpur, and presented to Lord Gough by the Honourable East India Company.

Ascending to the first floor, we meet in succession rooms devoted to (1) textiles (lace and embroidery); (2) wood carving; (3) glass and ceramics; (4) furniture; (5) casts of ivories and metal-work; and on the opposite side (1) woven materials, models of looms, &c.; (2) industrial models; (3) and (4) rooms intended for the famous collections of Irish antiquities made by the Royal Irish Academy; and (5) arms and armour. The gallery of the Central Court contains a number of casts of Celtic antiquities, a large collection of metal-work, and electrotypes, besides many other objects of considerable interest. In the gallery of the rotunda there are a number of casts of modern sculptures, &c.

On the south side of the building there is a second floor containing four rooms; these have been allotted to the Herbarium and Botanical Museum, the collections included in which are considerable, having been brought together from many different quarters. They have not yet, however, been arranged for public inspection. The various collections of Cryptogams are, perhaps, the most valuable. There are also several well-known collections of Irish plants.

It will be seen from the above sketch that this Museum covers a very wide field. This will be still further apparent from a study of the general "Guide" which is about to be issued.

There are some special features in the arrangement of the specimens which may be touched upon briefly. The objects in the Museum are largely provided with fully descriptive labels and maps. In the mounting of the specimens many novel devices have been made use of, and some ingenious contrivances have been founded upon inventions which, though used in American museums, have not hitherto been adopted in Europe.

The Museum is open free to the public daily, Sunday and two week-day evenings being included. The daily average attendance is about 600, and there is every reason for believing that the institution, which is now about to be fairly launched on its more extended career, will become increasingly popular and increasingly instructive to the people of Ireland.

The National Library is being arranged in its new quarters, upon principles which have been for many years the subject of earnest consideration and study by the librarian, Mr. William Archer, F.R.S.

The principal public reading-room is a very handsome apartment, capable of accommodating 200 readers. With the exception of a few works of reference, such as dictionaries, &c., the books are all arranged in stores, which are close at hand, in one of the wings of the building. These stores are in five stories, which are connected by ordinary (not spiral) stairs of low gradient, and the books are arranged on free standing presses, within easy reach of hand and eye; thus no ladders are

required, and no wall presses are used. Although the ultimate potential storage capacity of the building when complete may be extended to 600,000 volumes, at present only about 100,000 have to be provided for.

The arrangement of the books is according to a modification of the decimal system of Mr. Dewey, of the State Library of New York. It is claimed for this system that it brings together on the shelves all works of cognate character, be they general or specific.

Within the space available here it is not possible to fully illustrate this system, but a few lines may be devoted to explaining the general principles of the method. The whole Library must be regarded as being divided into nine libraries, numbered as follows: (1) Philosophy, (2) Religion, &c., (5) Natural Science, and up to (9) History. Each of these is again divisible, *if necessary*, into nine parts.

Thus the number 54 represents the 4th division (Chemistry) of the 5th class (Natural Science); 541 represents Theoretical Chemistry, and 5412 represents the 2nd division (Atomic Theory) of Theoretical Chemistry.

Every book as it is received in the Library will receive a number, which will at the same time indicate its place on the shelves, and be a summary of its contents.

When he receives the *title* from a reader, the attendant will, after a little practice, be able to go directly and without reference to the place marks, to the exact shelf or quarter of the stores by simply translating the title into its corresponding number.

One advantage of the system is that the special works contained in the Library on a given subject can always be seen together at a glance. It is needless to point out that the complex character of many books will furnish complex exceptions to the more simple nomenclature.

The Library, as an all-round, modern, working student's library in science and literature, is a very valuable one, though it is not at present so in the sense that it contains any particular literary treasures.

The administration of these several institutions, together with the Botanic Gardens at Glasnevin, and the Metropolitan School of Art, is carried on by the Science and Art Department, which is represented locally by the Director of the Science and Art Museum, under whom there are heads of the several departments and institutions. Two local bodies were created in 1877 to aid the Department in the supervision of these institutions, one the Board of Visitors of the Museum and Botanic Gardens, and the other the Council of Trustees of the National Library, the functions of the latter including the selection of books for the Library.

The total cost of the several institutions is provided in the annual estimates of the Science and Art Department which are voted by Parliament.

COMPARISON OF THE SPECTRA OF NEBULÆ AND STARS OF GROUPS I. AND II. WITH THOSE OF COMETS AND AURORÆ.[1]

II.

General Comparisons.

IN the preceding article I showed that the spectra of nebulæ, auroræ, bright-line stars, and stars of Group II. are closely related to the spectra of comets. In the table which follows, all the spectra are brought together and compared. It is not sufficient to show that each group resembles comets in some respects, as each one might have some feature which was absent in the other. I therefore give the following table to show how far they resemble each other. In the last column the dark bands which are simply due to absence of radiation, and are not really absorption-bands, are omitted.

Nebulæ.	Aurora.	Comets.	Bright-line Stars.	Stars with Mixed Flutings.
4101	411	—	4101	—
—	426	[426]	—	—
—	431	431	—	449 (bright space)
434	435	—	434	—
447	—	—	—	—
—	—	—	—	461–451 bright
468–474	474–478	468–474	468–474	472–476 bright
479	482	483	—	—
486	486	486	486	—
4958	—	—	—	4958–486 bright
500	500	500	—	502–4959 dark
509	—	—	507	—
517	517	517	517	516–502 bright
—	519	519	—	—
520	522	521	—	522–516 dark
—	—	—	—	524–527 dark
527	—	[527]	527	—
—	531	—	—	—
—	535	—	—	—
—	539	—	540	—
546	545	546	—	544–551 dark
554	—	—	—	—
559	558	558	558	559–564 dark
—	—	561	—	—
—	—	564	564	—
—	—	568	568	—
—	—	[579]	579	—
—	—	—	—	585–594 dark
5872 (D_3)	—	—	5872	—
—	—	[589]	589	—
—	606	—	—	—
—	620	[615]	—	616–630 dark
—	630	—	635	—

It will be seen that there are three flutings which run through the five columns, namely, 468–474, 517, and 558—these are due to carbon and manganese, and are the familiar cometary bands; four more—hydrogen 486, magnesium 500, magnesium 521, and lead 546—occur in four out of the five columns. Out of the thirty-four lines or flutings given, there are nineteen which occur in less than three columns, but this number is greatly reduced when slight differences of temperature, masking effects, and the exceptional conditions of comets are taken into account.

It is now universally agreed that comets are swarms of meteorites, and the tables which I have given show that nebulæ, bright-line stars, stars with mixed flutings, and the aurora, have spectra closely resembling those of comets, the special features of which are the carbon bands, to which I have recently added the absorption bands of manganese and lead; all are therefore probably meteoritic phenomena.

The following is a list of the bodies which contain either one or both of the carbon flutings near 517 and 468–474, the latter being a group of flutings, which, as I have before shown (Roy. Soc. Proc., vol. 35, p. 167), sometimes has its point of maximum brightness shifted from 474 to 468. The fluting near 564 has been omitted from the table, as it is generally masked, either by continuous spectrum or by the superposition of the fluting of manganese near 558. The wave-lengths given are as measured by the various observers stated.

The spectrum of the aurora is added for the sake of completeness.

It will be seen from the table that the record of the presence of carbon is unbroken from a planetary nebula through stars with bright lines to those resembling α Herculis, *i.e.* entirely through Groups I. and II. of my classification.

[1] Continued from p. 345.

Name.	Fluting 468–474.	Fluting 517.	Reference.
Planetary nebula	469·4 (Copeland)	—	Copernicus, vol. 1, p. 2.
Nebula in Orion	470 (Taylor)	—	Monthly Notices, vol. 49, p. 126.
Nebula, Gen. Cat., No. 4373	—	518 (Vogel)	Bothk. Beob., Leipzig, Heft 1, 1872, p. 57.
,, ,, ,, ,, 4234	—	518 (Vogel)	,, ,, ,, ,,
,, ,, ,, ,, 4390	—	518 (Vogel)	,, ,, ,, p. 58.
Nebula in Andromeda	468–474 (Fowler)	517 (Fowler)	Roy. Soc. Proc., vol. 45, p. 216.
,, ,,	—	517 (Taylor)	Monthly Notices, vol. 49, 126.
γ Argûs	468 (Ellery)	—	Observatory, vol. 2, p. 418.
,,	464·6 (Copeland)	—	Copernicus, vol. 3, p. 205.
Arg.-Oeltzen, 17681	461–470 (Vogel)	—	Astro-Phys. Obs. zu Potsdam, vol. 4, No. 14, p. 16.
,, ,,	473 (Pickering)	—	Astr. Nachr., No. 2376.
Lalande, 13412	469 (Vogel)	—	Astro-Phys. Obs. zu Potsdam, vol. 4, No. 14, p. 17.
1st Cygnus	470 (Wolf and Rayet)	—	Comptes rendus, vol. 65, p. 292.
,,	465–470 (Vogel)	—	Astro-Phys. Obs. zu Potsdam, vol. 4, No. 14, p. 17.
,,	468–474 (Fowler)	517 (Fowler)	New observations.
2nd Cygnus	470 (Wolf and Rayet)	—	Comptes rendus, vol. 65 (1867), p. 292.
,,	464 (Vogel) middle of band	—	Astro-Phys. Obs. zu Potsdam, vol. 4, No. 14, p. 17.
,,	468–474 (Fowler)	517 (Fowler)	New observations.
3rd Cygnus	470 (Wolf and Rayet)	—	Comptes rendus, vol. 65 (1867), p. 292.
,,	461–468 (Vogel)	517 (Vogel)	Astro-Phys. Obs. zu Potsdam, vol. 4, No. 14, p. 17.
,,	468–474 (Fowler)	517 (Fowler)	New observations.
γ Cassiopeiæ	—	517 (Sherman)	Astr. Nachr., No. 2691.
,,	468–474 (Fowler)	517 (Fowler)	New observations.
o Ceti	468–474 (Fowler)	517 (Lockyer and Fowler)	New observations.
α Herculis	468–474 (Fowler)	517 (Lockyer and Fowler)	New observations.
α Orionis	—	517 (Lockyer and Fowler)	New observations.
Aurora	474–478 (Vogel)	—	Bothk. Beob., Leipzig, Heft 1, 1872, p. 43.
,,	—	517 (Backhouse)	Nature, vol. 7, p. 463.

We have now to inquire into the previous work on this subject.

Carbon in Stellar Spectra.

Secchi, in 1869, was the first to call attention to the possible existence of indications of carbon in stellar spectra in connection with stars of his types III. and IV.[1] He even compared the spectrum of 152 Schjellerup with the carbon spectrum obtained from benzene. His micrometric measures of the distances of the principal bands in the two spectra from the sodium line D gave great weight to his statement.[2]

But although Secchi observed the coincidence of the edges of two dark bands in his types III. and IV., and remarked that the light-curve in one case faded towards the red, and in the other towards the violet end of the spectrum, he did not recognize that we were dealing with radiation in one case and absorption in the other.

Indeed, Secchi regarded type IV. as presenting chiefly radiation phenomena, for later,[3] when writing with respect to stars of this type he states:—

"Quelques-unes des raies noires et les plus importantes, coincident à très-peu-près avec celles du troisième type; cependant le spectre, dans son ensemble, *se présente comme un spectre direct appartenant à un corps gazeux*, plutôt que comme un spectre d'absorption. Si on le considère comme un spectre d'absorption, on trouve qu'il présente le caractère des composés du charbon, tels qu'on les obtient en produisant une série d'étincelles électriques dans un mélange de vapeur de benzine et d'air atmosphérique et dans l'arc voltaique entres les charbons."

From the foregoing, it is evident that Secchi had observed the coincidence of the flutings of carbon with the dark flutings in stars in his fourth type, but missed the significance of it altogether.

[1] *Atti dell' Acad. de' Nuovi Lincei*, xxv., 1872.

[2] These and other comparisons led Secchi to note:—"La conclusione è che nelle stelle di 4° tipo vi é certo il carbonio in una combinazione di debole tensione coll' idrogeno, é che questa combinazione esiste nello stesso stato, o in altro prossimo anche in quello di 3° tipo."

[3] "Le Soleil," vol. ii. p. 458.

Dr. Huggins, however, in a footnote to the first edition of Schellen's "Spectrum Analysis," edited by him, gave an observation of his of the spectrum of 152 Schj., and a diagram of the spectrum of this star, which combated Secchi's work. In his words:—

"He compared the spectrum of the star, using a narrow slit, with the bright lines of sodium and carbon. The line marked D he found to be coincident with that of sodium. The less refrangible boundary of the first of the three principal bright bands in the spectrum of carbon is nearly coincident with the beginning of the first group of dark lines; the second of the carbon bands is less refrangible than the second group in the star; the third band of the carbon spectrum falls on the bright space between the second and third group of dark lines in the spectrum of the star. The absorption bands are therefore not due to carbon."

Vogel, in 1884, showed that Dr. Huggins's observations were inaccurate; that the bands really did coincide with the carbon bands; and that Secchi's statement was perfectly correct with regard to this star (152 Schjellerup).[1]

[1] "Neben dem Spectrum des Natriums erschienen noch ganz schwach zwei Banden des Alkohol-spectrums, die vollkommen mit den dunklen Banden des Sternspectrums zu coincidiren schienen. Der Anfang der ersten Bande des Alkoholspectrums wurde zu + 14′ 37″ gemessen. Auf den Anfang der zweiten Bande wurde wiederholt der Faden gestellt, und coincidirte jedesmal der Faden so vollkommen als möglich mit der Bande im Sternspectrum. Auch directe Vergleichungen zwischen Alkoholspectrum und Sternspectrum konnten gemacht werden, da das Sternspectrum hell genug war und sich ganz gut von den das ganze Gesichtsfeld durchsetzenden mattleuchtenden Banden des Alkoholspectrums abhob." Following some measures made on June 1, it is noted:—"Bei den Vergleichungen mit dem Natrium- und Alkoholspectrum wurde wiederholt die Ueberzeugung gewonnen, dass eine Coincidenz mit den Natrium-Linien, sowie mit den beiden stärksten Banden des Kohlenwasserstoffspectrums im Spectrum der Flammen und des Sternes stattfand. Ich setzte an diesem Abend, da der Himmel besonders gunstig war, noch das stark zerstreuende Rutherfurd'sche Prisma ein und konnte damit wenigstens die beiden stärksten Banden im Sternspectrum messen und wiederum durch directe Vergleichung die absolute Coincidenz der hellsten Bande des Kohlenwasserstoffspectrums mit einer Bande des Sternspectrums beobachten." In summing up the observations of the spectrum of this star Prof. Vogel remarked, "Vergleicht man diese Beobachtungen mit den Seite 14 angeführten des Kohlenwasserstoffes, so ergiebt sich zweifellos das Vorhandensein von Kohlenwasserstoff in der Atmosphäre des Sternes."—"Astrophysikalischen Observatoriums zu Potsdam," No. 14, p. 23, 1884.

Similar comparisons of the carbon spectrum with the spectra of other stars of the same type were made, and the coincidences led Vogel to the following final conclusion :—

"Die charakteristischen Banden dieser Sternspectra scheinen durch die Absorption von Kohlenwasserstoffen, die in der Atmosphäre der betreffenden Sterne vorhanden sind, hervorgebracht zu werden."

Quite recently, Mr. Maunder, in commenting upon the Rev. T. E. Espin's admirable revision of Birmingham's "Red Star Catalogue," wrote:[1] "In the note on No. 364 [152 Schjellerup], it should surely have been made clear that the difference between Secchi's and Huggins's account of its spectrum was due to the one having compared it with the spectrum of a hydrocarbon, and the other with that of carbonic oxide, and that the perfect accuracy of Huggins's description has been abundantly confirmed, though, for the reason just given, he missed the recognition of the absorption bands of the stellar spectrum as those of carbon."

Mr. Maunder here refers—I presume with authority—to a statement made by Dr. Huggins which I have not been able to trace. In the note already quoted, Dr. Huggins refers to the spectrum of carbon without giving any idea of the actual compound used for making the comparison, and I have not been able to find any subsequent statement which justifies Mr. Maunder's remarks. Further, it is not sufficient to simply state the compound used, as the spectrum obtained depends upon the conditions of experiment. It does not follow, therefore, that, even if carbonic oxide were employed, the spectrum obtained was not the so-called "hydrocarbon" spectrum. I fancy that now most workers are agreed that the band at 517 is a true carbon band, and obtainable, therefore, from any carbon compound.

Dunér, in 1884, discussed the evidence as to carbon absorption in stars of type IV.[2] The mean wave-lengths given by him for the bands in this group are compared with those found by Vogel in the following table :—

Number of Dunér's band.	Wave-length.	Vogel's measures.	
			μμ
		Spectrum begins	660
	μμ	Band	656
2	621 ...	Band	622
3	604·8 ...	Band	606·5
4	589·8 ...	Line in a band	589·3
		End of the band	584·8
5	576·0 ...	Line	575·7
6 (beginning)..	563·3 ...	Line, beginning of a band	563·1
7	551 ...	Line	552
6 (end)	545 ...	Line	544
8	528·3 ...	Group of lines	528
9 (beginning)..	516·3 ...	Line, beginning of a band	515·9
		Line	513·2
9 (end)	496		
10 (beginning)..	472·7 ...	Beginning of a band ...	472·9
10 (end)	463		
End of spectrum	437 ...	Band	437
		Spectrum ends	430

Dunér compared Vogel's measures and his own with the following wave-lengths of the hydrocarbon bands said to be given by Hasselberg :—[3]

		μμ.
Beginning of band	1	618·7
End ,, ,,		594
Beginning of band	2	563·4
End ,, ,,		543
Beginning of band	3	516·4
End ,, ,,		507
Beginning of band	4	473·7
End ,, ,,		467
Maximum ...	5	436·7
Beginning of band	6	431·9
End ,, ,,		423

[1] *Observatory*, No. 164, July 1890.
[2] "Sur les Étoiles à Spectres de la Troisième Classe," Stockholm, 1884.
[3] "Ueber die Spectra der Cometen," p. 21.

These values differ slightly from those measured by Hasselberg in 1880, and given in the work referred to by Dunér.

From a comparison of the two sets of wave-lengths, those found in the spectrum of a body of type IV. and those given by Hasselberg, Dunér concluded that :—

"Les longueurs d'onde des bandes 6, 9 et 10 dans les spectres III.*b* sont donc à considérer comme identiques à celles des bandes 2, 3, et 4 dans le spectre de l'hydrogène carboné. Mais aussi la longueur d'onde 437 de la bande au violet, où pour mon réfracteur était la fin du spectre, et la longueur d'onde 430 de la fin du spectre visible selon M. Vogel, sont d'accord avec les deux bandes violettes de l'hydrogène carboné. On peut donc régarder comme extrèmement probable que :

"*Les bandes principales dans les spectres III.b sont dues à l'absorption exercée par un composé du carbon qui se trouve dans les atmosphères de ces étoiles.*"[1]

It will be seen from the passage which I have given in a note that most of the discussion had turned on the coincidence between bright carbon bands seen in the laboratory and dark absorption bands seen in stellar spectra (type IV.). It is not a little curious to see Dunér, in the passage I have underlined, holding to a possible similarity between stellar and cometary structure based upon carbon radiating in one case and absorbing in the other.

The next important advance was made by Dr. Copeland, who, in January 1886, in a communication to the Royal Astronomical Society on the spectrum of a new star in Orion, wrote as follows :—[2]

"The spectrum is unmistakably of the third type, of which α Orionis is the brightest member. But in this star the *bright bands* are so strikingly developed that they form the most salient parts of the spectrum. Adopting this view an examination of the preceding numbers and the descriptions of the bands, &c., to which they refer, reveals the startling fact that this spectrum is not so much a continuous one, interrupted by dark lines and dusky bands, as a *not very luminous spectrum upon which a series of bright bands are superposed.* One of the bright bands, that beginning with the 'very bright line,' W.L. 516·2 m.m.m. is most readily identifiable as

[1] Dunér, in his conclusions as to the spectra of stars of Class III., wrote :—

"Si l'on passe ensuite à considérer le développement ultérieur de l'étoile, il est évident qu'à mesure qu'elle se refroidit davantage, elle parvient enfin à une température ou le carbone qui doit se trouver en abondance, soit dans son atmosphère soit sous une forme quelconque dans son photosphère, peut se combiner avec l'élément l'hydrogène ou un autre, qui ensemble avec le carbone donne origine au 'Spectre de Swan.' A partir de cela, le spectre se montre coupé par une large et faible bande à la longueur d'onde 516 mm. et par une autre encore plus pâle à 473 mm., et les parties du spectre au-delà de celle-ci sont très faibles. Mais peu à peu ces deux bandes gagnent en intensité, et en même temps la bande à 563 mm. se fait valoir, d'abord à peine visible, puis de plus en plus forte. A cette époque se développe la bande étroite à 576 mm., et finalement les trois bandes principales sont presque égales entre elles en intensité, et on reconnait, dans le spectre, tous les détails caractéristiques. Ce serait s'engager dans une discussion inutile si l'on voulait seulement exprimer une supposition sur le moment ou les bandes secondaires dans le rouge et dans l'orange font leur apparition, aucun fait n'étant connu qui pût être cité à l'appui.

"Ce qui est sans doute très remarquable c'est que dans les spectres III.*b* on n'aperçoit trace de la bande carbonique à la longueur d'onde 618·7 mm. laquelle est si brillante dans les tubes de Plücker contenant de l'hydrogène carboné. Ceci est au reste en parfaite analogie avec ce qu'on voit dans les spectres des comètes qui doivent leur apparence au même composé carbonique qui les spectres stellaires III.*b*, et il y a des analogies aussi pour les autres bandes. Ainsi la bande à 563 mm. est souvent bien faible même dans de brillantes comètes, et la bande dans le vert est toujours la plus forte, aussi bien dans les comètes que dans les étoiles. La bande dans le bleu est quelquefois assez faible dans les spectres cométaires, tandis que dans les étoiles elle est seulement un peu plus faible que la bande dans le vert : mais il faut se souvenir qu'elle est située dans une partie déjà très faible dans les spectres des étoiles. Il est donc fort possible qu'un affaiblissement médiocre suffise pour rendre entièrement imperceptible la lumière restante. *Il n'y a donc peut-être pas à voir dans cela une diversité entre les comètes et ces étoiles.* Quant aux bandes violettes, elles sont très faibles dans les tubes de Plücker mais fortes dans le spectre de la flamme de l'alcool. On en a vu une trace dans les spectres des comètes les plus brillantes. Dans les étoiles III.*b* très brillantes et pas trop rouges, on a aussi une zone violette laquelle se termine, comme les mesures montrent, à la longueur d'onde 430 mm. donc à la position de la seconde de ces bandes, et à la position de la première il y a, dans les spectres de ces étoiles, une bande."

[2] *Monthly Notices R.A.S.*, vol. xlvi., p. 112

the great hydro-carbon band seen in the spectrum of every comet that has been examined under favourable circumstances. This identification is strongly supported by the second bright line, 513·7, which is also found both in hydro-carbon and cometary spectra. It is, however, on bringing the spectra of the star and of the blue flame of a spirit-lamp at the same time into the field of the spectroscope that their exact agreement becomes most evident. For not only do they agree perfectly in wave-length and in beginning with two plainly distinguishable bright lines, but also in the delicate gradations of light by which they similarly fade away towards the violet, thus forcing the extreme probability of a common origin upon the observer.

"But the presence of luminous lines does not rest on this single band, for the second cometary and hydro-carbon band which has its bright edge at W.L. 472·9 (Hasselberg, 'Ueber die Spectra der Cometen') is also found in the new star's spectrum at W.L. 472·2.

"Of the three other luminous bands agreeing with the coal-gas spectrum, which were all measured at Dun Echt in that of comet 1881 III.,[1] two lie beyond the limit to which I have yet traced the spectrum of this star, and the third, falling between W.L. 563 and 534, in a bright and otherwise difficult part of the spectrum, has not made its possible presence evident.

"This leaves the origin of the bright bands beginning at 542·8 and 494·4 an open question; but excepting their general appearance, there is no reason why they should be due to the same substance as the great band at 516·2. On the other hand, the presence of the bright hydro-carbon bands in a spectrum of type III. removes any difficulty there may be in accepting Secchi's conclusion that they appear in a reversed (dark) form in spectra of type IV."

Dr. Copeland also made determinations of the position of the bright bands in Nova Andromedæ,[2] and noted—

"It seems probable that the three 'bright' bands, of wave-lengths 546·8, 514·0, and 489·2, are identical with the three brightest bands afterwards measured with the same apparatus in Mr. Gore's Nova Orionis, of which the brightest parts were at wave-lengths 542·8, 516·2, and 494·4. The trace of a condensation of light at W.L. 471·6, seen on September 20, agrees well with the bright line in Nova Orionis at W.L. 472·2. . . . In conclusion, it seems worthy of remark that the spectrum described above is the same as that given by any ordinary hydro-carbon flame, burning so feebly that the spectrum of the blue base of the flame is just beginning to show through the continuous spectrum afforded by the white part of the flame."

Vogel made some observations of Nova Orionis,[3] and found that the wave-lengths of the absorption bands were the same as those of α Orionis and other stars of that group, the only difference being that the bright spaces were more strongly marked. Dunér also noted[4] very bright parts in the green and blue, which he identified as the bright zones 516·8–503·2 and 495·8–484·3. With respect to these bright parts, he thought they may be partly due to the contrast with the very dark and broad bands.

M. Ch. Trépied observed that the spectrum of Nova Orionis was like α Orionis and β Pegasi. He also remarked:—[5]

"Le 23 décembre, j'ai, pour la première fois, soupçonné l'existence de lignes brillantes dans le vert; mais cette observation est un peu incertaine. On sait combien il est difficile de décider si les apparences de lignes ou de bandes brillantes, dans un spectre faible, sont vraiment celles qui caractérisent l'état d'incandescence d'une matière gazeuse, ou s'il faut les attribuer à un effet de contraste causé par le voisinage des bandes obscures."

[1] *Copernicus*, vol. ii. p. 227. [2] *Monthly Notices*, vol. xlvii. p. 54.
[3] *Astr. Nachr.*, No. 2704. [4] *Astr. Nachr.*, No. 2707.
[5] *Comptes rendus*, vol. cii. p. 41, 1886.

M. Thollon observed the same Nova, and recorded—[1]

"Ce qui nous frappa tout d'abord fut l'éclat remarquable du rouge et surtout du vert, tandis que le jaune était relativement sombre. Cette particularité nous suggéra d'abord l'idée que nous nous trouvions en présence d'un spectre de bands brillantes, analogue à celui des comètes, mais bien plus compliqué. Les observations comparatives faites sur α d'Orion nous confirmèrent dans cette idée. Cette étoile, en effet, montre avec une parfaite évidence un spectre continu conservant partout l'éclat qui lui est propre, et coupé par des bandes et raies obscures."

With the exception of Dr. Copeland, however, no observer confronted the spectrum of the Nova with that of carbon, or the identification of the bright spaces with the carbon flutings would have been evident.

A short time after Dr. Copeland had published his observations, Mr. Maunder challenged the assertion[2] that in the Nova "the spectrum is not so much a continuous one, interrupted by dark lines and dusky bands, as *a not very luminous spectrum upon which a series of bright bands are superposed*." The accuracy of the observations was not, however, doubted, nor was the importance of the view denied.

The main objection urged by Mr. Maunder was that Dr. Copeland's measures of the bright parts in Nova Orionis did not exactly agree with laboratory determinations of the wave-lengths of the hydro-carbon bands. He does not, however, make mention of the fact that there are two perfectly distinct sets of bands seen under different conditions. Nor does he refer to the "laboratory work" which has been relied on to show that they are not hydro-carbon bands at all. The mean of Dr. Copeland's measures of the bright line in the green, beginning a band, is 516·2. The wave-length of the first carbon fluting of one series is given by Thalén as 516·4, which, therefore, gives a difference of 0002 in the two determinations. The line measured by Dr. Copeland at 513·7 is said by Mr. Maunder scarcely to support his view, since the second maximum of the carbon fluting has a wave-length 512·8, and of the blue carbon fluting it is noted, "The third hydro-carbon band, that in the blue with wave-length for its less refrangible edge 473·7, is indeed not far from the bright space Dr. Copeland has observed at λ 472·2, but the correspondence is certainly not very exact." Since this criticism was made, however, it has been shown that at different temperatures the maximum of the blue carbon fluting may shift from 468 to 474, so that Dr. Copeland's measures may represent the exact position of the band in the Nova.

But it is evident that a vast difference must exist between the accuracy attainable in the observatory and in the laboratory. Dr. Copeland's measures appear to give the smallest probable error in the determination of wave-lengths in such an object as Nova Orionis, yet he measured the brightest band once at 517·4 and on the following evening at 515·6, a difference of 0018. The difference of the observations *inter se* exceeds any of the differences between the bright parts measured by Dr. Copeland and the accepted wave-lengths of the carbon bands; nevertheless Mr. Maunder says the observations are "undoubtedly very accurate," hence it cannot reasonably be argued that the bright bands are not carbon because of a want of exact coincidence with those measured in the laboratory.

Mr. Maunder also notes that "the second band in order of brightness in the hydro-carbon spectrum begins at λ 563·4. This is certainly non-existent in spectra of the third type; a broad dark band—No. 4 in Dunér's nomenclature and my own, wave-length 564·2 to 559·2—occupies the very place." This contention, however, is no longer allowable, since the recent researches show that the carbon fluting of one series at 563·4 is masked by the

[1] *Comptes rendus*, vol. cii. p. 356, 1886.
[2] *Monthly Notices R.A.S.*, xlvi. p. 284.

absorption of the first manganese fluting at 557·6, and the same argument might be employed to abolish carbon from many cometary spectra.

My recent work has entirely justified Dr. Copeland's observations, and to him certainly belongs the credit of having established the existence of the carbon bands bright in a new star. J. NORMAN LOCKYER.

ON THE SOARING OF BIRDS.

THE interesting problem of the soaring of birds, though repeatedly discussed, especially in NATURE, has not yet found a satisfactory solution. This is the explanation I propose.

Suppose that a bird soaring horizontally with a certain velocity enters a current of air cutting his own course rectangularly. The bird will be seized and partly borne by the wind. Instead of passing by calm the distance *a* to *b*, he will advance from *a* to *c* in the same space of time (see Fig. 1; the arrow *ef* indicating the direction of

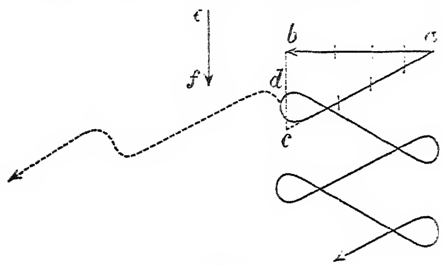

FIG. 1.

the wind, and the cross-lines the length-axis of the wing-area). The way *a* to *c* evidently being longer than *a* to *b*, the bird, on arriving at *c*, has a greater absolute velocity than if he had pursued, in a calm, his course *a* to *b*. It is equally evident that, if the initial velocity of the bird and the velocity of the wind are properly adapted, the velocity of the bird at the point *c* can, in spite of the resistance of the air to his advancing, be greater than at *a*. If arriving at *c* the bird can turn against the wind[1] without considerable loss of velocity, it is clear that he is able to continue his new course for a short space, before his velocity sinks to the initial velocity which he possessed at the point *a*. During this part of his course, the relative velocity of the bird (with relation to the air) is more than twice the absolute velocity of the wind, supposing the initial velocity of the bird equal or superior to that of the wind. Let *d* be the point where the absolute velocity of the bird has sunk to the initial velocity. If the bird turns at *d*, so that his course crosses the direction of the wind at right angles, he is again ready to begin the same course as when starting from *a*. Thus, on the way *a* to *c* the absolute velocity increases, on *c* to *d* it diminishes as much.

Let us now suppose the direction of the wing-plane unchanged: the course of the bird will no longer lie in the horizontal plane, but, from reasons now easily understood, *a* to *c* will gradually drop down to the earth, according as the relative velocity diminishes; on the other hand, *c* to *d* will rise according to the increment of the relative velocity. Which will be the greater, the sinking or the rising, depends on several circumstances, but principally on the force of the wind, the adaptation of the wing-plane, the size and form of the bird and the corresponding proportions between the bearing of the wings and the resistance of the air. This resistance is, of course, in proportion to the weight, less to the advancing of large birds than to the advancing of small birds. This is the reason why large and heavy birds are the best soarers.

[1] It has long been acknowledged that some birds possess the power of changing their direction without any sensible loss of velocity.

It results from this that a bird suitably built for the purpose can not only maintain the same level without working his wings, by a uniform and moderate wind, but also gain in height by adroit movements.

It may perhaps be objected that, according to this scheme, the course of the bird will not be spiral, but run in figures of eights gradually moving in the direction of the wind or in continuous windings on the one or on the other side and partly with the wind (Fig. 1). Indeed it is likely that the movements of the birds will often prove that they profit by this principle in manœuvres the purpose of which has not yet been understood.

The spiral soaring is still to be explained. I think we must suppose that commodiousness is the principal motive thereof. Let us fancy that a bird, having acquired the necessary initial velocity, soars in a calm without working his wings, not in a rectilinear course, but by suitable inclinations and turnings of the wings in circular courses. We know that, in order to perform this manœuvre, the bird drops the interior wing a little and raises the exterior wing just as much, so that the wing-plane, during this motion, forms a conic ring, the top of the cone pointing downwards. If the velocity did not diminish, the bird would be able to continue this course indefinitely, or he would rise or sink in a screw-formed course, according as the velocity should increase or diminish. By greater inclination of the wing-plane to the axis of the cone, the circles would become narrower; by diminishing inclination, they would become wider: both these motions are easily produced by minimal changes of the form of the wing-plane or of the place of the centre of gravity. Let us further suppose that the stratum in which the bird soars is continually moving in a certain direction. From the moment the course of the bird is perpendicular to the direction of the wind (point *a* in Fig. 2) till the moment

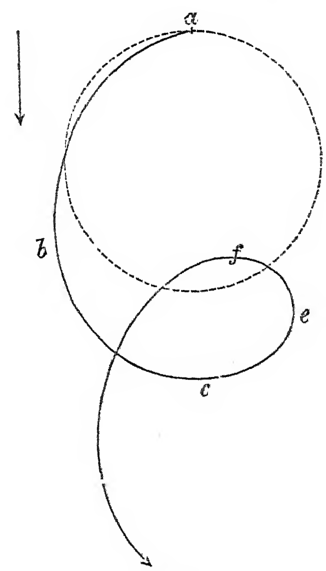

FIG. 2.

it grows parallel with it (*b*), the bird obtains from the wind an addition to his absolute velocity (not considering the loss occasioned by the resistance of the air) and also an increment of velocity from the moment his course deviates from the direction of the wind (*b*) till the moment it grows perpendicular to it (*c*). From this moment again the absolute velocity gradually diminishes, till, at last, at the point *f*, it reaches its minimum. From this point (*f*),

a new circle begins identical with the first one, if the absolute velocity in *f* is the same as that in *a*, which does not imply any impossibility, even including the resistance of the air to the advancing. It is, however, important that the increment of velocity during the course *a–b–c*, is equal to its diminishing during the course *c–e–f*. Certainly the resistance of the air caused by the wind is greater during the latter part of the course than during the former, but the way is shorter in which this greater resistance is working.

In which plane or in which planes the different parts of the course will pass depends upon the initial velocity and upon the changes of the relative velocity of the bird; naturally also upon the invariable quantities—the weight of the bird, the size and form of the wing-plane, so far as the latter has influence upon the resistance of the air to the advancing. Now in *a* and *f* the relative velocity is the same as the absolute or minimal velocity. In *c* the relative velocity is also the same as the absolute velocity, but in *c* they are both greater than in *a* and *f*, as we have shown here above. Thus the relative velocity has increased during the course *a–b–c*. During the course *a* to *b* no increment has occurred, on the contrary; so much the faster has it increased during the course *b* to *c*. During the course *c* to *e* the relative velocity increases continually, and obtains near *e* its maximum; whereas it gradually diminishes during the course *e* to *f*, so as to equal the initial velocity. Suppose then that the relative velocity diminishes somewhat during the course *a* to *b*. This diminution, however, will be over-compensated during the course *b* to *c*, the relative velocity in *c* being greater than that in *a*. During the whole course *c–e–f*, the relative velocity is greater than in *a* and *f*. Surely the supporting power of the current of air on the wings depends upon the relative velocity. It increases with the relative velocity, if we suppose everything else to be unchanged, particularly the angle of inclination of the wing-plane. If, therefore, the initial velocity in *a* by a certain pointing of the wing-plane is only just sufficient to maintain the bird at an unchanged level, the bird must, when describing the course *a* to *b*, gradually drop down. Even on the other side of *b* the sinking is continued until the relative velocity has increased so as to regain the same value as in *a*. From this point the course begins to rise and will continue rising until *f*, for to this point the relative velocity is greater than in *a*. Under such circumstances we cannot be astonished if the part *f* of the course will be in a higher plane than the part *a*, even if the resistance of the air to the advancing is infinitesimal.

Should the initial velocity in *a* be greater than what is required to maintain the bird on the same level, the bird would already there have a rising course, and it might easily happen that no part of the course would be descending. However, the resistance of the air increases much faster than the relative velocity, and therefore the most available initial velocity will be different for different birds and for different force of the wind. It is not as yet an easy matter to calculate the most favourable initial velocity to certain birds and to certain winds. But the discrepancies in the descriptions of the forms of the circles find, as may easily be seen, their explanation in supposing a different initial velocity. This is likely to be chosen differently by different birds, and may be different for the same bird according to different force of wind.

I am convinced that the bird always, even when soaring with the wind, has a greater velocity than the wind, and that thus during this part of the circle his speed is not hastened by the wind, but on the contrary he is here delayed, maybe less than in the other parts of the course. On the other hand, the velocity of the bird is augmented by the wind, as soon as the wind catches the bird from the side or obliquely from behind. This gain of velocity covers the loss caused by the resistance of the air to the advancing, and consequently allows the bird to maintain the necessary average velocity. It is less obvious, but nevertheless very likely, that the soaring bird, having gained the necessary velocity and having pointed his wings suitably, can, without changing the form of his wings, incessantly continue the soaring, as long as the force of the wind is unchanged.

Mr. Peal's [1] explanation no doubt comes nearest the truth, when he compares the soaring bird to a kite. We may consider the bird a kite, but the string which connects him with the earth is not fixed at a point of the surface of the earth, but the point of fastening moves with the wind, though it may be slower than the wind. It is the difference of velocity between the motion of the fastening-point and that of the air which affords the necessary power for the support and the rising of the bird.

MAGNUS BLIX.

Lund, Sweden.

ELECTROLYSIS OF ANIMAL TISSUES.

A PRELIMINARY account of part of the work was given in the Proc. Roy. Soc. Edin., 1888, and a short description of later results at a meeting of the Physiological Society at Cambridge in March last. The full paper is being published in a volume of memoirs from the physiological laboratory of the Owens College, Manchester. The chief results are here summarized.

(1) The first part of the research was directed to answering two questions: (*a*) *Is the conduction in animal tissues entirely or chiefly electrolytic?* (*b*) *What are the electrolytes?* It is shown that by far the greatest part of the conduction at any rate is electrolytic, and that the best conductors by far are the inorganic constituents of the tissues. Next to these, but at a great distance, come some of the nitrogenous metabolites. The proteids are exceedingly bad conductors.

(2) *The changes produced in simple proteid solutions* were next investigated. It is shown that the proteids are affected not by primary electrolysis, but by the products of electrolysis of the salts.

The effects vary to some extent with the current density. In solutions of coagulable proteids alkali-albumin is formed at the cathode, and acid-albumin at the anode, some of the proteid being coagulated at the latter.

(3) *The effects of electrolysis on isolated tissues and on some of the liquids of the animal body.*

Striped Muscle.—Great changes were found in the microscopic appearance of the fibres. The nuclei became very prominent in those near the anode, with apparent coagulation of the sarcous substance, suggesting the action of a dilute acid. At the cathode the fibres were more homogeneous than before. The striation was impaired. Chemically, the same changes in the proteids were found as in simple proteid solutions, and a distinct effect on the distribution of the salts was made out, by estimating the ash in different parts of the muscle.

Blood.—Entire defibrinated blood, blood serum, and pure hæmoglobin solutions were used. There was no indication that hæmoglobin, or any derivative of it, acts the part of an ion. At the anode the reaction becomes acid, and acid-hæmatin is formed, which remains partly in solution and is partly thrown down, the solution becoming less deeply coloured. When the current is strong or long continued the hæmatin suffers further change and is decolorized, apparently by the oxygen or chlorine set free. If a reducing agent is present at the anode, the hæmoglobin there is not affected by the electrolysis. At the cathode alkali-hæmatin is ultimately formed, although its less definite spectrum does not show itself so soon as that of acid-hæmatin at the anode. The proteids of the serum and corpuscles are

[1] NATURE, vol. xxiii. p. 10.

partly coagulated at the positive pole. At the cathode they are partly changed into alkali-albumin.

Bile and urine were taken as further examples of animal liquids.

(4) *The effect of electrolysis in the living body.*

Pithed frogs and anæsthetized rabbits were used. This part of the work is still incomplete.

G. N. STEWART.

LOBSTER CULTURE IN THE ISLE OF MULL.

WE have been favoured with a circular, issued by Mr. George Brook, Lecturer on Embryology in the University of Edinburgh, and Mr. W. L. Calderwood, late of the scientific staff of the Fishery Board for Scotland, expressive of an intentention to establish at Lochbuie a small marine laboratory. The promoters have set themselves to restore our shell fisheries to their former condition; and a leading item in their programme is the proposal to construct a lobster pond, with suitable apparatus for hatching and rearing lobsters. The cost of the entire laboratory, with pond and plant, is estimated at £400, that of maintenance at £150 per annum—exceedingly moderate sums, for which an appeal is made to the public. The condition into which our lobster fisheries have lapsed is shown by the fact that a lobster ground in the far west of Ireland is worked by a South of England boat. Our import lobster trade is yearly increasing, and the fact that our markets are not home-stocked is discreditable in the extreme. The problem of artificial culture necessary for the purpose in view has many times been attacked by British naturalists. Saville Kent had it constantly in mind while officiating at our several aquaria; he made it a primary object in his schemes for the establishment of marine stations in Jersey and at Brighton, and he meanwhile attempted to raise interest in it in a paper read at the International Fisheries Exhibition held at South Kensington. All this notwithstanding, the matter has, with us, not yet passed beyond the experimental stage, and we are behind in the international race. At Lochbuie the conditions should be favourable; and as Mr. Brook, in the preparation of his *Challenger* Report, has shown himself capable of performing a difficult task under exceptional conditions, we have full confidence in his ability to carry out his project. The promoters of this scheme propose in other respects to pursue a course of scientific study of the marine fauna of the west coast of Scotland, but their chief aims are unmistakably economic. We sincerely hope that they will confine their attention to the one or the other branch, for nothing can be plainer than that the extraordinary successes which have placed the fishery work of our American cousins foremost in that of the world, have been largely, if not wholly, due to their having kept pure science and economics scrupulously apart. The Lochbuie scheme is a modest though an ambitious one, and Messrs. Brook and Calderwood signify their intention of giving their services as superintendents. Recent proceedings in Parliament have shown that there is disaffection on the Scottish Fishery Board; and it would be an interesting circumstance should private enterprise, which has done so much for science in Britain, solve the difficulty in hand, while the State-aided body fritters away a handsome endowment.

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE nineteenth meeting of the French Association for the Advancement of Science opened at Limoges on the 7th inst.

After some remarks on the learned societies of Limoges, and some references to Gay-Lussac, the inauguration of whose statue took place on the 11th inst., Prof. A. Cornu, the President of the Association for the year, delivered a discourse on the part played by physics in the progress of the sciences. It is impossible in the space at our disposal to do justice to this interesting address, but the following will give an idea of its character.

Beginning with chemistry, Prof. Cornu pointed out that the introduction and use of the chemical balance by Richter, Wenzel, Dalton, and Lavoisier led to the substitution of the laws of multiple and equivalent proportions, and the indestructibility of matter, for the vague hypotheses held by the alchemists.

Two other physical instruments introduced into chemical methods are the calorimeter and barometer. By means of the first, Dulong and Petit's law, that the same quantity of heat is required to heat an atom of all simple bodies to the same extent, was discovered: and but for the second, Gay-Lussac could not have made his researches on vapour density, which, with the work of Ampère and Avogadro, led to the determination of the numerical relation between the temperature density and pressure of a gas and the notion of atomic volume.

Another common physical instrument, the thermometer, has furnished organic chemistry with the means of discovering important laws of organic series; and recently, with the calorimeter, it has enabled M. Rault to determine molecular weights by the freezing of dissolvents, and has furnished Thomson, Berthelot, Sarrau, Vielle, and other workers in thermo-chemistry with the means of arriving at the new mechanics of the affinity of atoms according to their size, like the universal law of gravitation.

The introduction of the spectroscope into the chemical laboratory for purposes of analysis, by Bunsen and Kirchoff, marks an important epoch in the history of chemistry. This instrument has been entirely created by the labours of physicists; the prism of Newton, the telescope of Fraunhofer, and the collimator of Babinet marking stages in its evolution Bunsen and Kirchoff demonstrated the power of their method of analysis by the discovery of rubidium and cæsium; in fact, it is only necessary to observe an unknown line in the spectrum of a substance to establish the existence of a new element.

It appears therefore that each time chemistry has borrowed from physics some new method it has entered into a prolific field of investigation, conceptions have been extended and given a more precise meaning, and chemical knowledge advanced in a manner proportional to the power of the adopted methods.

The other natural sciences have benefited in the same way. Up to the seventeenth century astronomers had no means of assisting their vision, and therefore they could only make observations of the movements of the heavenly bodies. In spite, however, of the simplicity of the means of observation, the work of Hipparchus, Ptolemy, Copernicus, Tycho-Brahé, and Kepler contained a considerable amount of information with respect to celestial motions, but nothing was known of the constitution of the bodies observed. With the refracting telescope of Galileo and Newton's reflector, astronomy underwent a transformation: the sun was found to have spots and faculæ; the plains, mountains, and craters of the moon were observed: Venus was shown to go through phases in the same manner as our satellite; Jupiter's belts and satellites were seen; and the beauty of Saturn and his rings revealed.

Later, Herschel's large mirrors, worked by his own hands, enabled him to discover double and multiple star systems; to prove that many stars are suns like our own, inasmuch as they have other bodies revolving round them.

Such was the revolution produced in astronomy by the employment of the first optical instruments. The intro-

duction of the spectroscope considerably extended the limits of investigation. The chemical constitution of the stars was determined in spite of their immense distances; the sun was shown to contain sodium, iron, magnesium, calcium, hydrogen, in a state of vapour at its surface—that is, the same elements as those which make up the earth's crust; it also contains nickel, an important constituent of meteorites, those nomadic bodies which fill interplanetary space. The sun and the bodies revolving round it are therefore composed of the same elements.

By means of the spectroscope it has been proved that the moon and the planets shine by reflected light, and that the stars, like the sun, are self-luminous, and made up of the same elements, thus demonstrating the unity of the chemical composition of the whole universe.

But the spectroscope has not only revealed the substance of the stellar world, it affords a means of investigating a component of stellar motion. The principle enunciated by Doppler, viz. that light-waves, like those of sound, vary in length with the relative velocity of the source producing it, remained unapplied for some time because there was, of course, no means of determining the proper colour of a star in repose and comparing it with that received, the variation being produced by motion in the line of sight. Fizeau showed, however, that by substituting lines in the spectrum for the idea of colour the conditions necessary for the application of the principle were met; all that was required being a line common to a star and some terrestrial element, and the measurement of the displacement of this line. This method was proposed by Fizeau in 1859, and has been considerably developed; numerous lines in stellar spectra are coincident with those of terrestrial substances. If they are all shifted towards the red the star is receding from the earth; if towards the violet the star is approaching us. The displacement of the line is measured with a micrometer, and a simple calculation gives the velocity with which the star is moving, whatever may be its distance.

It has been shown that for the application of the Doppler-Fizeau principle it is necessary to find in the spectrum of the star the lines of a terrestrial element. This common element is most often hydrogen—the simple body *par excellence*, the elementary substance of those who hold in the unity of matter.

Among all the methods of rendering impurities manifest, the simplest and most delicate is that of spectrum analysis. With the spectrum of hydrogen observed in the laboratory feeble lines of other substances are always present, and to decide upon the true hydrogen spectrum becomes therefore a difficult matter. But it was an astronomer and not a chemist who first described the pure hydrogen spectrum; the lines photographed by Dr. Huggins in the spectra of the white stars having since been shown to be reproduced in the laboratory when the spectrum of approximately pure hydrogen is observed.

In physics, the centre of natural philosophy, many branches have made rapid and definite advances. The results of the development of electrical science is seen on all sides, yet no science has had a more humble beginning. The first electrical experiment was made six centuries before our era: this was the attraction of light bodies by rubbed amber. The knowledge remained in this stage for more than twenty centuries; then the two electrical states were gradually recognized, and conductors and non-conductors were separated. In the establishment of the identity of atmospheric electricity with that obtained by electrical machines the death of Richmann at St. Petersburg should be noticed, and the discovery of the lightning conductor by the illustrious Franklin.

Everyone knows the story of the convulsive movements of a frog's leg in contact with a bimetallic arc observed by Galvani, an Italian physiologist. Volta saw in this circumstance that electricity might be developed by the contact of different substances; he discovered the law which permitted the energy to be multiplied; and in 1794 summed up all his works in an imperishable monument—the voltaic pile.

All the sciences benefited by the discovery, but chemistry gained the most. Carlisle and Nicholson decomposed water; Davy, with the great pile belonging to the Royal Institution of London, decomposed the alkalies and alkaline earths, formerly supposed elementary bodies. Later, Davy performed an experiment which eclipsed everything accomplished with the invention of Volta. By joining two carbon poles to his colossal pile, he produced a dazzling and continuous light, and discovered the electric arc now so commonly seen.

In 1820, Oersted discovered that the wire joining the poles of a pile exercised an influence on a magnetic needle. Ampère discovered the mutual action of electric currents, the mathematical law governing it, and, finally, the production of magnetism by the sole action of the voltaic current.

The discovery of the electro-magnet was a great event, not only in the history of science, but in that of humanity. In telegraphy it is the electro-magnet which transmits messages from one end of the world to the other with the velocity of light; in the telephone, the word itself; in the powerful machines derived from the memorable discoveries of Faraday, it is that which causes the transformation of energy.

Great advancements have also been made on the purely theoretical side. Ampère, Poisson, Fourier, Ohm, Gauss, Helmholtz, Thomson, and Maxwell have done much to connect electricity with mechanical laws. Again, electro-magnetic and optical phenomena obey the same elementary laws, and appear to be two manifestations of the movement of the same medium—the ether; thus optical problems may be settled with the equations of electro-magnetism. From an experimental point of view, results full of promise have already been obtained; the velocity of light, found by optical methods, has also been determined by measures purely electrical; and recently M. Hertz has accomplished experimentally the identification of electrical discharges with light-waves.

All these facts show that as our knowledge increases the distinctions between different branches of science vanish; the limits which have been traced between them are shown to be artificial, and only testify to ignorance of natural laws; but the efforts of successive generations have not been in vain, and we look forward to the time when these limits will be effaced, and all the branches of natural philosophy be united in one harmonious whole.

Prof. Cornu's discourse, of which the foregoing is but a sketch, was received with much applause.

After an address by the Mayor of Limoges, the Secretary of the Association, M. A. Gobin, read the report for 1889–90, and gave an account of the meeting in Paris last year. The financial statement by M. Gallante shows that the Association is in a prosperous condition, and increasing its number of members.

Many interesting and important communications were made in the different sections. The series of excursions included one of three days' duration, and visits were made to all places of interest in or near Limoges. The Congress will be remembered as a very successful one by all who were fortunate enough to be present.

C. H. F. PETERS.

BY the death of Prof. C. H. F. Peters, Director of the Litchfield Observatory, Hamilton College, Clinton, N.Y., astronomy has lost an assiduous observer. An interesting sketch of his career is given in the *Astronomische Nachrichten*, from which the following details are taken.

He was born at Coldenbüttel, in Schleswig, on Sept. 19, 1813, and educated at the Gymnasium of Flensburg and the University of Berlin, where he studied mathematics and astronomy. Having taken his degree in 1836, he tried to obtain an appointment at the Observatory of Copenhagen, but his application was not successful. He then went to Göttingen, to carry on his studies under Gauss. Afterwards he was induced to undertake some scientific investigations relating to Mount Etna.

Having accomplished the task entrusted to him, he was made Director of the Trigonometrical Survey of Sicily. Of this appointment he was deprived in 1848, when he gave great offence to the authorities by expressing sympathy with the revolutionary party. He escaped on board an English vessel to Malta, but soon returned to Sicily, and joined the Sicilian army under Mieroslawski, acting first as a captain, then as a major, in the engineering branch of the service. It was under his direction that Catania and Messina were fortified. When Palermo fell into the hands of the Neapolitans, in May 1849, he fled to France, but soon changed his residence to Constantinople, where he proposed to devote himself in peace to scientific research. Here he secured many friends, and it was intended that a scientific expedition, under the guidance of Peters, should be sent to Syria and Palestine. Various obstacles, however, stood in the way; and the scheme had to be given up after the outbreak of the Crimean War.

He now turned his attention to the United States; and, with recommendations from Alexander von Humboldt, he went in 1854 to Cambridge, Mass., and from thence to Washington, where he obtained an appointment on the Coast Survey. After working for some time in this position, and establishing an observatory at Utica, he accepted, in 1858, the offices of Director of the observatory at Clinton, N.Y. (now known as the Litchfield Observatory), and Professor of Astronomy at the Hamilton College. The duties of these offices he continued to discharge until his death, which took place on the morning of July 19. He was found dead on the road between the observatory and his house, and seems to have died of heart-disease when returning from his work.

In 1874 Peters acted as chief of the North-American Expedition to New Zealand for the observation of the transit of Venus. He discovered no fewer than 48 minor planets and several comets, and much important work was done at Hamilton College under his supervision, his celestial charts and star catalogue being of considerable value. Few astronomers leave a better record behind them, and his death is much regretted.

NOTES.

THE annual excursion of the Belgian Royal Malacological Society took place under the guidance of MM. X. Stainier and J. S. Gardner. The Eocenes from the Thanets to the Lower Bagshots were examined at Herne Bay and Sheppey, where Mr. Shrubsole assisted, and the Gault and Chalk at Dover and Folkestone. The Eocenes seen were pronounced to be in all respects identical with beds of corresponding age in Belgium. The Society proposes to revisit England next year.

AMONG the excursions which have been arranged by the Local Committee of the British Association is one to Malham in Craven. This is to take place on Thursday, September 11, under the guidance of the officers of the Yorkshire Naturalists' Union. The district to be investigated is the plateau of Malham and the escarpment which it forms along the South Craven fault. It includes the only lake in the West Riding, and the remarkably picturesque scenery of Malham and Yoredale. In addition to these attractions, every branch of natural history can be successfully pursued in this locality. This advantage arises chiefly from the diversified character of the geological formations, which include Silurian rocks, mountain limestone, Yoredale shales, and millstone grits. We believe that this will be a popular excursion among members of Field Naturalists' Clubs, who will have an opportunity of observing the methods of work adopted by the Yorkshire Naturalists' Union, and it is hoped that as this will be an essentially working excursion, any field naturalists and geologists who may take part in the Leeds meeting of the British Association will attend the Malham excursion.

WE learn that the French physicians who went to the International Medical Congress at Berlin were much gratified by the cordiality with which they were received.

IN a letter to the *Times* the other day, Mr. John Cordeaux referred to a unique collection of migrating birds formed at Heligoland by Herr Gätke as the result of work carried on during 40 years. This collection, he added, was to be brought to England, "having been secured to the nation by the munificence of a single individual." With reference to this statement, Prof. W. H. Flower writes from the British Museum (Natural History), Cromwell Road, to the *Times* as follows:—"May I supplement the letter of Mr. Cordeaux by saying that the individual by whose liberality Herr Gätke's collection has been secured for the nation is Mr. Henry Seebohm, and that arrangements are being made by which, when the collection arrives, it will be permanently exhibited in this Museum?"

A WORK on "The Birds of the Japanese Empire," by Mr. Henry Seebohm, is nearly ready for publication. It is illustrated with numerous woodcuts. Mr. R. H. Porter is the publisher. The same publisher has in the press "The Birds of Sussex," by Mr. William Borrer. This work is supplied with a map of Sussex, and with six coloured plates by J. G. Keulemans.

MESSRS. MACMILLAN AND CO. announce for publication this week an English translation of Prof. Ostwald's "Grundriss der allgemeinen Chemie" by Dr. J. Walker, of Edinburgh University. This work covers the same ground as the author's classical "Lehrbuch," but the treatment throughout is elementary, and, as far as possible, non-mathematical. The new modes of molecular-weight determination, van 't Hoff's theory of osmotic pressure, Arrhenius's hypothesis of electrolytic dissociation, and the interesting applications of these to purely chemical problems —all receive special attention at the hands of the author. The appearance of the book is particularly well-timed, as we learn that Profs. Ostwald, Raoult, van 't Hoff, and Dr. Arrhenius have intimated their acceptance of the invitation issued to them by the British Association, and will be present at the coming meeting in Leeds.

THE first volume of a work by Prof. A. de Mortillet, on the origin of hunting, fishing, and agricultural pursuits among primitive races, has just been published. It contains many interesting representations of prehistoric implements in the Saint Germain Museum.

ATTENTION has been called in various quarters (England, Belgium, France, and Germany) to the remarkably cold weather prevailing of late years, since 1885, in Central and Western Europe; the yearly averages being constantly under the normal. It now appears from an Algerian record, that these years have been warmer than usual in Algeria. It is also shown that there has been no change in the frequency of north and south winds, while in Europe the north-east winds have been increasing in frequency.

IN the new number of the Journal of the Anthropological Institute the most elaborate paper is one on the Dieri and other

kindred tribes of Central Australia, by Mr. A. W. Howitt. There are also papers on characteristic survivals of the Celts in Hampshire, by Mr. T. W. Shore; skulls dredged from the Thames in the neighbourhood of Kew, by Dr. J. G. Garson; and a new spirometer, by Mr. W. F. Stanley. In the paper on Celtic survivals in Hampshire, Mr. Shore refers to the feeling with which the May-day sunrise was regarded by the ancient Celts. "This May-day sunrise," he says, "was certainly reverenced in mediæval Christian time as well as in pagan Celtic time, for the line of about 20° north of east is the line of orientation of a large number of the oldest churches in Hampshire, and of many in other counties. It is a common orientation among the oldest churches of Hampshire, in which county there are as many as seventy examples of it. I cannot explain this on any other ground than the survival of a reverence for the May-day sunrise from Celtic pagan time to Saxon Christian time, and under a modification to a later date. It appears to me that, as there is evidence of the survival of part of the Celtic people, it is not surprising to find that traces of their May-day customs have survived also. It is of course possible that in this common line of orientation of many old churches we may see all that remains of one of the customs of the old British Christianity which existed before the coming of the Saxons."

According to the *Journal de la Chambre de Commerce de Constantinople*, quoted by the *Board of Trade Journal*, the silk section of the Agricultural Society of Moscow has offered a prize of 500 roubles for the best work on the anatomy and embryology of the silkworm. Works on this question must be sent not later than January 1, 1892.

The Rio Negro Salt Company seems to have had an interesting stall at the Rural Exhibition recently held at Palermo, near Buenos Ayres city. The Buenos Ayres *Standard*, in an article quoted by the *Board of Trade Journal*, thus calls attention to the subject:—"Here, in an unpretending but exhaustive manner, are displayed the products of those vast *salinas* or salt lakes which lie some few miles north of the town of Patagones, and which this company has recently commenced to work. There are large blocks composed of big crystals taken in the rough from the *salinas*; barrels of natural brine; compressed cakes for cattle; coarse salt for hides and meat curing; ground salt for kitchen use; and refined salt, dazzling as snow, and in every way equal to the English bottled salt, for use at the domestic table; in short, salt in every form that can be desired either for practical wants or the dainty demands of luxury. Pamphlets are distributed containing analyses by eminent men of science, which demonstrate the excellence and purity of its quality, and its adaptability for all known purposes. As regards quantity, we were informed that a calculation had been made that in a given year it would be possible to take from the Rio Negro lakes, occupying an extension of about nine square leagues, upwards of two millions of tons, and that, in the ensuing season an equal quantity of salt would be found, owing to the fact that every winter the lakes became filled with a brine of a density of from 25° to 32°, which in due time becomes a solid cake of salt."

A *Zeitschrift für Psychologie und Physiologie der Sinnesorgane* (*i.e.* organs of sense), has been recently started in Germany (April), under the editorship of Herren Ebbinghaus and König, the former of whom is known for some remarkable researches on the memory, and the latter for his studies in physiological optics. Among the contributors are Herren Aubert, Exner, Helmholtz, Hering, and other eminent men of science. The following are some of the subjects that have been dealt with: disturbance of the perception of very small differences of brightness by the proper light of the retina: simultaneous contrast; disappearance of after-images in eye-movements; memory of regularly successive and equal sound-impressions.

In the *Victorian Naturalist* for June, Mr. G. Lyell, Jun., of South Melbourne, notes that while walking along the edge of a mountain stream in Gippsland last January he observed a peculiar habit of the Victorian butterfly, *Papilio macleayanus*. One of these butterflies was seen to alight close to the water, into which it backed till the whole of the body and the lower part of the hind wings were submerged, the two forelegs alone retaining their hold of the dry land. After remaining in this position for something like half a minute it flew away, apparently refreshed. "During the morning," says Mr. Lyell, "I noticed quite a number doing the same thing. In one instance no less than four were to be seen within a space of not more than three yards, and to make sure that I was not deceived I captured several as they rose from the water, and found in each case the body and lower edge of the hind wings quite wet. While in the water the fluttering of the wings, so noticeable at other times, was suspended, and so intent were the butterflies in the enjoyment of their cold bath that they would hardly move, even when actually touched by the net. Apparently the heat of the weather drove them down to the water, as immediately they emerged they flew up again to the hill-sides. I have often noticed butterflies of the *Nymphalidæ* family settling near the pools, and apparently imbibing the moisture from the damp sand round the edges, but never before have I seen butterflies enter the water. Possibly it may be a peculiar habit of this particular species or genus. Numbers of the white butterfly, *Pieris harpalyce*, were flying about at the same time, but I noticed none alight near the water."

Some interesting observations on the growth of vegetation in the numerous lakes to the east of the Baltic have been lately made by Herr Klinge (*Engler's Bot. Jahrb.*). This growth depends on the mean direction of the wind during the period of vegetation. As south-west winds prevail in that region, the south-west border of a lake is protected, and the grassy and mossy growth naturally begins there, and spreads by degrees round the north and south ends. The north-east bank, on which break the waves from the south-west, shows hardly any trace of vegetation. It is generally steep, and tends to retire under the action of the waves. Something similar is met with in the Baltic: shore-meadows occur in the islands only on the east, wind-protected coasts. Further, rivers are displaced in the direction of the prevailing winds, eating away their eastern banks, while the western grow. The dead arms of the lower Embach are, with few exceptions, on the south side of the river, which, under the action of the wind, has been displaced northwards, *i.e.* (with reference to its direction) to the left, and so, contrary to Baer's law of river-courses. Indeed, the author rejects this law, and holds the principle of displacement according to prevailing winds to be universal. It is noteworthy that this relation has of late been pointed out by several Continental observers independently. Herr Klinge further finds that in the region east of the Baltic, hygrophilous (or moisture-loving) plants grow on the south-west side of the hills, and xerophilous plants on the north-east side.

The Märjelen Lake, which lies in Upper Valais, lately burst the glacier dam which lay across the valley. According to the Swiss *Vaterland*, a peasant who was close to the lake at the time declares the scene was most terrible and indescribable. When the ice dam gave way, the vast mass of water came tumbling out, sweeping away the huge fragments of the glacier, with the rocks upon it, tumbling into the crevasses, bursting them up in turn, and rising over the glacier in gigantic waves, again to carry all before it. Just at the end of the glacier the valley had narrowed into a little defile,

while the face of the glacier was some hundreds of feet high. The water seemed to have tunnelled under the ice, which, attacked above and below, gave way at last with a deafening crash, while the flood hurried down the mountain-side into the Rhone. The lake is nearly 8000 feet above the sea-level, and usually discharges its surplus water by subterranean channels, occasionally bursting its ice barriers as on the present occasion. The cantonal Government are constructing an overflow canal, which, it is hoped, will put an end to these periodical outbursts.

IN a paper printed in the new number of the Transactions and Proceedings of the New Zealand Institute, Mr. Taylor White describes an extraordinary meteor which he saw at Wimbledon, Hawke's Bay, on May 4, 1888, between 8 and 9 o'clock p.m. The nucleus, or head, was of oval form, of a transparent light-yellow colour, as of iron at a white heat. The tail was in the form of the tail of a pheasant, expanded—that is, the two centre streamers were of uniform length, the outer ones gradually shortening, so that the outermost streamer on either side was very much shorter than those in the middle. These streamers were of a dull, opaque orange. They were distinctly divided each from each by dark bands, which consisted of several fine black lines, to, probably, the number of five in each band. Mr. White is unable to fix the number of orange streamers, but would guess ten as probably correct. The colours blue and green were also certainly present. No sound was audible while the meteor was in view. "But," says Mr. White, "after I had gone into the house, and was describing what I had seen, the sound of its striking the earth or sea was heard—a loud and lengthened noise, to me like the violent shaking of all the forest trees, and evidently above ground, thereby differing from the sound accompanying an earthquake—coming from the westward; and this was followed, after a hardly perceptible interval, by a fainter sound, like an echo, to the north-east. The time which elapsed till the sound was heard was from three to five minutes." Various New Zealand daily journals gave full descriptions of the phenomenon at the time. According to the *New Zealand Times*, the apparent size of the meteor was "quite half that of the full moon."

THE editor of the Journal of the Royal Agricultural and Commercial Society of British Guiana contributes to the June number some interesting notes on luminous larvæ. Speaking of a form referable to the *Elateridæ*, or spring beetles, he says its luminosity, when observed in a dark place, is singularly striking and beautiful. The light is emitted along the whole length of the body—the head, the front part of the anterior segment, and the last segment of the body, being altogether luminous, while each intermediate segment gleams from a small area on each side of the back, two regularly-arranged rows of golden brilliants being thus observable. The light is continuous, and very bright, but it is intensified when the little creature is irritated. At intervals, one or more of the dorsal lights will be observed to be very dull or nearly extinguished, but apparently they are never quite put out.

MR. T. D. LATOUCHE, who contributes to the Records of the Geological Survey of India a paper on the sapphire-mines of Kashmir, takes the opportunity to offer some remarks on the extent to which the natives of India know the mineral resources of their country. He thinks he is not far wrong in saying that in very few instances in India have useful minerals been discovered in localities that were unknown to the natives, and in which the ores had not been worked by them at one time or another. Even the more uncivilized hill tribes are more or less well acquainted with the minerals their hills contain, and are by no means in the condition of the Blacks of Australia or the Bushmen of Southern Africa, in whose country the European prospector has found so great a field for his energies. To take a single instance: the Khasis of Assam, who, till the beginning of the present century, had hardly felt the influence of Western civilization, have for ages obtained their iron from an ore which occurs as minute grains of magnetite disseminated in the granite of their hills. Many a highly-trained European geologist might justly have been sceptical as to the possibility of obtaining a productive iron ore from granite, and would very possibly have passed the rock over as being utterly useless for such a purpose. Yet the Khasis discovered the mineral, and in all parts of the hills ancient heaps of slag testify to the use they made of their discovery; moreover, they obtained the ore by a process which was ingenious and even scientific—in fact, a kind of hydraulic mining somewhat similar to the latest process devised for obtaining gold in California. Can it be doubted that, if any other useful minerals existed in their hills, the Khasis would have found and worked them long ago? Similarly, in Kashmir, any mineral deposits that exist are probably well known to the natives, and, if useful, are already worked, and these are not of any great importance.

AT a recent meeting of the Wellington Philosophical Society, New Zealand, Mr. Hulke exhibited a specimen of a strange spider that carried its young on its body without web or filament, but simply attached to the body, until they were able to run by themselves.

MR. JOHN WHELDON has issued Part I. of a catalogue of botanical works, including the library of the Rev. M. J. Berkeley.

THE Glasgow and West of Scotland Technical College has issued its Calendar for the year 1890–91. The main objects of this institution are to afford a suitable education to those who wish to qualify themselves for following an industrial profession or trade, and to train teachers for technical schools.

WE have received the author's Hairless Paper-Pad Holder and Paper-Pad, issued by the Leadenhall Press. The Paper-Pad consists of a block of fifty sheets ($7\frac{1}{2} \times 8\frac{3}{4}$) of smooth and cream-tinted paper mounted together on a stout piece of blotting-paper, the price charged being only that for common scribbling-paper. The Paper-Pad holder is made of light wood, and should be grasped by the left hand, the right hand being free to travel over the surface of the paper-pad which is placed on it. After each sheet is used, it is torn off and placed under the pad and so blotted, and by this means the height of the pad and holder is kept constant. For writing in railway carriages, and for reporting, this form of support will be most serviceable, and it might also be used for the support of sketching-blocks.

MESSRS. MARLBOROUGH, GOULD, AND CO. are issuing what they call Marlborough pamphlet cases, the object of which is to preserve pamphlets from dust and destruction. The cases have no springs or other contrivances that could injure the contents by pressure, and in the bookshelf they resemble ordinary volumes, being "rounded and cloth-backed." The makers have sent us a case specially intended for numbers of NATURE.

THE additions to the Zoological Society's Gardens during the past week include two Brown Bears (*Ursus arctos* ♂ ♀) from Russia, presented respectively by Mr. A. C. de Lafontaine and Mr. D. B. Gellibrand; a Panolia Deer (*Cervus eldi* ♂), a Common Goat (*Capra hircus* ♂) from British Burmah, presented by Mr. Charles C. Galbraith; a Water Pipit (*Anthus spipoletta*), European, presented by Commander W. M. Latham, R.N., F.Z.S.; ten Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. W. Mauger; seven Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; an Axis Deer (*Cervus axis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on August 21 = 20h. 0m. 41s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4532	—	Bluish.	19 54 47	+22 25
(2) D.M. + 35° 4001 ...	9	—	20 6 4	+35 51
(3) D.M. + 35° 4013 ...	9	—	20 7 42	+35 51
(4) D.M. + 36° 3956 ...	9	—	20 10 25	+36 19
(5) 13 Sagittæ	6	Yellowish-red.	19 55 5	+17 13
(6) γ Sagittæ	4	Yellow.	19 53 54	+19 12
(7) θ Aquilæ	3	White.	20 5 36	− 1 9
(8) 229 Schj.	6·5	Red.	19 25 26	+76 21
(9) S Cephei	Var.	Very red.	21 36 35	+78 8

Remarks.

(1) This is the so-called "Dumb-bell Nebula." Dr. Huggins's record of the spectrum is as follows:—"The light of this nebula, after passing through the prisms, seemed concentrated in a bright line. This line appeared nebulous at the edges. No trace of the other lines was perceived, nor was a faint continuous spectrum detected. The light from different parts of the nebula is identical in refrangibility, and varies only in degree of intensity." The line referred to is, of course, the chief nebula line, near λ 500, and it will be seen that in this case it was not perfectly sharp and well-defined. It is important that the observation should be repeated by as many impartial observers as possible, as there is still a difference of opinion as to whether the line is really sharply defined or not. Other lines may also be looked for. Prof. Winlock simply observed that the spectrum consisted of a single line, and no continuous spectrum.

(2, 3, 4) These are the three "bright-line stars" first observed by Wolf and Rayet in 1867, and called by them 1st, 2nd, and 3rd Cygnus, respectively. They were subsequently observed in greater detail by Vogel, who found that many lines were common. It has been shown that most of the lines can be explained by a reference to the low-temperature spectra of manganese, iron, and sodium, in addition to the radiation of hydrogen and carbon. The most striking feature of the spectra is undoubtedly the bright band in the blue, which, standing out beyond the continuous spectrum, gives rise to an apparent absorption band on the less refrangible side. My own observations, and those of Mr. Lockyer, have shown that this band is coincident with the blue band seen in the spectrum of the spirit-lamp flame, and is therefore probably due to carbon. This conclusion is supported by the presence of another carbon fluting, near λ 517, which, however, is not so obviously seen because of the brighter continuous spectrum in that region. These are the brightest bands seen in the spectra of comets at mean distances from the sun, and the similarity leads to the conclusion that the structure of comets and bright-line stars is identical (see NATURE, August 7, p. 344). Bright-line stars are therefore probably swarms of meteorites. It is desirable that the coincidence of the carbon bands with those of the "stars" should be confirmed by other observers.

(5) The spectrum of this star is a well-marked one of Group II., the bands 2–9 being wide and dark. It is probably one of mean condensation, and the bright carbon flutings should therefore be well seen. It will be interesting also to know what dark lines, if any, are present at this stage.

(6) A star of the solar type (Vogel). Is the temperature increasing (Group III.) or decreasing (Group V.)?

(7) A star of Group IV. Observations of the comparative intensities of the hydrogen and other lines are required, in order that the temperature relatively to other stars of the group may be determined.

(8) This is a relatively bright star of Group VI., showing the secondary bands 2, 3, 4, 5, and 8, in addition to the usual carbon bands, which are very wide and dark. As in other stars of the group, further details, especially the presence or absence of solar lines, should be looked for.

(9) This variable of Group VI. will reach a maximum about August 25, the period being about 485 days. The magnitude at maximum is 7·4–8·5, and that at minimum about 11·5. I may repeat that we as yet know nothing of the spectroscopic changes which accompany the increase of light in a star of this kind, and continuous observations will therefore be of the utmost value. It has been suggested by Mr. Lockyer that the chief variation may be the relative paling of the principal bands at maximum. Colour changes should also be noted.

A. FOWLER.

MOSCOW OBSERVATORY.—Prof. Th. Bredichin has issued the second volume of the second series of *Annales de l' Observatoire de Moscou*, and it contains some interesting papers. In one "On the Origin of Periodic Comets," Prof. Bredichin brings forward many important facts. He first points out the similarity of the elements of some comets, and adduces evidence to show that they probably once formed part of a single comet which has become disintegrated by explosions and planetary perturbations; the multiple comets of Biela, Liais (1860), and 1882 II. being quoted as examples of such a development. After deducing the expressions for eruptions which do not take place in the plane of the orbit of the generating comet, and applying them to some examples, the elements of twenty-nine comets having a period less than 100 years is given, and some considerations relative to their perihelion distances, small inclinations, and direct movement are urged, as opposed to the hypothesis of the immediate transformation of parabolic to elliptic orbits. Two tables of 290 non-periodic comets arranged in the order of their perihelion distance (q), and 44 comets which have developed comæ, will be useful, irrespective of the fact that they demonstrate that when the value of q is sensibly greater than 1 the comet has little ability to develop eruptive phenomena and to produce new comets. A division of periodic comets into four groups is made, the periodic time of the respective groups being 73·8, 33·1, 14·1, and 6·0 years, and it is shown that the values of the eruptive action increases as the period decreases. The influence of Jupiter and Saturn on cometary orbits is, of course, considered, and some relations are pointed out between the times of revolution of the above-mentioned groups and those of these two planets. In another long paper "On the Origin of Shooting Stars," Prof. Bredichin attempts to prove that all meteors are ejections from comets. A paper by M. P. Sternberg gives the results of some determiuations of the length of the seconds pendulum made by Prof. Bredichin and himself in 1888–89 in various parts of Russia and Europe; and another paper, by M. Ceraski, "On Luminous Clouds," contains some interesting facts. On June 26, 1885, from observations made at two stations, separated by 10 kilometres, the vertical height of a luminous cloud was found to be nearly 75 kilometres. A map is also given for putting down observations of the paths of Perseid meteors, and this, with the eleven papers, renders the volume for 1890 as good as its predecessors.

LEANDER MCCORMICK OBSERVATORY.—Vol. i., Part 4, of the Publications of the Leander McCormick Observatory of the University of Virginia contains some double-star observations made in 1885–86. The working list from which the double stars were selected contained all known pairs between −30° and 0° having distances less than 4″, and several very close and difficult pairs north of the equator. The observers were Messrs. F. P. Leavenworth and Frank Muller, and the measures appear to have been made with much care.

COGGIA'S AND DENNING'S COMETS (b AND c 1890).—The following ephemerides are given by Dr. Berberich in *Astronomische Nachrichten*, No. 2984:—

Ephemerides for Berlin Midnight.

1890.	Coggia's Comet. R.A.	Decl.	Brightness.	Denning's Comet. R.A.	Decl.	Brightness.
	h. m. s.	° ′		h. m. s.	° ′	
Aug. 21	11 27 54	+16 49·2		15 28 28	+42 1·8	2·09
23	11 33 1	15 23·7	0·27	30 9	38 58·4	
25	11 37 55	14 0·8		31 51	35 54·3	2·17
27	11 42 37	12 40·2	0·23	33 33	32 50·0	
29	11 47 8	11 22·0		35 16	29 46·5	2·21
31	11 51 29	10 6·1	0·20	37 0	26 44·6	
Sept. 2	11 55 41	8 52·3		38 45	23 44·9	2·21
4	11 59 45	7 40·7	0·17	40 31	20 48·4	
6	12 3 41	6 31·1		42 18	17 55·5	2·16
8	12 7 31	5 23·4	0·15	44 5	15 6·8	
10	12 11 14	4 17·6		45 53	12 22·6	2·08
12	12 14 51	3 13·5	0·13	47 42	9 43·5	

The brightness at discovery has been taken as unity.

SEXUAL SELECTION IN SPIDERS.

EVERY student of zoology who, of late years, has attempted to follow the drift of all that has been written on the subject of natural selection cannot fail to have observed that the less important, though not less interesting, hypothesis of sexual selection has received relatively little attention. It must be seen, in fact, that to all intents and purposes the hypothesis has remained in the state in which it was left by Mr. Wallace's criticisms—lately repeated and extended in "Darwinism"—of Mr. Darwin's views. And further, it will probably be admitted that this circumstance is to be attributed, not to the fact that there exists amongst zoologists unanimity of opinion on the point—far from it; but to the fact that, owing to the great practical difficulties in the way of making fresh observations on the courtship of new groups of animals, there has been little or nothing to add to what has been already said. Consequently any contribution to the subject should be gladly welcomed; and no apology is needed for drawing attention to a paper on the "Sexual Selection of Spiders," which was published last year in the United States of America.

Moreover, since the paper in question is one of the occasional papers of the Natural History Society of Wisconsin, it is not likely to obtain a wide circulation, at all events on this side of the Atlantic, and to meet with that attention which all who read it must admit that it deserves. The author, Mr. G. W. Peckham, whose name has been for some years past well known to those who have devoted their time to the study of spiders, has in the present instance produced a work of much greater general interest than any that he has published before. For the series of observations which constitute a large part of its subject-matter affords a means of testing in practically an unworked order of animals—and one especially favourable for the purpose—the two hypotheses respecting sexual ornamentation formulated by Mr. Darwin and Mr. Wallace. And since the conclusions to be derived from these observations are, in the opinion of the author, all in favour of Mr. Darwin's views, it will be of interest to see from a critical examination of the contents of the paper to what extent this opinion is supported by the facts therein set forth.

Mr. Darwin's theory of sexual selection, or in other words the theory that the brilliant colours and ornaments of the male are due to the constant preference by the female of the best-decked males, is too well known to need further explanation here. Mr. Wallace, rejecting this theory on the grounds that there is but little evidence in its favour and much that is directly opposed to it, has put forward an alternative hypothesis which may be briefly epitomized as follows. The production of colour in organisms is normal, and needs no special accounting for; the more brilliant colouring of the male, the development of plumes, &c., is attributable to the greater vigour and activity of this sex, and when this colouring becomes intensified at the breeding season it is because vitality is then at a maximum; the duller colouring of the female, at all events in birds, is due, through the agency of natural selection, to the toning down or elimination of the normal tints on account of her special need for concealment.

Before proceeding to test these two theories in the light thrown upon the subject by a consideration of the secondary sexual characters of spiders, Mr. Peckham, turning his attention to other groups, urges the following cases as inexplicable by Mr. Wallace's views. If, it is asked, there is a causal relation between high vitality and ornamentation, how are we to account for the brilliant colours of some tropical pigeons which are remarkable neither for pugnacity nor activity? and how for the gaudy plumes of the birds of Paradise, which are by no means noticeable for fierceness of disposition? But in the case of these two objections the flaw seems to be the assumption that pugnacity and activity are the only criteria of high vitality.

It is clear, moreover, that at the time these were advanced the author had never read Mr. Wallace's last work, "Darwinism"; for on p. 292 of this volume reference is made to the birds of Paradise, and their gaudy adornment is spoken of in connection with the wonderful activity they display. The following question is also raised. Quoting *verbatim*—"Perhaps the most difficult fact to reconcile with the [Mr. Wallace's] theory is the absence of ornamentation and bright colour in the bats. They have a wide expanse of integument, and great activity, the conditions specified by Mr. Wallace for the development of gaudy pigment, and nothing, apparently, in their habits to keep it down; but, except in the frugivorous bats, we find little difference between the sexes, nor is there any appreciable approach to bright colours" (p. 9).

At first sight this objection seems to be valid; for, if we admit the truth of the author's premises, it seems that his conclusion is a just one. But can this be done? Is it the case that there is nothing in the habits of bats to keep down the development of gaudy pigment? Surely not. These animals are nocturnal or crepuscular, spending the day suspended from the roof of some cave or other dark secluded spot, and only issuing forth at nightfall to exercise their activity in hunting for prey. Thus that great expanse of integument above referred to—namely, the wings—is never exposed to the rays of the sun, and few things, if any, are more antagonistic to the development of gaudy pigment than absence of light.

These cases, then, do not seem upon examination to be seriously opposed to Mr. Wallace's views. But turning to the spiders, we find that the objections are of a somewhat different order from those hitherto considered. For after passing in review all the principal families of this group, and studying their moults, it is concluded (1) that there is no evidence to show that there is a causal relation between high vitality and adornment, since, in the first place, as a rule, the savage and powerful female is less, sometimes very much less, brightly coloured than the male, who is comparatively weak and unaggressive; and, in the second place, many of the sluggish and sedentary spiders, such as, *e.g.*, the *Epeiridæ*, are brilliantly tinted, whilst other active, restless forms, such as most *Lycosidæ*, are relatively dull-coloured; (2) that when the male differs from the female he departs in proportion from the normal colouring of the group, and that when the female, as well as the male, is showily attired, the resemblance between them is due to the partial or complete transmission of the male colouring to the female, the completeness of the resemblance depending upon the age of the male at the time of the acquisition of his adornments; and (3) that there is no reason to think that the females have had their colours toned down for the sake of concealment at the time of nesting, for in the *Attidæ*, where sexual differences of colour are best marked, the females have covered nests.

Mr. Wallace's theory, then, however satisfactory it may be in the case of butterflies and birds, fails apparently in every essential respect when applied to spiders. It is necessary, therefore, to look elsewhere for an explanation.

Now, when we consider the secondary sexual characters of the *Attidæ*, the first fact that strikes the attention is that these characters exist in the males as modifications in the form of the falces, palpi, first pair of legs, or clypeus—that is, of those portions of the anterior part of the body which are in full view when the male is approaching the female—and, moreover, that these modifications in form result often in an increase of surface for the development of gaudy, often strongly iridescent, hues.

Thus, in *Salticus formicarius* the ♀ has short vertical reddish-black falces, while those of the ♂ are horizontal, much enlarged, and copper-green in colour; and in *Icius palmarum* "the falces in the ♂ are compressed, horizontal, and three times as long as the face, the fang equalling the falx in length; the front surface of the falces is dark bronzy rufus, and on each outer edge is a wide band of snowy-white hairs. In the ♀ the falces are vertical, and only as long as the face, and the snowy-white hairs are absent. The ♂ is rendered still more striking by the long snowy-white hairs which cover his clypeus, while the forehead, and a space just below the first row of eyes, is covered with bright red hairs. All this ornamentation is lacking in the ♀."

The clypeus, too, is liable to a considerable amount of variation with sex. Thus, in *Dendryphantes capitatus* this part in the ♂ is conspicuously marked by several white bands, which contrast strikingly with the dark colour of the rest of the face; in the ♀ the whole clypeus is whitish, and in no way conspicuous. Again, in *Mopsus mormon* there is a high vertical ridge of hairs extending over the forehead in the ♂; in the ♀, on the other hand, these hairs are wholly absent. So, too, in the *Theridiidæ*, the heads of the males are frequently higher, in many species very much higher, than in the females.

With regard to the palpi and first pair of legs, it will be sufficient to say that in Keyserling's "Arachniden Australiens" thirty-four males are described (in the family *Attidæ*), having well-developed fringes or tufts of hair on the palpi, while there are only five females so ornamented, and several of these to only a moderate extent; and that in the ♂ of *Synageles picata* the tibia of the first pair of legs is enlarged and flattened, and the anterior face of the enlargement is of a brilliant steel-blue colour; in the ♂ of *Philæus metallescens* the anterior legs are elongate, of a brilliant steel-blue colour, and ornamented with rings, spots, and fringes of hairs, whilst in the females of these

two species the legs are neither modified nor adorned to any remarkable extent.

It must be understood that the few instances here given of the secondary sexual characters have been selected out of a number cited by the author, who could himself have filled a volume on the subject. Sufficient, however, have been given to show how commonly the anterior portion of the body varies in different ways in the male sex; and "it is of high importance to note that the bright-coloured hairs or metallic scales as well as the protuberances are either on the anterior surface, or in some way so placed as to be plainly in view from the front."

In seeking for an explanation of these sexual characters it does not appear that any of them are of special advantage to their possessors in the way of procuring food, avoiding enemies, fighting with rivals, &c.; consequently they cannot be attributed to the action of natural selection. But when considered in connection with the habits of the *Attidæ* at the time of mating, it is clear to the mind of the author that the clue is to be found in the theory of sexual selection. The courtship of a number of captive species is described, and described with a sense of the ludicrous which is quite irresistible. The following two may be selected as instances.

On p. 37 we read of *Saitis pulex*:—"On May 24, we found a mature female, and placed her in one of the larger boxes, and the next day we put a male in with her. He saw her as she stood perfectly still, twelve inches away; the glance seemed to excite him, and he at once moved towards her; when some four inches from her he stood still, and then began the most remarkable performances that an amorous male could offer to an admiring female. She eyed him eagerly, changing her position from time to time so that he might be always in view. He, raising his whole body on one side by straightening out the legs, and lowering it on the other by folding the first two pairs of legs up and under, leaned so far over as to be in danger of losing his balance, which he only maintained by sidling rapidly towards the lowered side. The palpus, too, on this side was turned back to correspond to the direction of the legs nearest it. He moved in a semicircle for about two inches, and then instantly reversed the position of the legs and circled in the opposite direction, gradually approaching nearer and nearer to the female. Now she dashes towards him, while he, raising his first pair of legs, extends them upward and forward as if to hold her off, but withal slowly retreats. Again and again he circles from side to side, she gazing towards him in a softer mood, evidently admiring the grace of his antics. This is repeated until we have counted 111 circles made by the ardent little male. Now he approaches nearer and nearer, and when almost within reach whirls madly around and around her, she joining and whirling with him in a giddy maze. Again he falls back and resumes his semicircular motions, with his body tilted over; she, all excitement, lowers her head and raises her body so that it is almost vertical; both draw nearer; she moves slowly under him, he crawling over her head, and the mating is accomplished."

Again, on p. 47, concerning *Dendryphantes elegans*:—"While from three to five inches distant from her, he begins to wave his plumy first legs in a way that reminds one of a windmill. She eyes him fiercely, and, he keeps at a proper distance for a long time. If he comes close she dashes at him, and he quickly retreats. Sometimes he becomes bolder, and when within an inch, pauses, with the first legs outstretched before him, not raised as is common in other species; the palpi also are held stiffly out in front with the points together. Again she drives him off, and so the play continues. Now the male grows excited as he approaches her, and while still several inches away, whirls completely around and around; pausing, he runs closer and begins to make his abdomen quiver as he stands on tip-toe in front of her. Prancing from side to side, he grows bolder and bolder, while she seems less fierce, and yielding to the excitement, lifts up her magnificently iridescent abdomen, holding it at one time vertical, and at another sideways to him. She no longer rushes at him, but retreats a little as he approaches. At last he comes close to her, lying flat, with his first legs stretched out and quivering. With the tips of his front legs he gently pats her; this seems to arouse the old demon of resistance, and she drives him back. Again and again he pats her with a caressing movement, gradually creeping nearer and nearer, which she now permits without resistance, until he crawls over her head to her abdomen, far enough to reach the epigynum with his palpus."

From these cases and the others that are given it is established that the attitudes and antics of the males are such as to display to the best advantage whatever adornments they possess, and it is concluded that the female selects as her mate the male which pleases her best on account of some superiority over his fellows in adornment either of colour, or special outgrowths, or both. Hence is deduced the further conclusion that the sexual ornaments of the male result from the constant preference by the females of the best-decorated males. But it is obvious that this conclusion is open to the same criticism as that advanced against Mr. Darwin's explanation of the sexual ornamentation of, *e.g.*, birds—namely, the criticism that the conclusion rests upon an inference and not upon a fact; and that the most important link in the whole chain of evidence is wanting—namely, the proof that the females select as partners the most beautiful males. This it will be remembered, is perhaps Mr. Wallace's strongest objection to the hypothesis of sexual selection; and when we consider all the cases that are quoted in this work it will be seen that many of those that are advanced as supporting this hypothesis are equally explicable by Mr. Wallace's views. Thus, although it is certainly the case that the females are as a rule the more powerful and more ferocious, yet, on the other hand, judging from the descriptions given of the contests and dances of the males, it seems to be this sex which excels in activity; and if activity be a criterion of high vitality we at once see the connection between high vitality and ornamentation. Again, from the fact that the female watches with attention the antics and gambols of the male, it is inferred that she is admiring the display of his agility and beauty: that, of course, may be the case, but is it not conceivable, considering the ferocity of her disposition, that she is merely on the alert to ward off an unwelcome advance, or is but awaiting a favourable opportunity to seize and destroy her persecutor? Or again, if it be asked why it is that the males perform the strange antics in the presence of the females if it be not for display, it may be answered that the excitement of the males, always great during the breeding season, attains to a maximum at that time in the society of the females, and shows itself in the performance of the strange antics that are so graphically described. The same objection may be made to the idea expressed on p. 41 that the seemingly terrible battles of the males are all sham affairs gotten up for the purpose of displaying before the females. The fact that the males fight when there is no female to watch them makes it more probable that the combats are due to playful excitability or genuine ferocity. And lastly, not only, as above stated, is there a lack of evidence to show that the female prefers as partner the most beautiful male; but, more than that, it appears that the success of one male over another in courtship may be attributed to excess of vigour. Thus, in the case of *Astia vittata*, a species in which there are two types of male, it is stated on p. 54 that "the *niger* form is much the more lively of the two, and whenever the two varieties were seen to compete for a female, the black one was successful. He is bolder in his manners . . ."[1]

Thus Mr. Peckham has not yet brought forward a sufficient number of facts to carry to all minds that conviction of the truth of the theory of sexual selection which he feels himself. But whatever be the value of the criticisms here advanced, and of others that will doubtless be thought of, everyone will admit that the paper contributes a number of new and interesting facts to the subject of sexual adornment, and most of its readers will probably feel inclined to think that the balance of the evidence, so far as spiders are concerned, tends to support the explanation proposed by Mr. Darwin. R. I. POCOCK.

THE TERMINOLOGY OF HYDROLYSIS, ESPECIALLY AS EFFECTED BY "FERMENTS."

ALL who consider the meaning of words, and who desire, as far as possible, to remove ambiguity from the terms employed in denoting chemical change, must have felt some dissatisfaction with the nomenclature used, chiefly by physiologists, in describing and discussing the remarkable phenomena

[1] In connection with this passage it is necessary to explain that at the time it was written I was not aware that Mr. Poulton, in his new work on "The Colours of Animals," cites the case of *Astia vittata* as affording the strongest support to the theory of sexual selection; nor did I see Mr. Wallace's review of this work in NATURE of July 24 (p. 289) until after the present article had been sent to the publisher. Consequently I had no means of knowing what Mr. Wallace's opinion on the point might be—except in so far as his reply to Mr. Poulton is the only one that common sense would immediately suggest to any man who holds Mr. Wallace's views on sexual ornamentation, or who criticizes the subject without prejudging it.—R. I. P.

presented by the living cell and which attend digestion. As the authors of a paper "On the Germination of the Gramineæ" (Chemical Society's Transactions, 1890, 458) have done me the honour to accept several of my suggestions, I venture to regard the opportunity as one which should not be lost of discussing the terminology of fermentation phenomena.

Changes such as a glucose undergoes when it is resolved into carbon dioxide and ethyl alcohol take place under the influence of the living organism, and there is every reason to believe only *within* the cell: they involve the formation of products containing in the gross neither more nor less than the original elements of the compound fermented; and when the products are compared with the compound from which they are derived, it is seen that their production in all cases involves the separation of carbon atoms which were directly united, and also considerable rearrangement of the constituent elements.

Changes such as that which cane-sugar undergoes on inversion take place not only within the cell but equally well without it under the influence of a substance which, although not living, is of vital origin: they appear always to involve the fixation of the elements of water; and the products of their action bear a very simple relation to the original substance, viz. always that of an alcohol to its ether, no separation of directly-united carbon-atoms, or any molecular rearrangement such as attends the former class of actions, taking place. The agents derived from organisms which effect changes of this second kind are spoken of as *unorganized ferments;* changes of the first kind are said to be conditioned by *organized ferments, i.e. organisms.*

There is thus, at the outset, a difficulty in assigning a consistent meaning to either term—fermentation or ferment; diverse phenomena produced by diverse agents being included under a single designation.

Fermentation.—The difficulty is in part met by restricting the term fermentation to changes such as occur, for example, during alcoholic fermentation; and there would seem to be no occasion to apply it more widely so as to include changes of the second kind, these, as before remarked, being apparently all cases of *simple hydrolysis.* This is true, even if the explanation of fermentation suggested by Baeyer in 1870 (cf. *Ber.*, 1870, 63; Journal of the Chemical Society, 1871, 331) be accepted, which represents fermentation proper as the outcome of hydrolysis—an explanation which the increase of knowledge in the interval entirely favours; inasmuch as hydrolysis takes place in the two cases with different results, and affects compounds of different types. Moreover, it is to be noted that not only is it impossible to represent the phenomena of fermentation proper as the outcome of simple hydrolysis, but that also, in certain cases, synthetic as well as analytic changes occur; in the case of butyric fermentation, for example. In fact, in many instances, apparently two series of concurrent changes take place: the one series involving what, in the light of Baeyer's explanation, may be termed *recurrent* as distinguished from *simple* hydrolysis; the other involving the interaction of the molecules of one or more products of this recurrent hydrolysis. There is thus an advantage in employing a somewhat empirical expression in connoting phenomena which do not all conform to one absolute type, but which have a common origin, as they are the outcome of protoplasmic activity, especially as most fermentations are attended with evolution of gas.

But it is to be remembered that, whereas carbohydrates and allied compounds such as glycerol, lactic acid, &c., are said to undergo *fermentation*, the decomposition of albuminoids under the influence of organisms is commonly termed *putrefaction;* this distinction, however, is made on account of the character of the products, not because there is any reason to suppose that the changes which occur are essentially different in character from those which the carbohydrates undergo. The want of a term indicative of the fact that the action is one which takes place under vital influence without reference to the character of the change—equally applicable to simplifications such as occur during alcoholic fermentation and to complications such as occur during butyric fermentation—is also felt in the case of changes such as alcohol undergoes under the influence of *Mycoderma aceti* and *vini*, or which ammonia undergoes on nitrification, and in the converse change of denitrification. Dr. W. Roberts has proposed to speak of changes induced by enzymes (*v. infra*) as cases of *enzymosis;* the corresponding term *zymosis* might well be employed as the synonym of fermentation, and would probably be found to be of more general application: thus alcohol may be said to undergo oxidation by zymosis or zymic oxidation under the influence of *Mycoderma aceti;* and in discussing putrefactive changes, it would be possible to speak of zymic changes as distinct from those arising from the unassisted interaction of the zymic products. Zymosis is preferable to *zymolysis*, as the effect is not always one of simplification.

Ferments.—The expression *ferment* is more frequently than not employed as the equivalent of *unorganized ferment;* consequently it is applied to the very agents which are incapable of producing fermentation proper. This has been so generally felt to be the case that several words have been coined in place of unorganized ferment, notably *zymase* and *enzyme* (cf. Dr. W. Roberts, Roy. Soc. Proc., 1881, xxxi. 145): the objection may be made to the former that it is indicative of vitality; the latter, however, is expressive, and serves only to indicate the vital origin of the agent, thus differentiating it from agents such as the mineral acids which act very similarly.

Enzymic action or enzymosis[1]—to use the phrase suggested by Dr. Roberts—appears, as already remarked, always to involve decomposition by means of water. On this account, in 1880, in the second edition of my "Introduction to the Study of Organic Chemistry" (Longmans; footnote, p. 190) I put forward the following suggestions:—

"Decompositions like those of starch into dextrose, of cane-sugar into dextrose and lævulose, of the fats into glycerine and an acid, or of ordinary ether into ethylic alcohol, which involve the fixation of the elements of water, may all be said to be the result of *hydrolysis;* and those substances which, like sulphuric acid, diastase, emulsin, &c., induce hydrolysis may be termed hydrolytic agents or *hydrolysts.* The substance hydrolyzed is the *hydrolyte.* The mere fixation of the elements of water, unaccompanied by decomposition, as in the conversion of ethylenic oxide into glycol, $C_2H_4O + OH_2 = C_2H_4(OH)_2$, may be termed hydration in contradistinction."

It is usually necessary to employ a specific enzyme (hydrolyst), or one of a very limited number, to effect the hydrolysis of any particular hydrolyte, and hence physiologists are in the habit of speaking of *amylolytic* ferments, *proteolytic* ferments, &c., meaning ferments capable of *splitting up* starch, proteids, &c. But the terms amylolytic, proteolytic, &c., are confusing to the student who has learnt that electrolysis signifies splitting up *by means of* electricity, and hydrolysis splitting up *by means of* water—not the splitting up *of* electricity or *of* water. As electrolysis and even hydrolysis are well-established terms which it would scarcely be politic to alter, it appears highly desirable to abandon the use of terms such as amylolytic, proteolytic, &c., and I would suggest that an enzyme capable of inducing the hydrolysis of starch should be termed an *amylo-hydrolyst;* one which affects albuminoids, a *proteid-hydrolyst;* one which affects fats, a *glyceride-hydrolyst.*

One case remains in which the use of the term ferment cannot be avoided by the adoption of this proposition—that of the so-called rennet ferment. It may well be that this is also a hydrolyst, and that in all cases the formation of a curd, clot, or coagulum initially involves hydrolysis—or, perhaps, hydration merely—and the consequent interaction of molecules of the product or products; but of this we know nothing at present, and the observed phenomena are of so different a character that it is desirable to connote such changes by a distinct expression. I would suggest that we should term the rennets *thrombogenic enzymes* or *thrombogens.*

HENRY E. ARMSTRONG.

SCIENTIFIC SERIALS.

L'Anthropologie, sous la direction de MM. Cartailhac, Hamy, et Topinard, tome i., No. 2 (Paris, 1890).—The covered mortuary-chambers at Les Mureaux (Seine-et-Oise), by Dr. Verneau. Through the accidental displacement of the soil in a field at Les Mureaux an important discovery was made in the winter of 1888–89 of a subterranean sepulchral passage, a so-called *allée couverte*, which was densely packed with human bones, intermingled with various stone, bone, and other objects. As in other constructions of the kind, this mortuary passage was divided into two paved and walled-in chambers, varying in width from 1·85 m. to 2·10 m. Owing to the thickness of the stone walls dividing these chambers, considerable labour must have been required to effect an entrance whenever a fresh burial took place. It would appear that sixty or more skeletons had been deposited in these chambers, but unfortunately many

[1] Sheridan Lea's zymolysis (cf. *Journal of Physiology*, 1890, xi. 254) is open to the objection above made to zymase.

of the bones were damaged or utterly destroyed by the workmen originally engaged in the excavations. In the course of M. Verneau's examination of these sepulchral chambers, he discovered that a special burying-place had been allotted to children close to the stone hearth, which was placed at the entrance of the *allée*, while the adult skeletons had been deposited in other parts of the chambers. He is of opinion that the *foyer* or hearth was designed to facilitate the ventilation of the air before the chamber was opened for the deposition of fresh bodies, but the presence in the surrounding ashes of half-burnt animal bones suggests the possibility that these rudely constructed hearths may have been used for the preparation of funeral repasts. The great number of artificially perforated cranial bones proves that the process of trepanning was of frequent occurrence amongst the Neolithic tribes of Les Mureaux. The great variety of cranial types, which range from the extremes of brachycephalism to those of dolichocephalism, shows that a blending of several distinct races had taken place prior to the settlement of these early people. The objects found at Les Mureaux, moreover, indicate that these tribes must have had communication with distant regions, for while the stones of which the *allée* is built have been obtained from the opposite shores of the River Seine, some of the shells, as patella, purpura, and others used for ornamentation, must have been derived from intercourse with people of the remote sea-coast. Perforated flint and bone pendants were found on several of the skeletons, as many as five of these objects being suspended around the neck of a very young child.—On the dietary of the Lapps, by M. Rabot. The author has borrowed so largely from the narratives of earlier foreign travellers, and more especially from those of Dr. Broch and other Danish writers, that his work can lay no claim to originality. According to the author it would seem that we are justified in assuming that the sedentary Lapps and most of those who have entirely given up a nomadic life live almost exclusively on fish, while the pastoral section of the people prefer animal food.—The cephalic index in the population of France, by Dr. Collignon. The author here shows how we may trace in the distribution of various cephalic indices the main centres of the different races which have occupied the French territories. Among the various peoples settled in France he distinguishes three groups—namely, (1) a Ligurian or Iberian people, the representatives of the Cro Magnon and other primitive tribes, who exhibited the dolichocephalic type, with a short stature and a brunette coloration. This race appears to have spread from the Gulf of Lyons to the Maritime Alps. (2) A Celtic brachycephalic race, which predominated in the districts extending from the Mediterranan to the eastern limits of France. (3) A blonde dolichocephalic people, who had forced themselves wedge-like through the Celtic mass of the population, separating it into two sections, and advancing from north-east to south-west.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 11.—M. Duchartre in the chair.—On the equilibrium and mutual reaction of the volatile alkalies, by M. Berthelot. The author has considered the cases of the mutual reactions of water, hydrochloric and sulphuric acids on piperidine, and has determined the various heat changes which result. He has also investigated the amount of heat developed in the action of ammonia and the fixed alkaline bases on the same compound. Pyridine and aniline have been similarly experimented upon.—On the meteoric iron of Magura, Arva (Hungary), by MM. Berthelot and Friedel. An examination of two samples of this meteorite has led the authors to the conclusion that the numerous small crystals are quartz, and not diamonds as has been supposed.—On an electric lamp called the Stella lamp, destined for use in mines, by M. de Garson.—On some new hydrates of gases, by M. Villard. The preparation of the hydrates of propane and of the fluorides of carbon is described.—On a new fatty acid, by M. E. Gerard. The new acid is intermediate between palmitic and stearic acids, and presents analogous properties; nevertheless its melting-point is notably lower than that of the more fusible of these two homologues. The author proposes to call it daturic acid.—Researches on the purple produced by *Purpura lapillus*, by M. Augustin Letellier.—On the multiplication and fertilization of *Hydatina senta* (Ehr.), by M. Maupas. This communication completes the work recorded in a former paper (*Comptes rendus*, cix., 1889) on the following infusorians: *Cycloglæna lupus* (Ehr.), *Notammata* species (?), and *Adineta vaga* (Davis), and consists of observations of two specimens of *Hydatina senta*.—On a peculiarity in structure of aquatic plants, by M. C. Sauvageau.—On the reputed digestive power of the liquid in the covered capsule of Nepenthes, by M. Raphael Dubois. The author has come to the following conclusions: (1) that this liquid contains no digestive juice comparable to pepsin, and that the Nepenthes are not carnivorous plants; (2) that the phenomena of disintegration or false digestion observed by Hooker were without any doubt due to the activity of external micro-organisms and not to the secretion of the plant.—Anatomical researches on hybrids, by M. Marcel Brandza. It has been found that (1) certain hybrids present in their structure a juxtaposition of particular characters such as are found in both parents; (2) in other cases the structure of different parts of the hybrid is, for every tissue, simply intermediate between that of the parents; (3) other hybrids have in certain organs a structure intermediate between that of the tissues of both parents, whilst in other organs a juxtaposition of anatomical characters peculiar to the parents is observed.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Swanage: its History, Resources as an Invigorating Health Resort, Botany, and Geology: edited by J. Braye (Everett).—Hand-book of Cyclonic Storms in the Bay of Bengal: J. Eliot (Calcutta).—Cyclone Memoirs, Part 2:—Bay of Bengal Cyclone of August 21–28, 1888 (Calcutta).—Waterways and Water Transport in Different Countries: J. S. Jeans (Spon).—The Protoplast: E. C. C. Baillie (Nisbet).—Smithsonian Report, 1886, Part 2, and 1887, Part 2 (Washington).—Obs. Meteorolójicas hechas en el Observatorio Astronómico de Santiago, 1882–84, 1885–87 (Santiago de Chile).

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THURSDAY, AUGUST 28, 1890.

THEORETICAL BALLISTICS.

A Revised Account of the Experiments made with the Bashforth Chronograph, to find the Resistance of the Air to the Motion of Projectiles, &c. By Francis Bashforth, B.D., late Professor of Applied Mathematics to the Advanced Class of R.A. Officers, Woolwich, and formerly Fellow of St. John's College, Cambridge. (Cambridge: University Press, 1890.)

ROBINS, in the last century, revolutionized the science of artillery by his invention of the ballistic pendulum; and in our own times Mr. Bashforth has accomplished the same thing for modern rifled artillery, by the aid of electricity and by his own chronograph.

Previous to Robins's experiments, the vaguest ideas prevailed as to the velocity of cannon shot and musket bullets: it was never supposed that such a light medium as the air could offer the enormous resistance it does; and the resistance of the air being supposed almost insensible, and Galileo's parabolic theory being applied, the velocity of projectiles was very much underestimated. At the same time, to reconcile Galileo's theory with the observed ranges in practice, it was usual to suppose the first part of the trajectory to be a finite straight line, the *point-blank range*, and to add the parabola at the end of the straight line.

The ballistic pendulum of Robins enables us to dilute the velocity of the bullet so as to make it easily measurable; and by firing at the pendulum from different distances, and calculating the loss of velocity through the air, we are able to obtain a fair estimate of the resistance. Robins found in this manner that the resistance of the air to a bullet, three-quarters of an inch in diameter, weighing one-twelfth of a pound, is about 10 pounds, or 120 times the weight of the bullet at a velocity of about 1600 feet per second. By firing with a charge of powder half the weight of the ball at the ballistic pendulum at ranges of 25, 75, and 125 feet, he found that the mean velocities of impact were respectively 1670, 1550, and 1425 f.s.

Now denoting by R the average resistance in pounds over the first 50 feet, in which the velocity fell from 1670 to 1550, the principle of energy gives, in foot-pounds,

$$50R = \frac{1670^2 - 1550^2}{2 \times 32 \cdot 2 \times 12}, \text{ or } R = 10.$$

Robins proceeds to theorize by the principle of mechanical similitude, and shows that a 24-pound cannon-ball fired with a charge of 16 pounds of powder, should acquire a velocity of 1650 f.s., and that the resistance of the air would then amount to 540 pounds, or nearly twenty-three times the weight of the shot. He is now able to clear up the difficulty of the supposed point-blank range, the distance during which the shot is conceived to fly in a straight line. To reconcile the parabolic theory of Galileo with the observed very small curvature of the trajectory at the outset, ancient writers on ballistics were in the habit of making a concession to the vulgar opinion (an opinion not yet extinct, although Tartaglia pointed out its fallacy) that the path of a shot was a straight line for a certain distance, called the point-blank range, during which the shot "flyeth violently," the *motus violentus* of old writers.

But now Robins is able to show that, in consequence of the much higher velocity of the shot, and the much greater resistance of the air than was ever considered, a 24-pound shot fired with two-thirds of its weight of powder, will, at a distance of 500 yards from the piece, be separated from the line of its original direction by an angle of little more than half a degree, so small an aberration as not to be noticeable with crude artillery appliances; and generally that the track of the shot departs greatly from the parabola, and is much more closely imitated by the combination of *motus violentus* in a straight line, *motus mixtus* in a curve or circular arc, and *motus naturalis* in a vertical line, the vertical asymptote of the true path, as taught by the old writers on artillery.

The treatise of Robins, "New Principles of Gunnery," 1742, attracted immediate attention, and was translated with a commentary by Euler.

The ballistic pendulum employed by Robins weighed about 56 pounds, and was used only with musket bullets; and to this day it will probably be found the most efficient instrument for measuring the velocity and retardation of small-arm projectiles; the threads or wires of the electric screens being easily missed by bullets, or, if struck, being apt to deflect them.

Experiments were made at Woolwich by Hutton in 1775 and by Gregory in 1815, and by Piobert, at Metz, in 1839, to apply the ballistic pendulum to cannon-balls; and although not such an accurate instrument on a large scale, in consequence of elasticity and vibration, still it was the only means at hand till the invention of the electric telegraph. The application of electricity to the measurement of the time of flight of the cannon-ball immediately suggested itself to various minds—Wheatstone, Konstantinoff, and Bréguet—and a chronograph was soon produced, capable of registering two instants of time, and thence one velocity; as performed at present by the Boulengé chronograph, now in universal use for the determination of muzzle velocities and the proof of powder.

Notwithstanding the obvious advantages of electricity so late as 1855 a monster ballistic pendulum was constructed to the order of the Government, and first set up at Shoeburyness, then at Woolwich, and finally dismantled without ever having been used in any course of experiments. The model alone of this instrument, shown at the Exhibition of 1862, is reported to have cost £800; but for all practical purposes the pendulum could have been replaced by a large box rammed with sand, and suspended by chains about 6 or 8 feet long, and the indications would probably have been more accurate. The experimenters who followed Robins would have succeeded better if they had expended all their care and ingenuity upon experiments on a small scale; and really with all their trouble it is found that, when checked by electric records, their results are not so accurate as the original observations carried out by Robins.

The problem of the electric chronograph was occupying Mr. Bashforth's mind when he received the appointment of Professor of Mathematics to the newly instituted Advanced Class of Artillery Officers in 1864; where he was well placed for carrying out his experimental ideas, with the assistance of his enthusiastic pupils.

The conditions to be secured which Mr. Bashforth set before himself were—

(1) The time to be measured by a clock going uniformly.

(2) The instrument to be capable of measuring the times occupied by a cannon-ball in passing over at least nine successive equal spaces.

(3) The instrument to be capable of measuring the longest known time of flight of a shot or shell.

(4) Every beat of the clock to be recorded by the interruption of the same galvanic current, and under precisely the same conditions.

(5) The time of passing each screen to be recorded by the momentary interruption of a second galvanic current, and under precisely the same conditions.

(6) Provision to be made for keeping the strings or wires of the screens in a uniform state of tension, notwithstanding the force of the wind and the blast accompanying the ball.

To secure these conditions practically, Mr. Bashforth had to invent his own chronograph, for a detailed description of which the reader must refer to the book; but it consists essentially of a brass cylinder provided with a heavy fly-wheel movable about a vertical axis; and of two markers tracing spiral lines on paper placed on the cylinder, and giving an indication by a jerk on the spiral corresponding to the cutting of one of the electric screens by the shot, or to half-seconds of the clock.

The fly-wheel being spun by hand, and the clock making continual half-second records, the word is given to fire the gun, and then the screen records are registered on the paper by the screen-marker. When the paper is full, after five or six rounds, the cylinder is transferred to a micrometer instrument, and the records read off with a vernier and microscope as accurately as possible.

We may take it that the average travel of the paper on the cylinder is an inch for about a tenth of a second, so that, with screens 150 feet apart, an average velocity of 1500 f.s. would give screen records at intervals of about an inch. Readings of tenths of an inch will give hundredths of a second, and of hundredths of an inch will give thousandths of a second, which is about as far as can be seen or measured with this instrument. But, by treating the last significant figure as indeterminate, and smoothing down irregularities by differencing and interpolation, Mr. Bashforth is able to assign probable values to the 4th and even 5th decimal of the second, in the instant at which any screen is cut.

Any improved instrument which would give a velocity to the paper of ten times or one hundred times of Mr. Bashforth's velocity would increase the recording accuracy theoretically to the same extent; but, as Mr. Bashforth claims for his instrument, he has located the shot at any instant to within about one foot of range, an error comparable with inaccuracies in the measured distance between the screens, inclination of the screens, and bending or stretching of the screen wires before breaking.

The chronograph having given us the instants of time, say $t_1, t_2, t_3, \ldots$ at which screens 1, 2, 3,... at equal intervals of l feet were cut by a shot, we have to employ the methods of Finite Differences for converting these records into expressions for the velocity and retardation at any point.

It will be noticed that the chronograph records give, by interpolation, t as a function of s, not s as a function of t, so that the velocity v is the reciprocal of dt/ds, while the retardation is $\frac{d^2t}{ds^2}v^3$; and if the shot weighs W pounds, the resistance of the air is $W\frac{d^2t}{ds^2}v^3$ poundals, or $W\frac{d^2t}{ds^2}v^3 \div g$ pounds: the shot flying so fast that, practically, we may take it as moving in a horizontal line.

Formulas of the calculus of Finite Differences will give us the values of dt/ds and d^2t/ds^2 in terms of the successive differences $\Delta t, \Delta^2 t, \ldots$ of t; those employed by Mr. Bashforth being—

$$l\frac{dt}{ds} = \Delta t - \tfrac{1}{2}\Delta^2 t + \tfrac{1}{3}\Delta^3 t \ldots\ldots$$

$$l^2\frac{d^2t}{ds^2} = \Delta^2 t - \Delta^3 t + \tfrac{11}{12}\Delta^4 t \ldots\ldots$$

To take a simple numerical illustration, suppose it was found by the chronograph that a shot weighing 70 pounds, flying horizontally, cut three equidistant screens 150 feet apart at instants of time 2·3439, 2·4325, 2·5221 seconds. The time from the first to the third screen being 0·1782 second, the average velocity over this 300 feet is $300 \div 0\cdot1782 = 1684$ f.s.; and we may take this as being the velocity at the middle screen—an assumption which is accurately true if the resistance varies as the cube of the velocity—that is, if d^2t/ds^2 is constant.

Again, $\Delta^2 t = 2\cdot3439 - 2 \times 2\cdot4325 + 2\cdot5221 = 0\cdot001$; so that $d^2t/ds^2 = 0\cdot001 \div (150)^2$; and therefore the resistance of the air is $70 \times (1684)^3 \times 0\cdot001 \div (150)^2 = 14{,}850$ poundals, or 464 pounds.

Experiment confirmed the reasonable hypothesis that the resistance of the air is proportional to the cross-section, or to d^2, if d is the diameter in inches; so that, for similar projectiles, Bashforth introduces his coefficient K, defined so as to make the resistance of the air at a velocity v f.s. to a projectile d inches in diameter to be—

$$d^2 K\left(\frac{v}{1000}\right)^3 \text{ poundals, or } d^2\frac{K}{g}\left(\frac{v}{1000}\right)^3 \text{ pounds};$$

while, if the weight of the shot is W pounds, the retardation due to the resistance is

$$\frac{d^2}{W}K\left(\frac{v}{1000}\right)^3 \text{ celoes};$$

and thus

$$\frac{d^2}{W}K = 10^9\frac{d^2t}{ds^2}, \text{ or } K = \frac{W}{d^2}10^9\frac{d^2t}{ds^2}.$$

The coefficient K is now found experimentally to be the same for all similar projectiles, whatever the weight, W pounds, or diameter, d inches; and the factor of mechanical similitude W/d^2, now called the ballistic coefficient and generally denoted by C, enables us to generalize the experiments made on one scale to projectiles of all sizes.

We now see the convenience of splitting up the resistance of the air into two factors, one of them being the cube of the velocity; for in the retardation the other factor is d^2t/ds^2, which is given very simply in terms of $\Delta^2 t, \ldots$

It is very often asserted that "Bashforth assumed the resistance of the air to vary as the cube of the velocity"; whereas in reality Bashforth found it convenient to take out the cube of the velocity as one factor of the resist-

ance, and to tabulate the other factor, as a slowly varying quantity.

Practically, we find that $\Delta^2 t$ is about one-thousandth of a second when $l = 150$, the distance between the screens in feet, so that d^2t/ds^2 is a decimal beginning with six or seven zeros. Mr. Bashforth avoids this inconvenience by writing the retardation—

$$10^9 \frac{d^2t}{ds^2}\left(\frac{v}{1000}\right)^3,$$

equivalent to reckoning the velocity in thousands of feet per second.

We have explained this notation at some length, as Mr. Bashforth has taken this and all other notation for granted as known, which is already given in his "Motion of Projectiles." The numerical values of K from the experiments are given in Table XI. for spherical and in Table XII. for ogival-headed projectiles; these two tables containing the complete theoretical deduction of all the author's numerous experiments.

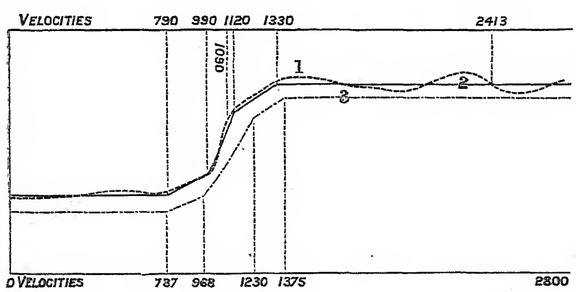
But as for very high or very low velocities the Newtonian law of resistance, varying as the square of the velocity, is more likely to be near the truth, the author has converted his coefficients K for the cubic law into coefficients k for the Newtonian quadratic law, tabulated in Tables I.–VI.; here, again, he has omitted to explain the formula required in the use of k; but it is easily inferred to mean that the resistance of the air is

$$d^2k\left(\frac{v}{1000}\right)^2 \text{ poundals, or } d^2\frac{k}{g}\left(\frac{v}{1000}\right)^2 \text{ pounds,}$$

so that the relation connecting k and K is $1000k = v\text{K}$.

In the practical use of the tables, we choose the one in which k or K is the more nearly constant and changes the slower.

The value of k for ogival-headed projectiles has been plotted graphically in the following diagram by Mr. A. G. Hadcock, quoted by Mr. Bashforth on p. 149; curve 1 being drawn from the result of Mr. Bashforth's experiments; curve 2 from the empirical laws of General Mayevski deduced from Bashforth's experiments; and curve 3 from the empirical laws of Captain Ingalls, drawn up to represent the resistance of the air according to Krupp.



The diagram is interesting as showing how far the Newtonian law is true for very low and very high velocities, and it confirms in a remarkable way the change in the value of k as we pass through the velocity of sound, so that its final value is about three times its initial value, as found out by Robins; insomuch that the resistance of the air to a 12-pound shot moving at 1700 f.s., which, according to the experiments of Newton on slow motions ("Principia," lib. ii., Props. 38–40), ought to have been 144 pounds, was found by Robins to be 433 pounds, or three times as much.

At velocities less than that of sound the projectile is always moving among its own waves; at greater velocities the point is supposed to be cleaving undisturbed air, like a swift steamer on the water; and now the chief element of resistance arises in the energy drawn off in the waves in the wake, waves which have been photographed by Mach and Salcher, according to an article signed "B." in NATURE.

Recently it has been discovered that with high velocities the shot carries the sound of the gun along with it, while backwards and sideways the sound is propagated at its ordinary rate; this phenomenon is sufficient to destroy the utility of range-finders based upon the observation of the velocity of sound.

Curve 3 indicates that the resistance of the air to Krupp's projectiles is about 10 per cent. less than to ours; this may be attributed to the sharper point, better centering obtainable with breech-loading, and a slightly less standard density of air; but Mr. Bashforth points out, with some justice, that the resemblance of Krupp's curve 3 to his own is rather suspicious, considering the small number of published experiments upon which Krupp's experiments are based.

Mr. Bashforth honestly prints all the values of K derived from his experiments, values often exhibiting great discrepancies among each other, and takes their mean as the true value; whether more delicate chronographs and improved electrical manipulation will enable us to refine on Bashforth's results remains to be seen, as a correction in the first decimal place of the value of K, depends upon the millionth of a second—a refinement we are very far off from having attained. Mr. F. J. Smith has given an account of a chronograph of his own invention, and in the August *Phil. Mag.* a description of a method of eliminating the latency in electro-magnetic records in chronographs, which may prove very useful. A chronograph to read directly to one ten-thousandth of a second is now the great desideratum: when chronographs were first brought out, the millionth of a second was glibly talked about, but so far, the thousandth is very good work indeed.

The experimental part of work is concluded when the value of K is obtained; but on these experiments Mr. Bashforth is able to build up his tables of T and S (XXIII.–XXXIII.), which enable us to calculate beforehand the performance of any gun, and save thousands of pounds in gunpowder at the price of a little ink.

Knowing C, the ballistic coefficient of the gun, then the formulas

$$t = \text{C}(\text{T}_\text{V} - \text{T}_v), \quad s = \text{C}(\text{S}_\text{V} - \text{S}_v),$$

connect the distance s in feet and the time t in seconds, for any initial velocity V, and final velocity v.

An additional table, for D, invented by Mr. W. D. Niven, is not given by Mr. Bashforth, but is found of great practical use; it gives δ, the deviation in degrees in a vertical plane for a flat trajectory, by the formula—

$$\delta = \text{C}(\text{D}_\text{V} - \text{D}_v).$$

Colonel Siacci, of the Italian artillery, has converted

Niven's D into circular measure, or natural tangents, and called it I; and has added another useful function, A, the altitude function. The use of these functions is indispensable in modern ballistics; but Mr. Bashforth does not mention them, as the chief purport of his book seems to be to put on record his own share of the work; and certainly, once the experimental part is done, it is a very easy matter to sit quietly indoors and theorize upon it.

A very searching test of Mr. Bashforth's tables was proposed in 1887, when it was decided to fire the "Jubilee rounds" from the 9·2 inch at elevations of 40°–45°, to see what is the extreme range attainable with modern artillery; and calculations were invited, to be sent in before the gun was fired. Mr. Bashforth prints the result of his calculations, which assigned a range of 19,426 yards with an elevation of 40° and an initial velocity of 2360 f.s. The range attained one day when the gun was fired was over 21,000 yards, and on another day was over 20,000, the difference being attributable to wind; so that, with no allowance for wind, and the fact that the initial velocity was really about 2375 f.s., we must consider that the calculation was close enough to show the value of Bashforth's coefficients; other calculators who allowed for the better shape and steadiness of the projectile obtaining even closer agreement. The calculation is interesting as showing the great height to which the projectile rises, and the consequent necessity for a frequent change in the coefficient of resistance due to the tenuity of the atmosphere.

Prop. VII., Robins's "New Principles of Gunnery," asserts:—"Bullets in their flight are not only depressed beneath their original direction by the action of gravity, but are also frequently driven to the right or left of that direction by the action of some other force."

This well-known effect in golf is still more marked in rifled artillery, especially with high-angle fire; and now in modern ballistic tables we have columns added for M and B, two functions calculated theoretically by General Mayevski, for assigning the value of this lateral deviation or drift.

Mr. Bashforth devotes chapter vi. to a popular exposition of this phenomenon, which is still somewhat wrapped in obscurity, in spite of all that has been written about it; a list of which writings is given by Captain Ingalls in his "Hand-book of Problems in Exterior Ballistics."

The stability of the axis of the projectile imparted by the rotation has the effect of making the head of the shot point slightly to the right of the vertical plane of fire with right-handed rotation, thus causing *drift*, and also of keeping the head a little above the tangent of the trajectory, so that in its descent the shot experiences a so-called *kite-like action*, tending to increase the range. It is well, however, for theorists to be on their guard in offering an explanation, as observers are not always agreed as to what really takes place.

Mr. Bashforth expresses a fear that, after all his labours, he will have produced very little effect; but we hasten to reassure him that his work is held in the highest estimation by those who have means of making a practical judgment.

A. G. GREENHILL.

BRITISH FOSSILS.

British Fossils, and where to seek them; an Introduction to the Study of Past Life. By J. W. Williams. Pp. 96, Illustrations. (London: Swan Sonnenschein and Co., 1890.)

AT the close of the introduction to this little volume, the author informs us that his object has been to convey to the young collector of British fossils the experience and knowledge acquired by others, whereby his own toil and labour may be lightened. The purport of this is admirable, but unfortunately the author has not succeeded in carrying out his good intentions. The volume is small, and merely a compilation; so there is no excuse for the number of errors and misprints by which its pages are disfigured.

The plan of the book seems to be to give a brief notice of each main geological horizon, with a list of some of the characteristic fossils, but we very much doubt whether long strings of generic names, like those given on p. 28, for example, are calculated to afford much assistance to the young collector, as there is practically no information as to what such terms really represent. None of the illustrations are original, the frontispiece being taken from Louis Figuier's "World before the Deluge," and most of the other figures from a well-known German work. And while on the subject of illustrations we should be glad to be informed why amphibians and reptiles like *Archegosaurus*, *Capitosaurus*, and *Placodus*, should have their skulls figured (as on pp. 45, 46) in a work on British fossils, when these genera are totally unknown from British strata. Such figures, as well as those on pp. 56, 57, may lead the inexperienced "young collector," for whom the book is avowedly written, to the conclusion that he may expect to meet with entire skulls and skeletons of fossil reptiles in his geological excursions. The proper course in these cases is, it need hardly be said, to give figures of teeth and some of the bones of such creatures, with which the tyro may be expected to meet, and to show how their generic affinities can be determined. Then, again, in reproducing the old figures of the Devonian fishes given on p. 33 the author might surely have alluded to the work of Dr. Traquair and other authorities showing how very far these figures are from being a truthful representation of these ancient creatures.

Leaving the illustrations, we may turn to the text. In glancing over the pages we were greatly puzzled to know what might be the meaning of the term *dermoid* types mentioned on p. 20, the repetition of the word indicating that it can scarcely be a misprint. Omitting mention of numerous misprints, obvious enough to the specialist, but terribly misleading to the beginner, we notice on p. 29 that *Tentaculites* is given as an Annelid, although its Pteropod affinities have long been known. Much discussion has taken place as to the affinities of the Palæozoic plant known as *Sphenophyllum*, but when on p. 43 the author calmly tells us that it is probably founded on the leaves of Calamites, he gives us a piece of information as new as it is erroneous. It is somewhat amusing to find the student referred, on p. 44, to the author's book on "Land and Fresh-water Shells," as if it were the only extant treatise on the subject; but when on p. 45 we are informed that Labyrinthodonts are characterized as a

whole by the presence of "a ventral armour of oval scales," we again have to wonder at the author's sources of information. It is indeed true that many Labyrinthodonts have a ventral armour of bony scutes, but these can scarcely be described as oval, and in the typical Labyrinthodonts, to which some authorities restrict the term, such scutes are totally wanting. The essential features by which the Labyrinthodonts are characterized the author carefully refrains from mentioning. A trap is set for the unwary on pp. 51, 52—the shell mentioned on the one page as *Ammonites bucklandi* being alluded to on the next as *Arietites bucklandi*. Equally unfortunate with the author's mention of the Labyrinthodonts is his allusion on p. 59 to the Mesozoic mammals, where he repeats the exploded idea that *Stereognathus* was an Ungulate, thus carefully ignoring all the recent work relating to that peculiar group known as the Multituberculata, which appears to be allied to the Duck-mole. On the same page *Megalosaurus* is carefully separated from the Dinosaurs, to appear as a carnivorous lizard, whereas in the list on p. 62 it is placed in the former group. How totally out of date is the list on the page last-mentioned ought, moreover, to have been known to anybody acquainted with recent palæontological literature. Page 63 is noteworthy as containing at least six misprints in the spelling of scientific names; but perhaps the climax of blunders is attained on pp. 74, 75. Thus, on the former page we are gravely told that *Hyracotherium* is a hog; and if one fact has been repeated over and over again almost *ad nauseam*, it is that *Hyracotherium* is one of the early progenitors of the horse, being, in fact, identical with the American *Eohippus*, and we can hardly believe that the author wishes the student to understand that horses are descended from hogs! On p. 75 *Lingula* is carefully separated from the Brachiopods, while the well-known Crag Polyzoan *Fascicularia*—one of the commonest of Suffolk fossils—is stated to be a shell!

Finally, the glossary is an explanation of certain mineralogical and chemical terms having for the most part no sort of connection with British (or, for the matter of that, with any other) fossils. What connection can possibly exist between "astrakanite—a compound of magnesium sulphate and sodium sulphate deposited in winter time in the salt lakes near the mouth of the Volga," and the fossils of the British Islands, we are totally at a loss to imagine. A similar remark will apply to eclogite, which is said to be a rock consisting of red garnets and hornblende; although it really is one of the pyroxenes.

R. L.

OUR BOOK SHELF.

Il Teorema del Parallelogramma delle Forze dimostrato erroneo (con figure.) By Giuseppe Casazza. (Brescia: Stabilimento Tipografico Savoldi, 1890.)

IT is curious that the mathematical paradoxer should confine himself principally to the problem of "squaring the circle"—that is, to the attempt to prove that π is the root of a quadratic equation with rational coefficients, in algebraical language; while other simpler questions are at hand in which he might prove himself superior to the conclusions of ages, by solving the problems of the "duplication of the cube" and the "trisection of an angle."

Some paradoxers attain their own ends by a wrong result, for instance, in putting $\pi = \sqrt{10}$—a result easily tested by counting the revolutions of a railway carriage wheel of given diameter, in a journey of given length; others by ignoring the rules of the game, as Napoleon is reported to have played chess.

Our author must be congratulated upon having started a fresh question of controversy, which had till now been universally regarded as settled for about three hundred years.

The "parallelogram of forces" must have been known experimentally for thousands of years longer; but in the orthodox world, what is considered at the present time the best and simplest way of proving it theoretically in a strictly rigorous manner? The proof of our youth given by Duchayla is now voted cumbrous and antiquated; and only retained by veteran examiners as a searching test of logical power. Nowadays we cannot afford the time to linger over the elements, and it is customary to treat the "parallelogram of forces" as a corollary to Newton's second law of motion; but this cannot be considered perfectly satisfactory, as we are making the fundamental theorem of statics depend upon a dynamical argument.

Maxwell pointed out that, as we were concerned with a statical theorem, it was better in the proof to ignore the word "resultant," and to present a system of balancing forces at each step; and in this way he succeeded in framing a more simple rigorous statical proof, starting from the axiom that the resultant of two equal forces bisects the angle between them.

Again, by determining the conditions of equilibrium of three parallel forces, instead of as usual determining the resultant of two parallel forces, one figure will serve for all possible cases.

Practically it is the "triangle of forces" which we always work with, and not the "parallelogram," with the advantage in graphical statics of using only three lines of construction instead of five.

To return to our author, it is difficult to make out, with an imperfect knowledge of his language, whether he is writing ironically or not. His dynamical language is very loose; he uses "force" and "velocity" as convertible; and throwing his remarks into the style of Galileo's dialogues, he seeks to controvert all Galileo's conclusions. On p. 17 he provides the critic with an appropriate and characteristic quotation with which to conclude—"ho pero spesso dei momenti in cui gettando all' aria i libri che mi trovo sotto mano, esclamo: *Ma io sono un allucinato!*"

A. G. G.

L'Esprit de Nos Bêtes. Par E. Alix. (Paris: J. B. Baillière et Fils, 1890.)

Les Facultés Mentales des Animaux. Par le Dr. Foveau de Courmelles. (Paris: J. B. Baillière et Fils, 1890.)

THE writers of these two books have very much the same object in view. Their aim is to show that the mental life of animals differs only in degree, not in kind, from that of man. If anyone still thinks that animals are merely machines, or that they have no higher faculties than instinct, it would be well worth his while to consider what either Dr. Courmelles or M. Alix has to say on the subject. No impartial person could study the evidence brought together by either of the two writers, and continue to doubt that animals display intelligence in the strictest sense of the term, and that they share in varying degrees many of the emotions which are often supposed to be exclusively characteristic of the human race. Of the two works, the one by M. Alix is the more elaborate. In both books the materials are well arranged, and the authors have persistently sought to present their facts and ideas brightly and pleasantly.

Elementary Arithmetic. By C. Pendlebury, M.A., F.R.A.S., and W. S. Beard, F.R.G.S. (London: George Bell and Sons, 1890.)

IN a book on elementary arithmetic it is necessary that there should be throughout a good and well graduated series of examples. The authors of the present volume have got together a large number of examples and problems for written work, and in addition they have arranged numerous sets for use in oral teaching form, a very important feature in an elementary work of this kind. The explanatory matter is written in intelligible and simple language, and great attention has been paid to the treatment of the money rules and the more important weights and measures.

This book is intended to serve as an introduction to the one on "Arithmetic for Schools," and the examples have been arranged so that they are all different from those given in the advanced work.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

British Association Procedure.

IT has been to me a matter of surprise that a letter by Prof. O. Lodge published in these columns on October 17, 1889, did not elicit other similar communications, as the views he enunciated are undoubtedly those of a not inconsiderable number of active members of the British Association. Prof. Lodge pointed out that the British Association week undoes the benefit of the previous holiday, mainly because the conditions under which the work of the Sections is carried on are prejudicial to health. This I know from considerable personal experience to be the case; and in this and previous years I have had the remark addressed to me: "Surely you are not going to *that* British Association meeting to make yourself ill again."

Prof. Lodge suggested that the Sections should sit from 10 a.m. till 1 or 1.30 p.m., and that the Sectional Committees should meet afterwards. Such an alteration would doubtless mitigate the evils inseparable from the present system, and it is to be hoped that a determined effort will be made at the ensuing meeting to promote its adoption. A recommendation somewhat to this effect was, I believe, made to the Council by at least one of the Sections at the Birmingham meeting, but nothing came of it. This is, perhaps, not surprising; indeed, it is a question whether anything ever does come of recommendations to the Council—so some say.

Machinery devised 50 odd years ago is no longer capable of satisfactorily coping with modern requirements. We do not go to British Association meetings to sit for hours together to hear papers read such as we have listened to *ad nauseam* during the previous sessions—our main object is to meet and exchange views; but everything seems to be done to prevent rather than to promote this. Far fewer papers should be read; far more care should be devoted to the selection of papers; much more should be done to encourage discussion, especially between Sections; and ample time and opportunity should be given for conversation.

The Sectional Committees are absurdly unwieldy bodies, and in the case of some Sections, practically comprise the entire Section: an appeal was made to the Council by my Section to put a stop to a practice which enables all the nobodies to become members of the Sectional Committee, but without result: I believe we were told that we could do as we liked. Had this been the case, we should scarcely have troubled the Council. The Sectional officers, with at most half a dozen other members, would form a far more useful Committee than any larger number; but if it be thought otherwise, let the whole Section sit as a Committee.

Lastly, a word may be said as to the date of meeting. Could any time be more unsuitable than the beginning of September? Most of those who are engaged in advancing science are then in the very middle of their holiday, and can attend only at grave personal inconvenience.

HENRY E. ARMSTRONG.

The Mode of Observing the Phenomena of Earthquakes.

THE publication of Mr. Davison's paper "On the Study of Earthquakes in Great Britain," in NATURE of the 7th inst. (p. 346) furnishes me with an opportunity of making a few *remarks*, followed by a *suggestion* as to the mode of recording the effects of seismic disturbances of the earth's crust on the apparent change of position, especially of vertical objects, in the field of vision of the observer.

Remarks.—It will, I think, be admitted that the descriptions of the alleged rocking to and fro of walls, towers, and chimneys, may not unfrequently convey an exaggerated idea of what really takes place; and, probably, the same is true of the narratives of personal experiences of reeling or rolling movements on the part of the narrator. I refer, of course, to the alleged *extent* of these movements, for no one can doubt their actual *occurrence* as the result of a *tremblement de la terre.* Such composite structures as walls, towers, and chimneys have a real flexibility and elasticity, as is shown, for example, by the opening and shutting up of cracks and fissures in their substance. But the extremely vivid accounts which we read of the swaying to and fro of solid buildings, as witnessed by persons in the upright position, and by others who are recumbent, suggest at least that some of these recorded disturbances of position in external objects may be more apparent than real, and may depend on some sudden uncontrollable movement of the head, and therefore of the optic axes of the observer's eyes.

It is well known that, when the head is moved swiftly to one side and back again to a vertical position, upright objects seen in front, appear to shift from their vertical position in an opposite direction, and then back again. It is not here needful to explain scientifically this very obvious phenomenon. A similar apparent displacement of objects, though in a vertical direction, occurs when the head is nodded backwards and forwards. Movements of the head in intermediate directions produce intermediate effects; whilst rotatory movements of the head give rise to corresponding though mixed kinds of disturbance of objects in the field of vision. Lastly, if the observer is in a horizontal position, as in bed, for example, a sudden rolling over of the head to one side and back again produces like phenomena.

Now such disturbing movements of the optic axes must frequently occur in the case of persons suddenly subjected to the consequences of earthquake tremors, whether such persons are in a vertical or in a recumbent position; and it is difficult to understand how they should not *occasionally* seem to exaggerate the apparent effects of the disturbance of the earth's surface and the objects planted upon it. The equilibrium of the observer's head is suddenly disturbed in a given direction, and an opposite involuntary movement instantly occurs in order to restore the previous condition of things. Granting, then, the *objective* reality of the swaying movements of vertical structures during earthquakes, there seems to me to be reason to think that the effect of these is occasionally enhanced, and their record influenced by the *subjective* impressions due to movements occurring in the observer's own optical apparatus.

Suggestion.—Supposing this view to be correct (though I can furnish no direct proof of its truth from earthquake records), it appears to me that the *suggestion* of which I spoke at the commencement of this letter would be a useful addition to paragraph (*a*) of Section 2, Division A, of Mr. Davison's paper (p. 348), which relates generally to the "situation of the observer." This suggestion is that it should always be noted and stated towards which *point of the compass* the observer's face was directed at the moment of each observation, concerning the deflection of upright buildings, rocks, and so forth, especially of their lateral deflections. For it is obvious that if a sufficient number of such observations were recorded, it ought to happen, on my hypothesis, that persons who looked *across* the earth-waves would be moved up and down, and thus would have the vertical movements only of objects in front of them exaggerated, whilst persons who looked along the waves would be swayed sideways by the undulations of the soil, and would therefore have the extent of the lateral movements apparently increased. Under the former condition, the "little hills" might appear to dance; in the latter case, cliffs, towers, walls, and chimneys might seem to sway inordinately from side to side.

Many such observations, duly recording the variations in the apparent *extent* of the movements noted, together with the *positions* of the observer as regards the compass, would, when

compared with the ascertained direction of the earth-waves, confirm or upset my general supposition. But, if this were found to be correct, such observations would furthermore constitute, even in the absence of special seismic instruments, a certain amount of evidence as to the actual direction of the earth-waves on any particular occasion.

Similarly, a record of the exact direction of recumbent observers in regard to the points of the compass, might, when compared with their respective descriptions of the movements of objects about them, serve a similar purpose.

Man himself would thus to a certain extent—that is, as regards the local direction of the earth-waves—be his own seismometer. Possibly, some evidence on this subject might even now be obtained by comparing the descriptions of the appearances with the ascertained directions of the outlook of different observers. JOHN MARSHALL.

92 Cheyne Walk, Chelsea.

On a Problem in Practical Geometry.

IN treatises on practical geometry rules are given by which an arc and its chord or an arc and its tangent may be divided proportionally, but they leave an error which is often too great.

By the following method the points of division move step by step towards their required positions until errors are of less than any assigned amount.

Let GMH be the chord (Fig. 1), M its middle point, AOB the perpendicular diameter. In AOB produced take a series of points B′B″B‴..., determined thus: BB′ = BG, B′B″ = B′G... Then evidently circles with centres at BB′B″..., passing through G, form a series of which each has on its circumference the centre of the succeeding one. These circles cut the line AM in a series of points A′A″..., and the arcs

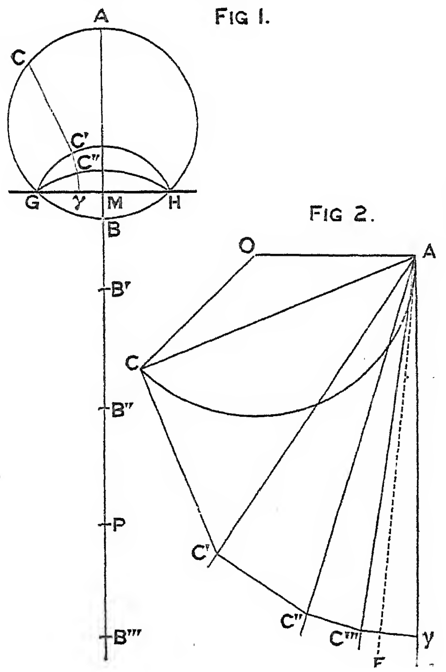


FIG 1. FIG 2.

GAH, GA′H, GA″H... get rapidly nearer the straight line GH. Any point C on the given arc GAH may now move to its destination γ on the line GH by stepping up to each circle in the direction of its centre; along a path CC′C″..., made up of CC′ tending to B, C′C″ tending to B′, and so on.

That all the arcs which this path crosses and the chord to which it tends are divided proportionally at the points CC′C″... γ follows from the almost obvious theorem that if the centre of one circle is on the circumference of another, lines drawn from that centre intercept arcs of the circles having a constant ratio.

If the circles become inconveniently large before the required approximation is reached, we may use the following: C′C″... are the centres of the circles inscribed in GCH, GC′H..., which are easy to construct. Also it may be noticed that the angle made with AO by each of the parts of CC′C″... is half that made by the preceding part, and the process may be brought to an end at any stage with progressive accuracy by making the last angle one-third instead of half of the preceding one.

In Fig. 1 the process is closed after the second stage by drawing the last line (C″γ) not towards B″, but towards the middle point P of B″B‴. This may be done as soon as the last arc GC″H comes to be less than a quadrant.

When the chord becomes the tangent AT at A, the points AA′A″... coincide, all the circles have AT touching them at A, the radius of each is half that of the succeeding one, the arcs intercepted AC, AC′, AC″... are equal, and so we get in the limit Aγ, the length of the arc AC laid out on the tangent.

But in Fig. 2 an alternative construction is shown. Bisect TAC by AC′, TAC′ by AC″, and so on. Draw CC′, C′C″... at right angles to AC, AC′... The process is shown closed after the third stage by drawing C‴γ at right angles, not to AC‴, but to a line AF such that C‴AF is one-third of C‴AT. In the result Aγ is equal to arc AC.

JOHN BRIDGE.

Caught by a Cockle.

I HAVE often intended writing to you describing a curious occurrence which I witnessed on the coast of Queensland in September 1889, but I have as often forgotten to do so when the opportunity came. While out shooting, along a sandy beach, I noticed a small muddy patch just covered by the rising tide. In this I observed a bird, a sand-piper, which seemed to be striving in vain to rise. I could not think how the bird had become caught, but on coming up to it I found that one claw of one foot was firmly held by a large cockle (about 1½ in. by 2 in.). Of course the bird would have been drowned eventually (though the benefit to be derived by the cockle seems rather problematical); and though it seemed to be aware of its danger, yet it had made no attempt to free itself by trying to bite through the claw, as one sometimes reads of animals doing when caught in traps. As I believe this is rather an uncommon incident, I must make that my excuse for troubling you. D. MCNABB.

H.M.S. *Dart*, New Hebrides, July 3.

ON STELLAR VARIABILITY.

ON the hypothesis of the meteoritic origin of the various orders of cosmical bodies there is a grand and orderly variation, both in light and colour, in the case of every undisturbed swarm during its condensation from its most nebulous condition to that of a cool dark globe.

As by virtue of the ordinary evolutionary process an *undisturbed* swarm successively passes through the changes the results of which define the various groups, the light will wax through Groups I. to IV., and then wane till it is finally extinguished; at the same time the colour sequences will be successively passed through. But with such a variability as this, compared with the period of which our *annus magnus* is but a point of time, we have now nothing to do. We have to deal really with *disturbed* swarms or with double or multiple swarms through their various stages of condensation.

Let us take the purely disturbed swarms first. Imagine a nebula, sparse, and therefore so dim as hardly to be visible at all. Then, further, imagine the appulse of another, or the approach of a meteoritic stream. We shall have the condition which must bring about increased luminosity; the outburst may be short or sudden; the greater luminosity may last a short or a long time; the dying down of the light may be fast or slow. In that we shall have the possibilities of new and dying stars.

If the spectrum of the light produced by this clashing be observed, we may not have precisely the same phenomenon as that observed in the various groups defining

the result of the orderly condensation of a single swarm, for the simple reason that we shall have two swarms or bodies to deal with. Even if the very highest temperature is reached, we shall not have *exactly* the same spectrum as that presented by Group IV.

The most stupendous case illustrating the above remarks is to be found in the Pleiades, the true structure of which has been revealed to us by Mr. Roberts. The principal stars are not really stars at all; they are simply *loci* of intercrossings of meteoritic streams, the velocities of which have been sufficiently great to give us, as the result of collisions, a temperature approaching that of α Lyræ, so far as we can judge by the spectrum; but that the α Lyræ conditions are not present is evidenced by the fact that in Pleione the broad dark hydrogen lines have a bright thin line running down their centres, indicating that we have intensely-heated hydrogen outside that which is absorbing.

So long as these meteoritic streams are interpenetrating and disturbing each other, so long the Pleiades will shine; but their light may soon cease if the disturbance comes to an end, for we are not dealing with masses of vapour like α Lyræ. Indeed, one of them seems to have already become invisible. Of the seven daughters of Atlas, one has disappeared. The "septem radiantia sidera" are seven no longer. The seventh had vanished before the time of Aratus.

> "The Pleiades; small the region
> They fill, and pale the light they dart.
> Seven journeyers men call them
> Though only six are visible to ken.
> No star, I wis, has vanished from Heaven's floor
> Within mortal tradition, and idly is that number
> Fabled. Natheless seven the names they bear:
> Alcyone, Merope, Celæno, Electra,
> Sterope, Taygete, and stately Maia." [1]

At the beginning of the action to which I have ascribed the present light of the Pleiades, we should have the appearance of a "new star," and the greater the light produced and the more sudden the outburst the more certainly would the appearance of a new star be chronicled. Many such stars have burst forth, and the phenomena recorded have been entirely in harmony with the explanation afforded by the hypothesis; but, as the discussion of these phenomena is not yet complete, I shall not in the present article touch further upon them; but I may point out that, before the existence of "variable stars" was recognized, as it is now, the increase in magnitude of a variable at maximum, rendering visible to the naked eye what was before invisible, was attributed to the creation of a new star. Hence it is that the first work done on the periodicity of variable stars grew out of observations of so-called Novæ.

Leaving on one side, then, any question of Novæ, we will inquire into the growth of our knowlege of stars the light of which is known to wax and wane with more or less regularity, and see to what causes this variability has been ascribed. We have to consider those shorter periods of light-variation, well within human ken, light-changes which, instead of taking millions and perhaps billions of years, are undergone in a few days, or weeks, or months. Such changes have been abundantly chronicled from the earliest times and acknowledged to be among the most mysterious phenomena presented to us in the skies.

In this historical survey we must first consider the case of Mira or o Ceti. It is now nearly three centuries ago since Fabricius noticed this star (August 1596), thinking it to be a *nova*, and watched its disappearance in the following October.[2]

Not only Fabricius but Kepler looked upon Mira Ceti as a new star similar to those of 1572 and 1604. Indeed, it was regarded as such until 1638, when some observations by Phocylides Holwarda brought out for the first time the fact that the changes in magnitude repeated themselves. The work done by this astronomer is quoted by Hevelius.

Holwarda first observed the star in December 1638, when it was brighter than a third magnitude; he watched it decrease to the fourth, and disappear during the summer of 1639. On December of the same year he again observed it. There is no doubt, indeed, that Holwarda was the first to demonstrate by these observations that the light of stars is liable to periodic changes in intensity.

Fullenius, a teacher of mathematics at Franecker, was the next to observe Mira. He noted that the star was visible on September 23, 1641, and the same date in the following year. In August 1644, however, no trace of it could be made out.

Junquis, a professor at Hamburg, recorded that Mira was of the third magnitude on February 18, 1647, and was invisible from July 1648 to November of the same year.

It was Hevelius, however, who made the first detailed investigation into the variations of the light of this star. Beginning in January 1648 he assiduously watched the changes in magnitude until March 1662, and placed the question of variability beyond the possibility of a doubt.

During the fifteen years of observation Hevelius saw the star go through its changes in magnitude many times, and noted that it was always invisible for several months in the year. He did not, however, determine the period, although it will be seen that the following observations would have been sufficient for him to have deduced an approximate value:—

Sept. 10, 1660—"Instar stellæ 4 magn. fere."
Aug. 20, 1661—"Vix quartæ magnitudinis extitit."
Interval, 344 days.
Sept. 20, 1660—"Æqualis illi in ore Ceti."
Aug. 29, 1661—"Æqualis illi in ore Ceti."
Interval, 353 days.

The determination of the period of Mira Ceti was deduced by Bouillaud in 1667 from all the observations which had been made from its discovery in 1638 to 1660. This discussion occurs in a rare book having the title "Ismaelis Bullialdi ad Astronomos monita duo: Primum de Stella Nova, quæ in Collo Ceti ante annos aliquot visa est. Alterum, de nebulosa in Andromedæ cinguli parte Borea, ante biennium iterum orta."

A review of the book appeared in the first volume of the Philosophical Transactions (p. 381), from which the following account of Bouillaud's conclusions have been taken:—". . . That one *period* from the *greatest phase* to the next consists of 333 days; but that the interval of time betwixt the times of its beginning to appear equal to stars of the *sixth magnitude*, and of its ending to do so consists of about 120 days; and that its *greatest appearance* lasts about fifteen days: all which yet he would have understood with some latitude.

"This done, he proceeds to the investigation of the causes of the vicissitudes in the emersion and disappearance of this star, and having determined that the apparent increase and decrement of every lucid body proceeds *either* from its changed distance from the eye of the observer, *or* from its various site and position in respect of him, whereby the angle of vision is changed, or from the increase or diminution of the bulk of the lucid body itself; and having also demonstrated it impossible that this star should move in a *circle* or in an *ellipsis*, and proved it improbable that it should move in a *strait line*, he concludes that there can be no other genuine, or at least no other more probable cause of the emersion and occultation than this: That the bigger part of that round body is obscure and inconspicuous to us, and its lesser part lucid, the whole body turning about its own center and one axe, whereby for one determinate space of time it exhibits its lucid part to the Earth,[1] for

[1] Poste's translation, p. 13.
[2] Kepler, "De Stella," chap. xii. p. 112.

[1] Here we have the germ of Sir Wm. Herschel's reference to the action of varying amounts of spotted surface; Maupertius' idea of rotatory disks; and Prof. Pickering's suggestion of axes of different lengths.

another, subducts it, it not being likely that fires should be kindled in the body of that star, and that the matter thereof should at certain times take fire and shine, at other times be extinguist upon the consumption of that matter. . . ."

This, so far as I know, is the first proposed explanation of stellar variability on record.

The next star in which variability of light was observed was χ Cygni. Kirch's observations made in 1686 and subsequent years were communicated to the Royal Society. He observed the star with the aid of an eight-foot tube in August 1687. It became visible to the naked eye in October, increased in brightness and reached a maximum in November, and finally disappeared. This observer also found that the star had always the same brightness at a maximum, and in assigning it a period of $404\frac{1}{2}$ days, he noted that this duration was irregular.

These observations bring us to the time of Newton, who at once saw that the cause of true Novæ must be distinct from that producing variability pure and simple. He ascribed the sudden appearance of new stars as possibly due to the appulse of comets :—

"Sic etiam stellæ fixæ, paulatim expirant in lucem et vapores, cometis in ipsas incidentibus refici possunt, et *novo alimento accensæ pro stellis novis haberi.* Hujus generis sunt stellæ fixæ, quæ subito apparent, et sub initio quam maxime splendent, et subinde paulatim evanescunt. Talis fuit stella in cathedra Cassiopeiæ quam Cornelius Gemma octavo Novembris 1572 lustrando illam cœli partem nocte serena minime vidit ; at nocte proxima (Novem. 9) vidit fixis omnibus splendidiorem, et luce sua vix cedentem Veneri. Hanc Tycho Brahæus vidit undecimo ejusdem mensis ubi maxime splenduit ; et ex eo tempore paulatim decrescentem et spatio mensium sexdecim evanescentem observavit."[1]

But with regard to the ordinary variables, he accepts Bouillaud's suggestion, and adds another :—

"Sed fixæ, quæ per vices apparent et evanescunt, quæque paulatim crescunt, et luce sua fixas tertiæ magnitudinis vix unquam superant, videntur esse generis alterius, et revolvendo partem lucidam et partem obscuram per vices ostendere. Vapores autem, qui ex sole et stellis fixis et caudis cometarum oriuntur, incidere possunt per gravitatem suam in atmosphæras planetarum et ibi condensari et converti in aquam et spiritus humidos, et subinde per lentum calorem in sales et sulphura et tincturas et limum et lutum et argillam et arenam et lapides et coralla et substantias alias terrestres paulatim migrare."

Both Montanari in 1669 and Maraldi in 1692 observed that the magnitude of β Persei or Algol was variable.

The information they gave with respect to changes of the star from the second to the fourth magnitude, though important, was not very definite, and it was left to Goodricke, an English astronomer, to discover, in 1782, the periodicity of these variations and to conclude:—(1) "That the star changes from about the second to the fourth magnitude in nearly three hours and a half and then back to the second magnitude again in the same time. (2) That this variation occurs about every two days and twenty-one hours."[2] Flamsteed observed the star in 1696, and found it to be of the third magnitude, and Goodricke, by comparing it with one of his own, deduced the more accurate value of 2 days, 20 hours, 48 minutes, 56 seconds. At the end of the observations Goodricke added the note: —"I should imagine that the cause of this variation could hardly be accounted for otherwise than either by the interposition of a large body revolving round Algol, or some kind of motion of its own whereby part of its body covered with spots or such-like matter is periodically turned towards the earth."

Another variable observed by Goodricke was β Lyræ.

[1] "Principia," p. 525 (Glasgow, 1871).
[2] "Phil. Trans., 1783, p. 474.

His first observations brought him to the conclusion that the star had a periodical variation of nearly six days and nine hours, but a further investigation showed that the true period was twelve days nineteen hours, there being two maxima and minima. At one minimum the magnitude of the star is between four and five, at the other between three and four.

Zöllner, in a relatively recent discussion advances very little beyond the views advocated by Newton. In considering the main causes of variability, he lays the greatest stress upon an advanced stage of cooling, and the consequent formation of scoriæ which float about on the molten mass. Those formed at the poles are driven towards the equator by the centrifugal inertia. and by the increasing rapidity of rotation they are compelled to deviate from their course. These facts, and the meeting which takes place between the molten matter, flowing in an opposite direction, influence the form and position of the cold non-luminous matter, and hence vary the rotational effects, and therefore the luminous or non-luminous appearance of the body to distant observers. This general theory, however, does not exclude other causes, such as, for instance, the sudden illumination of a star by the heat produced by collision of two dark bodies, variability produced by the revolution of a dark body, or by the passage of the light through nebulous light-absorbing masses.

Among modern inquirers Prof. Pickering has been more original in his suggestions. He has shown that the light-curves of some stars may be explained by supposing them to have axes of different lengths, with dark portions at the ends, symmetrically situated as regards the longer axis.

In the following discussion of the cause of variability suggested by the meteoritic hypothesis, I shall divide variability into regular and irregular, defining regularity by constantly recurring maxima and minima on the light-curves.

THE CAUSES OF VARIABILITY SUGGESTED BY THE METEORITIC HYPOTHESIS.

Regular Variability.—All regular variability in the light of cosmical bodies is caused by the revolution of one swarm or body round another (or their common centre of gravity).

In the case of the revolution of one swarm round another an elliptic orbit is assumed, and the light *at maximum* is produced by collisions among the meteorites at periastron.

In the case of the revolution of a swarm round a condensed body, the light *at maximum* is produced by the tidal action set up in the secondary swarm.

In the case of one condensed body revolving round another, the light *at minimum* is caused by an eclipse of one body by the other. This can only happen when the plane of revolution of the secondary body passes very nearly through the earth.

Irregular Variability.—All irregular variability in the light of cosmical bodies is caused (*a*) by the revolution of more than one swarm or body round another (or their common centre of gravity) ; or (*b*) by the interpenetration of meteoritic sheets or streams.

In the case of the revolution of more than one swarm round another in elliptic orbits, the irregular maxima are caused by differences of period and periastric conditions

So far as I know, the only previous explanation of variability on such a basis as the one above stated, which assigns the revolution of one mass round another as a cause of variability, is the one we owe to Newton, who suggested that such stellar variability as we are now considering was due to conflagrations brought about at the maximum by the appulse of comets ; and no doubt his

idea would have been more thoroughly considered than it has been hitherto, if for a moment the true nature of the special class of bodies we are now dealing with had been *en évidence*. We know that some of them at their minimum put on a special appearance of their own in that haziness to which I have before referred as having been observed by Mr. Hind. My researches show that they are all nebulæ in a further stage of condensation, and such a disturbance as the one I have suggested would be certain to be competent to increase the luminous radiations of such a congeries to the extent indicated.

Some writers have objected to Newton's hypothesis on the ground that such a conflagration as he pictured could not occur periodically; but this objection I imagine chiefly depended upon the idea that the conflagration brought about by one impact of this kind would be quite sufficient to destroy one or both bodies, and thus put an end to any possibilities of rhythmically recurrent action. It was understood that the body conflagrated was solid like our earth. However valid this objeetion might be as urged against Newton's view, it cannot apply to mine, because in such a swarm as I have suggested, an increase of light to the extent required might easily be produced by the incandescence of a few hundred tons of meteorites.

I have already referred to the fact that the initial species of the stars we are now considering have spectra almost cometary, and this leads us naturally to the view that we may have among them in some cases swarms with double nuclei—incipient double stars, a smaller swarm revolving round the larger condensation, or rather both round their common centre of gravity. In such a condition of things as this, it is obvious that, as before stated, in the swarms having a mean condensation this action is the more likely to take place, for the reason that at first the meteorites are too sparse for many collisions to occur, and that, finally, the outliers of the major swarm are drawn within the orbit of the smaller one, so that it passes clear. The tables, which shall be given hereafter, show that this view is entirely consistent with the facts observed, for the greater number of instances of variability occur in the case of those stars in which, on other grounds, mean spacing seems probable.

I propose here to consider the suggested cause of variability somewhat in detail. I will begin with Groups I. and II.

In these groups the variability is produced by the revolution of one or more smaller swarms round a central swarm, the maximum luminosity occurring at periastron, when the revolving swarms are most involved in the central one.

According to the theory, the normal condition is that which exists at minimum, and in this respect it resembles that suggested by Newton—namely, that the increase of luminosity at maximum was caused by the appulse of comets. All other theories take the maximum as the normal condition, and the minimum as a reduction of the light by some cause—large proportion of spotted surface, or what not.

Anything which in the normal minimum condition of light-equilibrium will increase the amount of incandescent gas and vapour in the interspaces will bring about the appearance of the hydrogen lines and carbon flutings as bright ones. The thing above all things most capable of doing this in a most transcendental fashion is the invasion of one part of the swarm by another one moving with a high velocity. This is exactly what I postulate. The wonderful thing under these circumstances then would be that bright hydrogen and carbon should *not* become more luminous, not only in bright-line stars, but in those the spectrum of which consists of mixed flutings, bright carbon representing the radiation.

We may consider three cases of revolution. Taking that first in order which will give us the greatest light range, we find that this obviously will occur in those systems in which the orbits are most elliptic and the periastric distances least.

On the other hand, a mean ellipticity will give us a mean range.

In these two cases, to account for the greatest difference in luminosity at periastron passage, we have supposed the minor swarm to be only involved in the larger one during a part of its revolution, but we can easily conceive a condition of things in which the orbit is so nearly circular that it is almost entirely involved in a larger swarm. Under these conditions, collisions would occur in every part of the orbit, and they would only be more numerous at periastron in the more condensed central part of the swarm, and it is to this that I ascribe the origin of the phenomena in those objects—a small number—in which the variation of light is very far below the normal range, one or two magnitudes instead of six or seven.

Now it is at once obvious that we should get more variability in these early groups than in any of the more condensed ones, for the reason that in the latter we require the conditions either that the plane of revolution should pass through the earth, or that the light of the central star shall be relatively dim.

This point is best studied in relation to Group II.

The total number of stars included in Argelander's Catalogue, which deals generally with stars down to the ninth magnitude, but in which, however, are many stars between the ninth and tenth, is 324,118. The most complete catalogue of variables (without distinction) that we have has been compiled by Mr. Gore, and published in the Proceedings of the Royal Irish Academy (Series II., vol. iv., No. 2, July 1884, pp. 150-163). I find 191 known variables are given; of these 111 are in the northern hemisphere and 80 in the southern hemisphere.

In the catalogue of *suspected* variable stars given in No. 3 of the same volume (January 1885, pp. 271-310), I find 736 stars, of which 381 are in the northern and 355 in the southern hemisphere. Taking, then, those in the northern hemisphere, both known and suspected, we have the number 492. We have, then, as a rough estimate for the northern heavens one variable to 659 stars taken generally.

The number of objects of Group II. observed by Dunér, and recorded in his admirable memoir, is 297; of these 44 are variable. So that here we pass from 1 in 657 to 1 in 7. Of the great development of variability conditions in this group then there can be, therefore, no question.

Further, while by the hypothesis there is no limit to the increase of luminosity, the variability presented by these objects is remarkable for its great range. The light may be stated in most general terms to vary about six magnitudes—from the sixth to the twelfth. This, I think, is a fair average; sometimes a difference of eight magnitudes has been observed; the small number of cases with a smaller variation I shall refer to afterwards. A variation of six magnitudes means roughly that the variable at its maximum is somewhere about 250 times brighter than at its minimum; a variation of eight magnitudes means that it is 1600 times brighter at maximum than minimum.

These values alone would indicate a condition of things in which the minimum represents the constant condition, and the maximum, one imposed by some cause which produces an excess of light. These various conditions having been premised in considering these groups, I will first deal with the nebulæ.

That many of the nebulæ are variable is well known, though, so far as I am aware, there are no complete records of the spectroscopic result of the variability. But bearing in mind that in some of these bodies, such as the Dumb-bell Nebula, we have the olivine line almost by itself; and in others, which are usually brighter, we have the lines of hydrogen intensified, as in Orion; and in others,

more condensed still, the flutings of carbon added, as in Andromeda; it does not seem unreasonable to suppose that any increase of temperature brought about by the increased number of collisions should increase the intensity of the lines of hydrogen or carbon in the spectrum of a nebula.

The observations already accumulated show conclusively that in the nebulæ—even those so far condensed as the one in Andromeda—the temperature is low; in other words, the meteorites are very far apart; regular variability, therefore, would for this reason be very difficult to detect. It is probable, therefore, that in all the cases previously recorded, we are not dealing with the results of rhythmic action, but the interpenetration of nebulous streams or sheets. When, however, we come to the stars—that is, the more condensed nebulæ—in Group I. and Group II., the temperature is higher, the condensation is greater, and the interaction of double or multiple nebulæ can be more easily traced. This fundamental difference of structure between these bodies and stars like the sun should be revealed in the phenomena of variability; that is to say, the variability of the uncondensed swarms should be different in *kind* as well as in degree from that observed in bodies like the sun or α Lyræ, taken as representing highly condensed types.

Since the stars with bright lines, as I have shown, belong to the former group, and since, therefore, they are very akin to nebulæ, we might, reasoning by analogy, suppose that any marked variability in their case also would be accompanied by the coming out of the bright hydrogen lines. This is really exactly what happens both in β Lyræ and in γ Cassiopeiæ. In β Lyræ the appearance of the lines of hydrogen has a period of between six and seven days, and in γ Cassiopeiæ they appear from time to time, although the period has not yet been determined.

Another star of Group I., η Argûs, is also remarkable from the fact that its light varies in the same sort of way. This star is in the southern hemisphere, and during the last twenty or thirty years a considerable discussion has been going on among astronomers as to whether the surrounding nebula is or is not changing its position with regard to the star in question, which has a bright-line spectrum like β Lyræ, and a period not of thirteen days, but of seventy years. The light varies from the sixth to the first magnitude.

Leaving Group I. and coming to Group II., there is one star, Mira Ceti, whose variations in light-intensity may be taken as characteristic. The history of the discovery of this star's variabiltty has already been given. What happens to it in just a little less than a year is this. First, it is of the second magnitude, and then in about eighty days it descends to the tenth, and, so far as observations with ordinary instruments go, it is invisible. In about another hundred days it again becomes visible as a star of the tenth magnitude. It then increases its light to the second magnitude, and begins the story over again. But sometimes at the maximum its brilliancy is not quite constant. That is to say, sometimes it goes nearer the first magnitude than the second. What happens to the light of the star below the tenth magnitude it is not easy to say. What one knows is that to some telescopes it remains invisible for about 140 days or something like that, and then it begins its cycle over again.

I owe to the kindness of Mr. Knott the opportunity of studying several light-curves of "stars" of this group, and they seem to entirely justify the explanation which I have put forward. It is necessary, however, that the curves should be somewhat carefully considered, because in some cases the period of the minimum is extremely small, as if the secondary body scarcely left the atmosphere of the primary one but was always at work. But when we come to examine the shape of the curves more carefully, what we find is that the rise to maximum is extremely rapid: in the case of U Geminorum, for instance, there is a rise of five magnitudes in a day and a half; whereas the fall to minimum is relatively slow. The possible explanation of this is that the rise of the curve gives us the first sudden luminosity due to the collisions of the swarms, while the descent indicates to us the gradual toning down of the disturbance. If it be considered fair to make the descending curve from the maximum exactly symmetrical with the ascending one on the assumption that the immediate effect produced is absolutely instantaneous, then we find in all cases that I have so far studied that the star would continue for a considerable time at its minimum.

Broadly speaking, then, we may say that the variables in this group are *close doubles;* the invisibility of the companion being due to its nearness to the primary or to its faintness.

We now pass from Groups I. and II. to III., IV., and V. These contain the hottest, and therefore brightest bodies in the heavens. They are, moreover, more condensed than those we have considered. On this ground, their normal light cannot be *increased* to any very great extent by any constantly recurring action, but it may be *reduced* by eclipses caused by the revolution of still further condensed secondary swarms. The nearer the primary, and therefore the smaller the period of the secondary body, the more likely is the eclipse to occur regularly. There are several Group IV. stars of this class, notably Algol, to the first observations of which we have already referred.

This body, which is always visible in our latitudes, well illustrates this class of variable. If we take the beginning of a cycle, it is a star of the second magnitude; suddenly in three hours it goes down to the fourth, and then it comes up in another three hours to the second, and goes on again for very nearly three days; and then it goes down again, comes up again, and goes on again for another three days, and so on.

There is another star very like this—a star which is in 81° N. declination, No. 25 in Argelander's Catalogue. The difference between Algol and this is that the rise and fall are a little more rapid. Its light is feeble for about the same time as the other one, but at the bottom the curve is flat, by which I mean that, instead of going suddenly down and coming suddenly up again, it stops at its least luminosity for some little time.

Prof. Pickering[1] has demonstrated by photographs of the spectra of Algol that Goodricke's explanation of its periodical variability is correct, the companion having no light of its own. In the case of the star D. 81° N. 25 there must be luminosity from the star which eclipses the other. And a very beautiful justification of this view has recently been noted, because, although there is no change in the spectrum of Algol, there is a considerable change in the spectrum of the star, the bottom curve of which is flat, showing that probably the companion has an absorbing action of some kind on the light of the central star passing through it or its surroundings. The light practically changes very much as our sunlight would change if it had to pass through the atmosphere of another sun somewhat like itself coming between us.

In Group VI. we again have a new condition. In these stars the light is relatively faint, and the variation is doubtless due to swarms of meteorites moving round a dim or nearly dark body, the maximum occurring at periastron when the tidal action in the swarm is greatest; hence the addition of the light of what we with our solar conditions should term a large comet would make a great difference in the total radiation.

J. NORMAN LOCKYER.

[1] Proc. Amer. Acad. Sci., vol. xvi. p. 17

SOME POINTS IN THE PHYSICS OF GOLF.

It is not an easy matter to determine the initial speed of a golf-ball:—but this is so only because the direct processes which have given us so much information about the flight of military projectiles are here practically inapplicable. No doubt, a ballistic pendulum, or a Bashforth chronograph, might after long and tantalizing experiment give us the desired information. If they did, they would give it much more accurately than we are otherwise likely to obtain it. But the circumstances of a "drive" at cricket or golf are so uncertain, even with the best of players, that it would be waste of time, and wanton vexation of spirit, to employ these instruments of precision. Yet the questions involved are of a very interesting kind, not only from the purely physical point of view but also in consequence of the recent immense development of these national games; so that there is considerable inducement to attempt at least a rough solution of some of them.

The following investigation, because based mainly on mere eye-observations usually of a rather uncertain and difficult kind, is offered only as a rude attempt at a first approximation; and I am quite prepared to find myself obliged to modify the results, when new and more accurate information is forthcoming.

My main reason for bringing it forward in such a condition is to enlist if possible (at this, the proper season) a few keen and accurate observers, who may occasionally find themselves in a position to obtain data of real value. Thus I shall devote what might otherwise be considered an excessive amount of attention to the nature of the real *desiderata*, and to the quality and the sources of the more common errors of the estimates which have been kindly furnished to me. Such as they were, however, they enabled me to state to the Royal Society of Edinburgh, on July 20, conclusions as to initial speed, and coefficient of resistance, nearly agreeing with those given below.

The influence of even a moderate wind on the flight of a golf-ball is so very considerable that, in the first part of my paper, I shall consider the flight of a golf-ball in a dead calm only, and when it has been driven fair and true without any spin. In a former article (NATURE, Sept. 22, 1887) I have discussed the effects and the causes of spin. Also I shall confine myself to the "carry," as the subsequent motion depends so much upon purely accidental circumstances. By far the most valuable data connected with the subject are those which can be obtained in calm weather alone, and which bear on the form, dimensions, and duration of the first part of the course of the ball. It is mainly due to the excessive rarity of *perfectly* calm days that our knowledge of the data is so slight.

Under these restrictions, it is somewhat curious to find that the extreme carry of a golf-ball is not very different from that of a cricket-ball. Both may be spoken of as somewhere about 200 yards. But the circumstances of propulsion are in general very different:—for, unless it is specially teed on a slope, or driven with a spoon (in which case its initial speed is necessarily reduced), a golf-ball goes off at a very moderate inclination to the horizon:—while the sensational drives at cricket usually have the unquestionable advantage of a much higher trajectory.

Theoretically, the proper position of an observer who wishes to secure at once all the required data should be some miles to one side of the plane of flight, so that he should see the trajectory, as it were, orthogonally projected on a dark background of cloud. The small size of the ball, even if there were not other insuperable difficulties, makes observations in this way impossible. Hence each distinctive feature of the trajectory must be separately studied; and this implies either a staff of observers, or, what is much less easy to obtain, a player who can make a number of successive drives almost exactly "similar and equal" to one another. I am convinced that many of the great incongruities which I have found among the data furnished to me, even by skilled observers, are due mainly to the fact that the measures of different characteristic features had been made on drives essentially different in character from one another.

Another fertile source of error lies in the too common assumption that, because a gentle breeze only is felt by the players, who may possibly be in the lee of a sand-hill, there is nothing beyond a similar breeze at heights of 60 to 100 feet; whereas, at that elevation there may be a pretty strong wind. Unless attention is most carefully paid to this, the estimates of the position of the highest point of the trajectory are sure to be erroneous.

The desiderata which are of real importance; and which must, if possible, be obtained from one and the same drive:—the air being practically motionless:—are

1. The initial inclination to the horizon.
2. The range (on a horizontal plane) of the carry.
3. The maximum height attained.
4. The horizontal distance of this maximum from the tee, expressed (say) as a fraction of the range (2).
5. The time of flight.

To these we may add, though it is of less importance, and also much more difficult to estimate with any approach to accuracy,

6. The final inclination to the horizon.

These data are not independent:—in fact theory (such as it is) shows several relations among them. But, as no one of them, except the second and fifth, admits of accurate determination, it is desirable to measure as many of them as possible; so that they may act as checks on one another.

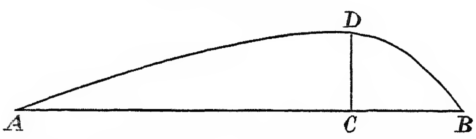
We may also add, what I have recently been endeavouring to obtain:—

7. The horizontal distance passed over in the first second.

This, if properly ascertained, would be one of the most directly useful of the whole set of attainable data.

My experience has been that observers always over-estimate the values of the quantities 1, and 6, above:—though they state their ratio fairly well as about 1 : 3. The time of flight, 5, also is usually given too great. But the greatest over-estimate occurs in the case of datum 4. This exaggeration puzzled me very much at the outset of my inquiry. It is easy to see that, in order to produce a path such as that sketched below, in which, (according to estimates sent me from St. Andrews a couple of months ago, when I was unable to procure them myself)

$$AC : AB :: 3 : 4;$$



(where D is the highest point of the trajectory) the initial speed and the resistance must *both* be very great. For clearness, the vertical scale is much exaggerated.

Thus I was led to make some experiments with the view of finding an approximation to the utmost admissible initial speed. This I tried to obtain by measuring the speed of the club at impact, and multiplying by 1·6. A hollow india-rubber shell, of the size of a golf-ball, was teed in front of a horizontal axle on which were fixed, six inches apart, two large pasteboard disks with broad borders of very thin white calico. The ball was teed on a level with the axle, midway between the planes of the disks, and three inches beyond their extreme edges. A stout wire, dipped in black paint, projected from the nose of

the club. A drive was then made, in a direction parallel to the axis; first, with the disks at rest; second, when they were revolving about nine times per second. From the result of the first experiment, the correction for the second, due to the fact that the club did not move exactly parallel to the axis, was roughly determined. The results obtained varied within wide limits; *i.e.* from 140 to 700 feet per second for the speed of the club-head at impact. But the majority of the experiments gave from 200 to 300 feet per second. The golfer whose services I enlisted for these experiments, though a very good player, confessed that the novelty of the circumstances had prevented his doing himself justice:—the revolution of the disks, in particular, tending to prevent him from "keeping his eye on the ball." There can be little doubt that the main cause of discrepance among the results was the fact that the correction had to be found when the disks were at rest, and to be applied to data obtained when they were moving. At the time, I formed from these experiments the conclusion that the initial speed of the *ball* must be somewhat over 400 feet per second. I have since been led to believe that this is an under-estimate. I hope when I return to my Laboratory, to carry out this class of experiments with more satisfactory results; by repeating, under favourable conditions, an electrical process which recently failed from the employment of inadequate apparatus.

So long as the speed of a spherical projectile is less than that of sound, it appears that the resistance of the air is at least approximately as the square of the speed. (It is on this account that the effect of even a light head, or following, wind is so considerable. For it is the *relative* speed that determines the resistance, and even a small change in a quantity makes an important change in its square.) Our knowledge of this question is as yet very imperfect; but we cannot fall into any egregious error by making our calculations on the assumption that this law is correct. To apply it, however, we require a numerical datum, *e.g.* the resistance (in terms, say, of the weight of the golf-ball) for unit speed.

Robins, more than a century ago, gave as the result of experiments a statement equivalent to the following:—The terminal speed of an iron sphere in ordinary air is that which it would acquire by falling, *in vacuo*, through a space of 300*d* yards, where *d* is the diameter in inches.

From this it is easy to calculate that the resistance-acceleration of a golf-ball should be about

$$-\frac{v^2}{400};$$

where v is the speed in feet per second, and the denominator is 400 feet.

In the recent edition of *The Bashforth Chronograph*[1] we find that, for an iron shot whose diameter is d inches, and mass w pounds, the acceleration due to the resistance of the air at speed v (expressed in feet per second) is

$$-\frac{118{\cdot}3\,d^2}{w}\cdot\frac{v^2}{1000^2}.$$

It is clear that this expression holds for spheres of any material. For the whole resistance depends only on the size and speed, while the acceleration due to it is inversely as the mass. Now for an average golf-ball $d = 1{\cdot}75$ nearly; and $w = 0{\cdot}101$, because the specific gravity of gutta-percha is nearly the same as that of water. Hence we may express the acceleration by

$$-\frac{v^2}{280}$$

very nearly:—the denominator being in feet.

I have decided to employ Bashforth's result as probably the more accurate:—my own independent estimate, above alluded to, having given 300 in place of 280. It indicates resistance some 43 per cent. greater than that deduced from the older reckoning of Robins. In the formulæ below we will write a for Bashforth's 280 feet.

For a golf-ball not under the influence of gravity the equation of motion would therefore be

$$\ddot{x} = -\frac{\dot{x}^2}{a};$$

which gives, if V be the speed when $t = 0$,

$$\frac{1}{\dot{x}} - \frac{1}{V} = \frac{t}{a},$$

or

$$\dot{x} = v = \frac{V}{1 + \frac{Vt}{a}}.$$

From this we have

$$x = a \log\left(1 + \frac{Vt}{a}\right),$$

and

$$v = V\epsilon^{-\frac{x}{a}}.$$

Thus in general, as $\epsilon^{-0{\cdot}7} = \frac{1}{2}$ nearly, the speed, whatever it be, is reduced to half when the ball has moved through 196 feet, or about 65 yards. The time of passage is 280/V.

In treatises on *Dynamics of a Particle* (Tait and Steele, for instance) it is shown that, for the assumed law of resistance, the approximate equation of a flat trajectory is

$$y = \left(\tan\alpha + \frac{ga}{2V_0^2}\right)x - \frac{ga^2}{4V_0^2}\left(\epsilon^{\frac{2x}{a}} - 1\right).$$

In obtaining this result it has been assumed that dx/ds may be treated as being practically unity. This gives a fair approximation to the form of the path of a golf-ball up to, and a little beyond, its highest point; but can scarcely be relied on for the last 30 yards or so of the path, where the inclination to the horizon becomes considerable. But the error will not be a very serious one. If we reject this approximate equation we are forced to use the intrinsic equation of the path, which can be integrated exactly. But, though its use can be made comparatively *simple* by employing a graphic method, it is always very *tedious*, and therefore only to be resorted to in the last extremity; and when we are in possession of data far more exact than any yet obtained. The same may be said, so far as data are concerned, of the elaborate Tables calculated by Bashforth. If we had *accurate* information as to the speed at the highest point of the trajectory, these would give us all that could be desired.

In the above formula V_0 represents the horizontal component of the initial speed:—or, practically, with the limitation introduced, the initial speed itself. α is the angle of projection, and has been carefully determined as on the average about $13^\circ{\cdot}5$. Its tangent is 0·24. Mr. Hodge, to whose valuable assistance I owe this as well as many of my other data, found it absolutely necessary to use a clinometer, as the eye-estimates of the angle of projection are almost always greatly exaggerated. The only other datum required to complete the equation is an approximate value of V. Two methods of finding it were tried, as follows:—

From a number of (necessarily very rough) observations, made by holding to the ear a watch ticking 4 times per second, it seems that in the first second a well-struck ball goes on an average somewhere about 100 yards.

Hence the initial speed must be about

$$280(\epsilon^{15/14} - 1) = 537 \text{ feet per second.}$$

An error of 1 p.c. in this measurement entails 1·6 p.c. error in the result.

The average time of flight seems to be about 4·5 seconds

[1] Cambridge University Press, 1890. For this reference, and for some much needed explanations, I am indebted to Prof. Greenhill.

for a very good drive. As the length of the path is somewhere about 600 feet, the initial speed must be about 468. This, also, is an exceedingly rough estimate, as the effect of gravity has been omitted. The percentage error here is the same as that of the observed time, but has the opposite sign.

Taking them together, these two estimates appear to indicate an initial speed of about 500. Let us for a moment assume this to be the true value, and see how it will agree with the other facts of the case.

Introducing the assumed data, we have for the typical trajectory

$$y = 0{\cdot}258x - 2{\cdot}524(\epsilon^{x/140} - 1).$$

The value of x for the maximum of y is given by

$$0{\cdot}258 - \frac{2{\cdot}524}{140}\epsilon^{x/140} = 0;$$

so that $x_0 = 372$, and $y_0 = 62$, at the highest point of the trajectory. These values, especially that of y_0, agree very well indeed with those independently observed; so that we have a first hint that our assumptions cannot be much in error.

The range (so far as this approximation goes) is to be found by putting $y = 0$ in the general equation. This leads to

$$14{\cdot}31 = \frac{140}{x}(\epsilon^{\frac{x}{140}} - 1).$$

By the aid of a table of values of the function $(\epsilon^z - 1)/z$, which I constructed for the purpose of this inquiry, I find easily

$$\bar{x} = 140 \times 4{\cdot}08 \text{ nearly} = 571.$$

This, again, is a tolerable approximation to the observed range; and, as above stated, we could not expect more. Now nothing in golf is more striking than the well-known fact that, once a player is able to drive a fairly long ball, he secures comparatively little increase in his range by even a great additional exertion. Assuming that the additional effort is well and truly applied (and this is usually, as most men too well know, a *very* large assumption indeed) its only effect must be to increase the initial speed. Let us see how an increase of initial speed to 600 feet per second will increase the range, other things being the same. Performing the calculations as before, the rough equation for the range becomes

$$20{\cdot}165 = \frac{140}{x}(\epsilon^{\frac{x}{140}} - 1);$$

and $\bar{x}$ is found to be $140 \times 4{\cdot}51$, nearly, $= 631$ feet, or only about 20 yards more. Yet the initial energy of the ball was 44 per cent. greater. So far as this point is concerned, our result is in good accord with experience. On the other hand, if we assume the initial speed to be 400 feet per second only, we find

$$\bar{x} = 3{\cdot}55 \times 140, \text{ nearly}, = 497.$$

This represents a fair, but not an exceptionally good, drive. It thus appears that our assumption, of an initial speed of about 500, meets adequately the requirements of the data for a really fine drive, so far as yet tested.

The ranges for initial speeds of 100, 200, ... , 600 feet per second are, in order, 112, 277, 400, 497, 571, 631. (Had there been no resistance, the ranges would have been as the square numbers, 1, 4, 9, ... , 36.) From these data it would appear that the great majority of golfers give the ball an initial speed of some 200 to 250 feet per second, only:—very frequently not so much, even off the tee:—and that to obtain a carry of double amount, the ball must have nearly quadruple energy.

We may now apply the test supplied by the datum (4). We have, for initial speed 500,

$$x_0 = 372, \bar{x} = 571,$$

so that, in the figure above,

$$AC : AB :: 372 : 571.$$

The ratio is rather *less* than 2 : 3; whereas according to observation, it ought to have been greater; though, of course, always less than 3 : 4. But I do not attach much importance to this discrepancy, as the estimate made of the highest point of the path is at best a rude one, and depends very much upon the position of the observer. For instance, it is almost impossible for him to make even a guess at its true position if it should happen to be situated nearly above his head.

I have calculated a number of trajectories for larger values of a, and with V correspondingly reduced, so as to keep the carry the same. But all seem to give too great a value for the maximum height attained; and to place that maximum too near the middle of the carry; to suit the long, raking, drives which have furnished my data. The estimated value, 500 feet per second, of the initial speed in "tall" drives like these, may appear a little startling at first. But anyone who knows how to *cut* a tough ragweed with a thin cane, instead of merely bruising it, as ninety-nine men in a hundred would certainly do at the first attempt, will recognize the sort of *nip* which a really skilled golfer gives at the instant of striking the ball.

It is curious to reflect that it is the resistance of the air, alone, which makes it possible for the legislature to tolerate the game of golf. For the normal drive which was studied above would, but for the resistance of the air, have a carry of 1250 yards (more than two-thirds of a mile) and the ball would fall at that distance with its full initial speed of 500 feet per second! The golfer might deal death to victims whom he could not warn with the most Stentorian "Fore." He could carry, at St. Andrews, from the first tee to the "Ginger Beer" hole! This illustrates, though in a very homely and feeble way, the service which the atmosphere is perpetually rendering us by converting into heat the tremendous energy of the innumerable fragments of comets and meteorites which assail the earth from every side with planetary speeds.

When there is a steady wind, even when it blows in the plane of flight, the mathematical problem is much more difficult:—and this difficulty is not sensibly less when an approximate solution only is sought. For the speed of the wind depends on the height above the earth; and, even if we take the simplest law for this dependence, neither of the equations can be treated separately.

It is easy, however, to see the general nature of the effect. In driving against the wind, the resistance (which of course depends on the *relative* velocity) is greater than in still air:—but its direction is no longer in the line of flight, except at the highest point of the path. It acts in a direction less inclined to the horizon than is the path, and therefore its effect on the horizontal component of the velocity, as compared with that on the vertical component, is greater than in still air. With a following wind, unless it be going faster than the ball, the opposite effects are produced. The general result is to affect the carry considerably, and the vertical motion but slightly. The time of flight is probably a little shortened by a following wind, while it is lengthened by a head wind. The belief, prevalent among golfers, that a ball rises higher against a head wind, and lower with a following wind, than it would do in a calm, is due directly to the effect of perspective:—the highest point of the path being shifted nearer to, or further from, the player. The true effects on the greatest height reached are usually too small to be detected by a casual observer.

The diameter of a cricket-ball is nearly 3 inches, and its weight 5·5 oz. The value of a for its motion is therefore 327 feet. Partly on this account, but more on account of its lower speed, a cricket-ball has its path much less affected by resistance than is that of a golf-ball. If we take its maximum initial speed as 130, the initial resistance is only about 1·6 times its weight; while for a golf-ball it rises to about 28-fold its weight. Their momenta are nearly equal, being about 45 and 50 respectively. But their kinetic energy is very different in the two cases, being 90, and 390, foot-pounds respectively. This, again, is in full accord with every-day experience. In the simple vernacular of the cricketer, a well-struck golf-ball would be characterized, at least for the first fifty yards or so of its course, as a "hot" one indeed!

The article may fitly close with a few remarks on another very prevalent fallacy:—viz. the belief that a golfer continues to guide his ball with the club long after it has left the tee. How any player who has ever "jerked" a ball (and who has not?) could maintain such an opinion is an inscrutable mystery. But it is a physical fact, established by actual measurement, that when a block of wood weighing over 5 pounds is let fall on a golf-ball (lying on a stone floor) from a height of 4 feet, the whole duration of the impact is less than 1/250 of a second. When it falls from a greater height the duration of impact is less. But if the elastic force which made the block rebound had been employed to move the golf-ball itself, whose weight is only 1/10 of a pound, (or 1/50 of that of the block) the operation would have occupied only 1/50 of the time; say the 1/12,500 of a second. In the case before us we are dealing with much greater speeds, and therefore with still smaller intervals of time. It is with veritable *instants* like these that we are concerned when driving a golf-ball. The ball has, in fact, left the club behind, before it has been moved through more than a fraction of its diameter.

Another way in which this important point can be made plain to anyone is as follows:—When two bodies impinge, the whole time of the mutual compression is greater than that which would be required to pass over the space of linear compression with the relative speed, but less than twice as great. And the time of recoil is greater than that of approach in the ratio 1 : e:—where e is the "coefficient of restitution" which, with hard wood and gutta-percha, is about 0·6 when the relative speed is very great. Hence the whole time of impact between the club and the ball is that in which the club, moving at 300 feet per second, would pass through about four times the linear space by which the side of the ball is flattened.

P. G. TAIT.

THE WORKING EFFICIENCY OF SECONDARY CELLS.

UNDER this title a paper was contributed, at the recent meeting of the Institution of Electrical Engineers at Edinburgh, by Prof. Ayrton and Messrs. Lamb, Smith, and Woods, which contains some considerable additions to our knowledge of the subject of secondary cells. The cells on which the tests were made were of the 1888 E.P.S. type, and were charged and discharged at the maximum working currents, these being kept constant in value by hand and automatic regulation. In the most important series of tests the limits of volts employed was 2·4 volts for charge and 1·8 volt for discharge: it was found that a lower limit than this led to detrimental actions in the cells, with loss of active material.

The advantages of a constant current are that it is a nearer approximation to practical working conditions, and that the calculations are much simplified: in fact, the ampere efficiency is got by simply multiplying together the ratio of the charge and discharge currents and the ratio of the times occupied in charging and discharging. The true (or watt) efficiency was found by plotting time readings of the P.D., and taking the ratio of the areas of the curves thus drawn: this, multiplied by the ampere-efficiency, is the required true efficiency.

The first important point brought out in the paper is the importance of the resuscitating power possessed by accumulators. In an early set of tests, made on well-charged cells, the authors found a quantity efficiency of over 100 per cent. with correspondingly abnormal watt efficiency, and this, although the tests occupied five days, from which they conclude that, "if accumulators be well charged up before being tested, five days' continuous alternate charging and discharging with the maximum currents allowed by the manufacturers fails to give the normal working efficiency."

Since these results were so unsatisfactory, some method of avoiding drawing on a previous store had to be adopted. Some experimenters secured this condition by running down a cell, and then leaving it short-circuited for some time. In the present series of experiments the required condition was fulfilled as follows: the cells were continuously charged and discharged with regularity until the successive charges occupied exactly the same time, and successive discharges did also. When the cells arrive at such a "steady state," it can evidently be taken that no drawing on a previous store is taking place. It was, then, under these conditions that the experiments were made.

As such a long series of experiments would entail much labour in keeping the current constant, an automatic regulator was devised to effect this, together with further automatic devices for breaking circuit when the P.D. reached any predetermined value, and for telling the time when such break occurred. The authors state that these apparatuses worked to within ½ per cent. of the supposed limits. Throughout the investigation D'Arsonval instruments were adopted, and by suitably suspending the movable coil, the calibration curve was absolutely a straight line. In these further tests the same instrument was used for measuring volts and amperes, the requisite alteration of circuit being made by a rocking commutator. The volts were read frequently, and curves of P.D. plotted. With this apparatus and measuring instruments, the curves given below for steady state of

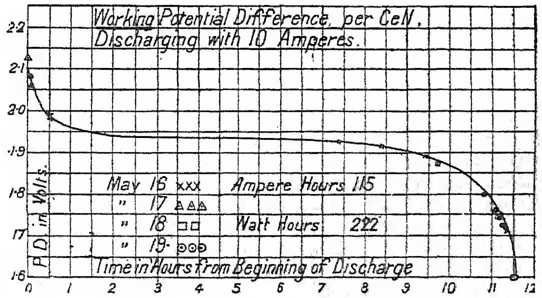


FIG. 1.

charge and discharge between limits of 2·4 and 1·6 volts per cell were obtained.

From these curves efficiencies of 98·3 per cent. for current, and 86·5 per cent. for energy, were obtained.

It was then found that 1·6 volt was far too low a limit to take, as scaling of the plates took place, and so (as mentioned above) limits of 1·8 and 2·4 volts were adopted. After several charges and discharges, the cells arrived at a "steady state" again, the successive times being as follows:—

Discharges. h. m.		Charges. h. m.
10 10	... ...	11 38
10 10	... ...	11 37
10 11	... ...	11 37

"showing to what an absolutely definite state cells arrive after a definite cycle of charge and discharge between fixed limits has been repeated continuously, without interruption, for some weeks." These results give an ampere efficiency of 97·2 per cent. and an energy efficiency of 87·4. These they adopt as the true steady values for this type of cell, and this shows a working storage capacity of 21,380 foot-pounds per pound of plate.

The next point brought out is the effect of rest on a charged cell; the cells were fully charged, and allowed to rest in that state, being well insulated from everything. In every case the first discharge and charge show a marked falling off in capacity and efficiency, the latter being reduced to 58 per cent. in one case cited. A point of some theoretical interest is brought out in connection with the curves obtained in this part of the investigation: a normal discharge curve *falls* rapidly at first, then remains constant, and falls again, as shown in Fig. 1;

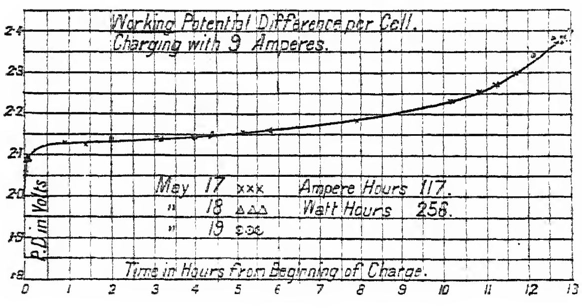


FIG. 2.

the first discharge curve, *after rest*, *rises* at first instead of falling.

The authors sum up this part of their paper as follows:—

"From all that precedes, it follows that the previous history of an accumulator produces an enormous effect on its efficiency. If, for example, an E.P.S. accumulator be over and over again carried round the cycle of being charged up to 2·4 volts per cell and discharged down to 1·8 volt per cell, the charging and discharging currents being the maximum allowed by the makers—viz. 0·026 ampere per square inch in charging, and 0·029 ampere per square inch in discharging—the '*working efficiency*' thus obtained may be 97 per cent. for the ampere-hours and 87 per cent. for the watt-hours. If, on the contrary, the cell be constantly charged up before being tested, then for the first few charges and discharges between the above limits, and with the same current-density in charging and discharging, even the *energy* efficiency may be as high as 93 per cent.; whereas, if the accumulator has been left for some weeks, then, although it was left charged, the energy efficiency for the first few charges and discharges will be as low as 70 per cent.

"While, on the one hand, our tests show that continued rests of a charged accumulator appear to be far more serious for the accumulator than we had previously imagined, the working efficiency appears to be higher than has hitherto been supposed, since we believe that about 84 per cent. efficiency in the watt-hours is all that the advocates of accumulators have claimed for them."

The next section deals with some points connected with the chemical action, and it is shown that the actual amount of SO_3 liberated on charge per ampere-hour, as calculated from the change of specific gravity, agrees well with the ordinary simple formulæ. We understand a further paper on this point may probably be forthcoming later on, which will deal with the chemical changes going on in the plugs at various points in the charge and discharge. As this involves the partial destruction of a cell, and a lengthy series of analyses, it was not found possible to put it in the present paper.

The next point brought out is of considerable interest: during several charges and discharges, the difference of temperature between the working cell and a neighbouring idle cell was observed frequently, and it was noticed that the cell cooled during discharge, in spite of the heat generated by the resistance. This was simultaneously observed by Prof. Duncan in America. The general shape of the temperature curves is given below.

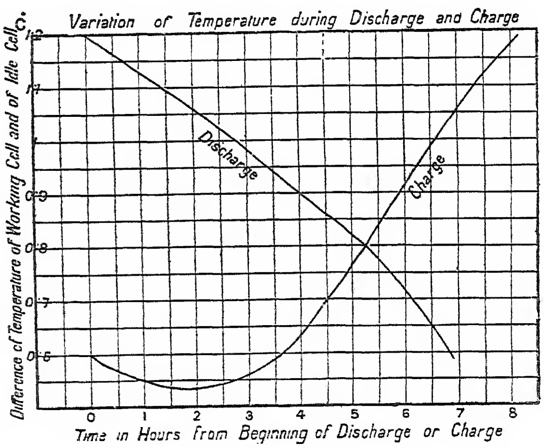


FIG. 3.

From the mean excess of temperature of the cell the authors deduce the somewhat startling fact that 17 per cent. of the energy put into the cell is wasted by radiation and convection. As they found that but 13 per cent. is really lost, it follows that the rest of the energy must be given by some sort of primary battery action, so that they consider an accumulator is partly a reversible and partly an irreversible battery. In this way the gradual deterioration is accounted for. Possibly this may partly account for the short life of small accumulators.

The concluding section deals with the question of the resistance of cells when brought to a steady state. The method adopted is that of introducing an opposing E.M.F. in the voltmeter circuit when time readings of the E.M.F. are being taken on breaking circuit. From these the E.M.F. at breaking circuit is found by producing backwards to zero, and the P.D. being also measured, together with the current before breaking, we

can get the resistance at the moment of break. The method is delicate, and seems to have yielded good results: but lack of time has prevented this section being dealt with with other than one set of current values.

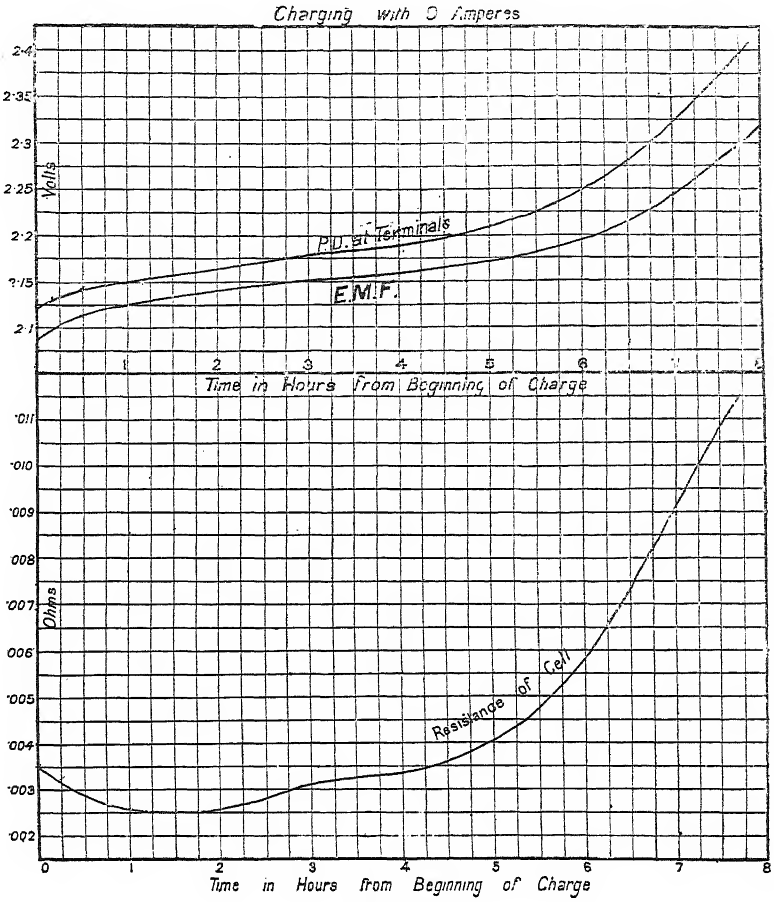


FIG. 4.

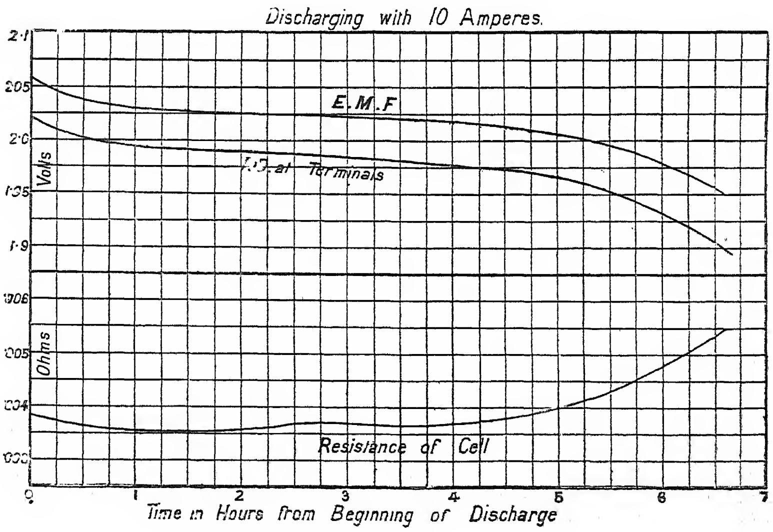


FIG. 5.

We print the curves given in the paper for resistance on charge and discharge.

The paper seems, on the whole, a useful and suggestive contribution to the current knowledge on the subject.

NOTES.

AT a meeting lately held at Stonyhurst College, as we have already recorded, it was decided that a memorial of the late Father Perry should be established. It is proposed either that a new telescope with a 15-inch object-glass shall be erected at Stonyhurst, or that the present equatorial stand shall be furnished with a 15-inch objective. Whichever be adopted, the telescope and the house in which it stands will be called the "Father Perry Memorial," and the work done with the instrument will be published under this name. For the complete telescope and house, £2700 would be required; for the objective alone the sum needed would be £700. It is hoped that funds large enough for the more magnificent monument may be obtained. A more appropriate memorial could not have been suggested, and we have no doubt that many friends and admirers of the late Father Perry will be glad to take this opportunity of expressing their appreciation of his character and work. Subscriptions should be sent either to the "Father Perry Memorial" account, at the London Joint Stock Bank, Limited, Pall Mall Branch, London, S.W., or to Arthur Chilton Thomas, Hon. Secretary and Treasurer, *pro tem.*, Marldon Chambers, 30 North John Street, Liverpool.

THE remains of Captain John Ericsson are now being conveyed across the Atlantic to their last resting-place in Sweden. The transfer of the body, on Saturday, from New York to the war-ship *Baltimore*, was made the occasion of a striking ceremony in honour of the memory of the great inventor. The coffin, wrapped in the flag which floated from Ericsson's famous naval ram, the *Monitor*, in the struggle with the *Merrimac*, was escorted down Broadway by a procession; and among the mourners were the Secretary of the Navy, Admiral Wordon, who commanded the *Monitor* in the engagement with the *Merrimac*; Admiral Braine, commanding the Navy; General Howard, commanding the Army; the Mayors of New York and Brooklyn; and the members of the Swedish Legation. The New York correspondent of the *Daily News* says that dense crowds witnessed the procession, standing with bare heads as it passed. Flags, Swedish and American, were displayed in great profusion throughout the city and harbour. Great bands of streamers festooned fronts of the buildings along the city's main thoroughfare, and from the windows hung colours in endless profusion and variety. The harbour never presented a more charming picture. All the shipping along the water front were dressed for the day with the flags of all nations, at half mast. The body was placed upon a tug at the Battery, and was taken down a long line of shipping to the *Baltimore*, which lay waiting to receive it. The day was rarely beautiful, and the great harbour swarmed with craft of all descriptions. Below the *Baltimore*, stretching in a long line down the bay, were ranged other war-ships. Minute guns were fired till the body reached the ship, when the brief ceremonies of receiving the body were completed. The *Baltimore* then ran up the Royal naval ensign of Sweden, and steamed slowly down the line of battle-ships towards the sea, each vessel raising the same ensign as she passed and firing a salute of 21 guns. The forts at the Narrows also saluted as she passed.

THE meeting of the Iron and Steel Institute in America promises to be very successful. About 300 members and many of their friends will leave England for New York next month. The week beginning on September 29 will be devoted to the reading of papers and discussions by members of the American Institute of Mining Engineers and the English Iron and Steel Institute; and, again, on October 9 and the two following days, these two bodies will co-operate at an international meeting at Pittsburg. Afterwards, excursions will take place to the iron ore and copper regions of Lake Superior and to the new iron-making district of Alabama. The American Reception Committee will provide sleeping and luncheon cars to take their visitors over 3000 miles of the United States.

THE eighth meeting of the International Congress of Americanists will be held in Paris from October 14 to 18. Questions relating to history and geography, archæology, anthropology and ethnography, and linguistics and palæography, have been drawn up by the organizing committee for the consideration of the Congress. Communications regarding the forthcoming meeting should be addressed to M. Désiré Pector, General Secretary of the Organizing Committee, 184 Boulevard Saint-Germain, Paris.

THE American Forestry Association is to meet at Quebec on September 2, and will sit four days. By invitation of the Government of the Province of Quebec, the meetings will be held in the Parliament buildings.

THIS week the Sanitary Institute has been holding its twelfth Annual Congress at Brighton. The business of the Congress was opened on Monday evening, when Sir Thomas Crawford delivered his presidential address in the music-room of the Royal Pavilion, before a large assembly. After a tribute to the late Sir Edwin Chadwick, the president dealt with "some fragments of the story of laws violated to the prejudice of health." On Tuesday an interesting address on "The Living Earth" was delivered by Dr. G. Vivian Poore, President of the Section for Sanitary Science and Preventive Medicine. Mr. W. H. Preece lectured on Tuesday evening on the sanitary aspects of electric lighting.

THE new number of the Journal of the Royal Horticultural Society contains a full and interesting report of the Daffodil Conference held at Chiswick in April last. The Conference lasted two days, and on the first day the chair was taken by Prof. Michael Foster, who, in the course of his opening address, offered some remarks on the naming of different forms of the daffodil. He urged that new names should be given only to forms which can be readily recognized as distinct by ordinary people, and are more beautiful than, or differ in beauty from, their forerunners. A new name should also, he thought, be, if possible, one that can be easily written, and easily read, and that "can be spoken, if not easily, at least without great effort." Mr. J. G. Baker, who presided on the second day, said that twenty years ago very few people took any interest in daffodils, but now the daffodil shared with the primrose the honour of being the most popular flower of the spring-time. "The genus," he added, "is of great interest from a botanical point of view. We are obliged as botanists to deal with all plants on one uniform plan as regards arrangement and nomenclature. From that point of view we look upon *Narcissus pseudo-narcissus* as a single aggregate species, and, comprised within this, there are in gardens about 200 forms. In the whole genus we have only about twelve or sixteen distinct species in this sense. The greatest change at the present time is the raising of forms from species or varieties not known to hybridize before, and it is wonderful that all the Narcissi cross so freely, many of them—as, for instance, *N. pseudo-narcissus* and *N. poeticus*—being so distinct from each other in form."

THE Australasian Association for the Advancement of Science has published a volume containing a report of its first meeting, held at Sydney, New South Wales, in August and September 1888. The editors are Prof. A. Liversidge, F.R.S., and Prof. R. Etheridge, Jun. The volume includes many valuable addresses and papers, and is well illustrated.

THE death is announced of Dr. Felix Liebrecht, an early and highly successful student of folk-lore and comparative mythology,

He was born at Silesia, in 1812, and after studying at Breslau, Munich, and Berlin, became a professor at Liége, where he remained during the greater part of his working life. He translated the "Pentamerone" of Basile from the Italian in 1846, and in the following year issued a version of the romance of Barlaam and Josaphat from the Greek of Johannes Damascenus. In 1851 he translated Dunlop's "History of Fiction." A curious treatise by Gervase of Tilbury attracted his notice as being a sort of encyclopædia of mediæval folk-lore, and he brought out an edition of it in 1856 enriched with many valuable notes. A selection of his contributions to periodical literature on his special subject appeared in 1879, entitled "Zur Volkskunde."

PROF. FLOWER and Mr. Lydekker are engaged in preparing for publication a work entitled "An Introduction to the Study of Mammals, Recent and Extinct." It is based mainly upon the articles contributed by the first-named author and Mr. G. E. Dobson to the ninth edition of the "Encyclopædia Britannica," but much new matter will be added, and the whole brought up to date. The publishers are Messrs. Black, of Edinburgh, and the work is expected to appear before the end of the year.

MESSRS. D. C. HEATH AND CO., Boston, announce the publication of a new number in the series of "Guides for Science Teaching," issued under the auspices of the Bostonian Society of Natural History. The book is entitled "Insecta," and is written by Prof. Hyatt, Curator of the Natural History Society. It is extensively illustrated.

MR. J. B. MARCOU'S "Bibliography of North American Palæontology in the year 1886" has been reprinted from the Smithsonian Report for 1886–87.

THE City and Guilds of London Institute has issued its programme of technological examinations for the session 1890–91. In a special paper attention is called to various alterations and additions.

THE University Correspondence College has published, in its Tutorial Series, a Directory containing all necessary information as to London Intermediate Science and Preliminary Scientific Examinations.

DURING the cruise of the U.S.S. *Thetis* in the Behring Sea and Arctic Ocean, in 1889, several officers were directed to prepare reports on subjects connected with the waters and regions visited by the vessel. One of the reports drawn up in accordance with instructions related to the Eskimos of north-western Alaska, and was written by Mr. John W. Kelly, who had spent three winters among the north-western Eskimos. This report has now been published by the United States Bureau of Education, and all students of ethnography will find in it much that cannot fail to interest them. It is accompanied by English-Eskimo and Eskimo-English vocabularies, prepared by Ensign Roger Wells, chiefly from information furnished by Mr. John W. Kelly. These vocabularies are primarily intended for teachers in Alaska, but it is expected by the Bureau of Education that they will also be of service to officers of the navy and of the revenue marine service, to all Goverment officials in Alaska, to committees of Congress visiting the country, and to many others who for any reason may desire to study the Eskimo language.

THE U.S. Bureau of Education has issued a Bulletin, by Prof. C. F. Smith, of the Vanderbilt University, on "Honorary Degrees as Conferred in American Colleges." The author protests vigorously against the lavish way in which various American institutions raise incompetent persons to the rank of "doctor."

WE have received from the Santiago Observatory (Chile) two volumes comprising the meteorological observations for the years 1882–87. This Observatory, which is furnished with the best instruments, has published observations in the present form since 1873, although they had been taken and partially published for many years previously. The observations for three hours daily are given in a tabular form, and the means for each day are laid down in curves. Full particulars as to the instruments and methods of observation are given in an earlier volume, and the series presents most valuable materials for the study of the climate in those distant parts.

THE Italian Meteorological Office has published its "Annals" for 1886, consisting of four folio volumes. The first volume contains the Report of the Director of this extensive organization, and shows that there were in that year 123 observatories and stations at which complete observations were made, and 630 stations recording temperature and rainfall. This part also contains some valuable memoirs, among which may be mentioned: the climate of Massowah, by P. Tacchini; the comparison of anemometers, and evaporation, by Dr. Ragona; the temperature of snow at different depths, and of the air above it, by Dr. Chistoni, &c. Vols. ii. and iii. contain the ten-day, monthly, and annual means for the various stations, and the results of evaporation and cloud observations. Vol. iv. deals more especially with earthquake phenomena, and contains investigations on several shocks which have occurred in Italy, together with memoirs on the various seismographs used in different countries.

THE Archæological Survey of India perseveres with its unostentatious task of reclaiming from ruin and oblivion the countless inscriptions which lie scattered about India, offering a clue to many questions of ancient history and philology. These despised or neglected records are found in all sorts of likely and unlikely places. The *Englishman* of Calcutta refers to one which has lately been recovered from obscurity, and which is just a thousand years old. It was found incised on a stone slab partly fixed in the wall of a house and used as a seat, in the bazaar of Pahoa or Pihewa, in the Umballa district. Considerable difficulty was experienced in inducing the owner of the house to allow the stone to be removed, but the treasure was eventually acquired, and now lies in the Lahore Museum. The inscription consists of twenty-one stanzas of Sanskrit verse, and is an account of the building and endowment of a temple of Vishnu, together with a eulogy of the family who performed the meritorious deed. Regarding one of the brothers we are told that "when suppliants with rapture looked on his lotus face their mental anxiety completely vanished in an instant; and the crowd of hostile trumpeting elephants always shook before him in battle ready to disperse." This may be taken as a characteristic Oriental rendering of the sentiment of the familiar Scotch song, "His step is first in peaceful hall, His sword in battle keen." For extravagance of laudation, however, a higher place must be given to an inscription found near Jubbulpore, in which it is said of a certain king, that although the tread of his armies roused the apprehension of the three worlds—heaven, earth, and hell—yet there was no dust raised, as the road was flooded by the tears of the captive women who followed in his train.

THE *Photographic News* concludes an interesting article on the photographing of clouds with the following suggestions, which are offered for the benefit of those who have not had much experience in making cloud negatives:—If the sun is to be included in the picture, films or ground-glass backed plates should be used. Any lens which will take a good landscape can be used, and its smallest stop should be employed. As a rule, the exposure will be about one second on a slow plate, but in the case of red sunrises and sunsets, this may often be increased to as much as eight or even ten seconds unless isochromatic plates are available. The development must be very carefully watched, and not carried too far.

COUNTLESS swarms of rats periodically make their appearance in the bush country of the South Island, New Zealand. They invariably come in the spring, and apparently periods of about four years intervene between their visits. In a paper published in the new volume of the Transactions and Proceedings of the New Zealand Institute, Mr. Joseph Rutland brings together some interesting notes on the bush rat (*Mus maorium*). In size and general appearance it differs much from the common brown rat. The average weight of full-grown specimens is about 2 ounces. The fur on the upper portions of the body is dark brown, inclining to black; on the lower portions white or greyish-white. The head is shorter, the snout less sharp, and the countenance less fierce than in the brown species. On the open ground bush rats move comparatively slowly, evidently finding much difficulty in surmounting clods and other impediments; hence they are easily taken and destroyed. In running they do not arch the back as much as the brown rat. This awkwardness on the ground is at once exchanged for extreme activity when they climb trees. These they ascend with the nimbleness of flies, running out to the very extremities of the branches with amazing quickness; hence, when pursued, they invariably make for trees if any are within reach. The instinct which impels them to seek safety by leaving the ground is evidently strong. A rat, on being disturbed by a plough, ran for a while before the moving implement, and then up the horse-reins, which were dragging along the ground. Another peculiarity of these animals is that when suddenly startled or pursued they cry out with fear, thus betraying their whereabouts, an indiscretion of which the common rat is never guilty.

IN a paper recently read before the Vienna Academy, Herren Elster and Geitel give the results of a year and a half's observations of atmospheric electricity on the north side of Wolfenbüttel (bordering an extensive meadow). They used a stand carrying a petroleum flame and connected by insulated wire with an electroscope. A marked difference was found in the phenomena of spring, summer, and autumn, on the one hand, and winter on the other. In the former the daily variation of the fall of potential showed a distinct maximum between 8 and 9 a.m., as Exner found at St. Gilgen, and a distinct minimum between 5 and 6 p.m., whereas Exner found a maximum about 6. In winter there is great irregularity; but a weak minimum occurs about 11 a.m., and a more decided maximum about 7 p.m. It appears to the authors that other factors than humidity, with which Exner seeks to explain the variations, are concerned in the case. When the temperature goes below zero, cold mist being then generally present, there is often rather a sharp rise in the values, the aqueous vapour having then less action. Rainfall in a neighbouring region lowers the fall of potential both in winter and summer, and a disturbance of the normal course will announce a coming change in places still unclouded. Snow, it seems, rather raises the values. It has been shown by Linss that the course of the fall of potential is inversely as the coefficient of dispersion of the air for electricity; which, again, depends not only on the dust and aqueous vapour present, but also, according to Arrhenius's theory, on a sort of electrolytic or dissociative action of the sun's rays on the atmosphere (thus it has been shown that electricity escapes from a conductor under the influence of ultra-violet rays). The authors find their results support this latter view. They consider that the electric processes during formation of precipitates are the chief cause of the disturbance of the normal condition.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss White; a Common Fox (*Canis vulpes*), British Isles, presented by Mr. H. Fane Gladwin; a —— Fox (*Canis* ——) from Buenos Ayres, presented by Mr. J. R. Bell; a Common Otter (*Lutra vulgaris*), British Isles, presented by Mr. W. Corbet; a Punjab Wild Sheep (*Ovis cycloceros*) from India, presented by Dr. W. King; two European Scops Owls (*Scops gui*) from Austria, presented by Mr. Edward R. Divett; six Prussian Carp (*Carassius vulgaris*), British fresh waters, presented by Mr. G. S. Godden; three Pochards (*Fuligula ferina*), Europe, purchased; a Yak (*Poëphagus grunniens*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), and three Cambayan Turtle-doves (*Turtur senegalensis*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on August 28 = 20h. 28m. 16s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4572	—	—	20 17 28	+19 45
(2) P Cygni	Var.	Yellow.	20 13 44	+37 41
(3) D.M. + 17° 4370 ...	7	Reddish-yellow.	20 33 4	+17 53
(4) ε Delphini	4	Yellowish-white.	20 28 0	+10 56
(5) α Delphini	3·5	White.	20 34 30	+15 31
(6) 228 Schj.	7	Reddish-yellow.	19 28 1	−16 34
(7) X Ophiuchi	Var.	Red.	18 33 7	+ 8 44

Remarks.

(1) This is a fine though small planetary nebula, with four minute stars in close proximity. Lord Rosse described it as "a beautiful little spiral." In 1866 Dr. Huggins observed the spectrum of the nebula, and recorded:—"The spectrum of this nebula consisted of one bright nebulous line of the same refrangibility as the brightest of the lines of nitrogen. No other line was certainly seen." It is evident that this observation has an important bearing on the character of the chief line of the nebula spectrum, and it would be well if some other observer would take the trouble to reobserve it. It should be noted also whether the nebulosity is limited to one side of the line, or is equally visible on both sides.

(2) In 1600, this appeared as a bright star, but it has since been comparatively faint. It is especially interesting from the fact that its spectrum contains bright lines, chief amongst these being the lines of hydrogen and D_3. The Henry Draper Memorial photograph, however, shows in addition a very bright line near λ 447, which Mr. Lockyer suggests, from its association with D_3, is Lorenzoni's *f* of the chromosphere spectrum. It will be remembered that this line occurs also in the spectrum of the Orion nebula. Hence, the lines in the visible part of the spectrum which are common to P Cygni and the Orion nebula are hydrogen (F and G), D_3, and 447 (*f*); another bright line in the violet is also common to the photographs of the two spectra. This similarity is evidently in favour of the view that stars with bright line spectra are similar in constitution to nebulæ, though they are probably more condensed. It will be an interesting inquiry to see if P Cygni has anything more in common with nebulæ in the visible spectrum.

(3) According to the records of the spectrum of this star, it is one of the finest of Group II. Dunér calls it superb, all the bands 2–9 being extremely wide and dark. It affords a good opportunity for further observations of the peculiarities of this class of spectrum.

(4) Gothard states the spectrum of this star as ? II.*a*, whilst Vogel writes it I.*a* (Group IV.). My own observations confirm Vogel's statement, the spectrum being almost like that of α Lyræ.

(5) A star of Group IV.

(6) This is a star of Group VI., showing the secondary bands 2, 3, 4, and 5 (all in the red and yellow) in addition to the bands of carbon. The star is not so red as most of the members of the group, and this is no doubt due to the absorption of red light by the secondary bands. Other details should be looked for.

(7) This Group II. star will reach a maximum about September 5, and the appearance of bright lines, as in other variables of the same type, may be expected. The period is about 300 days, and the magnitude changes from 6·8 to 9.

A. FOWLER.

OBSERVATIONS OF SATURN AT THE DISAPPEARANCE OF THE RING.—In a memoir "Sur la variabilité des anneaux de Saturne," published in the *Bulletin Astronomique* (vol. ii. p. 28), M. E. L. Trouvelot touches on some interesting phenomena that he observed in 1877–78, before, during, and after the passage of the sun and earth across the plane of Saturn's rings. On May 18, 1877, M. Trouvelot remarked that the illuminated surface of the ring appeared notably less luminous than the planet; further observations confirmed this, and left no doubt that its relative light diminished up to the passage of the sun across its plane. It was also observed that the colour of the light of the ring appeared yellowish and slightly orange when compared with that of the planet, whereas observations made between 1872–76 indicated that the planet was of a yellowish colour when compared with the ring. The two sets of observations are thus diametrically opposed to each other; and it appears that, when the height of the sun above the plane of the ring is reduced to 4° 30′, the surface of the latter gradually diminishes in light with the approach of the sun to the plane, and afterwards the opposite surface increases in light intensity until the angular distance of the sun from the plane of the ring is again 4° 30′. The cause of this diminution and increase is not well known. It may be due to the change in the angle of incidence of the sun's rays, and, therefore, in the amount of light reflected or to the absorption of the sun's rays by the atmosphere belonging to the rings, or to many other causes.

From October 6, 1877, when the sun was 1° 49′ north of the plane of the rings, to February 6, 1878, when the sun crossed the plane, the illuminated surface gradually decreased in width until it appeared as a thin line difficult to recognize, because of its extreme tenuity. It was observed that the decrease in the width of the illuminated ring appeared to be produced by a shadow slowly obscuring it, and M. Trouvelot attributes the shadow to the existence of a zone elevated above the general level of the ring and slightly inclined towards the planet. To produce the observed phenomenon, a protuberant zone on the ring B, and 6000 kilometres from its outer edge, would have to have an elevation of about 400 kilometres above the plane of the rings: that is, if the north and south surfaces are symmetrical, the thickness of the zone would be 800 kilometres. In consequence, however, of the position of the zone on the ring B, and 25,600 kilometres from the edge of A, the better half of it is generally invisible, hence in practice the thickness may be said to be 400 kilometres, or nearly 249 miles.

Prof. A. Hall has a short note on "The Thickness of Saturn's Ring," in the *Astronomical Journal*, No. 222, and develops the equation by means of which it may be determined. He also notes that Dusejour gives a value equivalent to 958 English miles in his "Traité Analytique," t. ii. p. 127 (Paris, 1789), as the result of a discussion of the disappearances and reappearances of the ring observed before 1789. Herschel, by comparing the thickness of the ring with the apparent diameters of the satellites, found the value 856 miles (Phil. Trans., vol. lxxx. pp. 6 and 7, 1790).

Schroeter found the value of 539 English miles from measurements of the width of the trace of the ring on the ball of the planet ("Kronographische Fragmente," pp. 157 and 211, Göttingen, 1808).

W. C. and G. P. Bond by comparing the amount of light received from the surface of the ring a short time before its disappearance with the light received from *the edge of the ring* found the value <43 miles.

With respect to this latter value M. Trouvelot remarks: "Mais Bond, qui ignorait que le système des anneaux de Saturne n'est pas plan, et que c'est à une assez grand distance de son bord extérieur qu'il atteint son maximum d'épaisseur, ne pouvait arriver qu'à une évaluation erronée et trop petite de cette épaisseur."

Several other points are touched upon in M. Trouvelot's memoir, viz. that Cassini's division appeared more visible on the eastern side of the planet than on the western, when the elevation of the sun above the plane of the ring was between +0° 45′ and +0° 27′; and that the edge of Saturn, like that of Jupiter, was notably more luminous than other parts of the globe. The difference in outline between the preceding and following parts of the ring, the deformation of the limb of the planet at different dates, and many observations possible during the disappearance of the ring are also considered.

The memoir concludes with some remarks and suggestions on the observations that should be made during 1891–92. The next disappearance of the ring is on September 22, 1891, and it will reappear on October 30 of the same year. Again, in May 1892, Saturn will be well situated for observations on the structure of the rings causing the shadow noticed in 1877–78. It is to be hoped therefore that the increased powers now at our disposal will enable many of the questions raised by M. Trouvelot to be definitely settled.

OBJECTS HAVING PECULIAR SPECTRA.—In *Astronomische Nachrichten*, No. 2986, Prof. E. C. Pickering, Director of Harvard College Observatory, notes that an examination of photographs taken during March and April 1890, by Mr. S. J. Bailey, near Closica, Peru, has led to the discovery of some interesting spectra.

A photograph of the spectrum of R Carinæ (R.A. 9h. 29·7m., Decl. −62° 21′, 1900) taken on April 4, 1890, shows that the G and *h* lines due to hydrogen are bright, as in Mira Ceti and other variables of long period.

Two photographs taken on March 19 and 20, 1890, of the star, Cordova General Catalogue, No. 15,177, magnitude 8½ (R.A. 11h. 0·8m., Decl. −65° 1′, 1900), show that it has a spectrum consisting mainly of bright lines, and similar to that of Wolf and Rayet's three stars in Cygnus.

The nebula, General Catalogue, No. 2581 (R.A. 11h. 45·1m., Decl. −56° 29′, 1900) has the same spectrum as General Catalogue 4628. Both these objects show bright lines in the ultra-violet portion of their spectra, which have not been discovered in any other planetary nebulæ.

D.M. +30° 3699, magnitude 9·3 (R.A. 19h. 31·9m., Decl. +30° 19′, 1900), is seen to have bright lines in its spectrum on photographs taken at Cambridge with the 8-inch Draper telescope on June 18, 23, and 25, 1890. The spectrum of this star differs from that of other bright-line spectra of which photographs have been obtained.

ON THE CAPTURE OF YOUNG (IMMATURE) FISHES, AND WHAT CONSTITUTES AN IMMATURE FISH.

SINCE steam-trawling became prominent, frequent complaints of the constant and great destruction of very young fishes by this mode of fishing have been made; indeed, besides the injury to the so-called eggs of food-fishes—then said to be deposited on the bottom—no subject attracted more attention in the Royal Commission of 1883–84—presided over by Lord Dalhousie. Recently the subject has again been urged before the National Sea Fisheries Protection Association—especially by the fish-merchants of London (on the alleged grounds of the diminution in size of the valuable food-fishes)—and, with the assistance of the Board of Trade, an International Conference, to discuss remedial measures "to be taken for the preservation and development of the fisheries in the extra-territorial waters of Europe," was convened in the Fishmongers' Hall. It would be a misapprehension, however, to suppose that those who attended the Conference confined their attention to extra-territorial waters, since the inshore ground (within the three-mile limit) is really more important, *e.g.* in regard to the preservation of certain flat fishes, than the offshore. Thus, as formerly shown, the plaice for the most part passes its early life in the shallow sandy bays of the inshore, and as it attains a length of about fifteen inches it in most cases frequents the deeper water offshore, where it chiefly spawns, the pelagic ova and larvæ being carried shorewards to repeat the process. In the same way multitudes of small turbot, brill, and soles pass their early existence not far from low-water mark on sandy beaches; ling in the barred condition amongst rocks at extreme low-water; and cod, coalfish, pollack, and whiting, near the same regions. Remedial measures therefore, applied, for instance, to the plaice in extra-territorial waters, could only affect the adult or nearly adult fishes, and mainly in regard to the spawning individuals, a point no doubt of vital importance, but which nevertheless does not touch the question before us, viz. the young or immature fishes.

In most modes of fishing as at present followed, young or immature fishes are captured. Thus, in line-fishing a considerable number are hooked throughout the year, and in certain parts of the east coast many in September and October. When we consider the large number of men engaged in line-fishing, and the almost constant nature of such captures, we cannot conscientiously overlook it. The drain on the young cod, haddock,

whiting, ling, and other fishes is a steady one, and though many are replaced in the sea by the fishermen it is doubtful if they will survive. The smaller of the first three, indeed, are generally dead when brought on board. The use of the hook, on the other hand, for the capture of flat fishes—more particularly plaice in sandy bays—contrasts favourably with the work of sailing trawlers on the same ground, since a larger size of fish on the whole is secured: though scarcely a single fish thus obtained is mature. It is probable that the smaller mouth of the pleuronectids prevents the younger forms from so readily taking the hook, the size of which, moreover, would appear to be related to that of the young fishes captured. The liners themselves as a rule apply the remedy, since they leave the ground frequented by small fishes, *e.g.* haddocks, and seek more mature forms. They appear to be aware that these young fishes haunt the same area a considerable time. This practice cannot be too much commended.

Beam-trawling and otter-trawling, again, are credited with the capture of many young (immature) fishes. In the case of the beam-trawl, now so extensively used, if the meshes of the net be small and work carried on inshore, or where multitudes of young fishes are, large numbers especially of flat fishes are taken. In ordinary steam-trawling for profit, as observed off the east coast in 1884, however, the actual captures of small (immature) fishes were not as a rule serious. For the most part they consisted of common and long rough dabs, neither of which when adult is a large fish, though both, besides other uses, form an important item in the diet of the more valuable fishes. Off Girdleness (Aberdeenshire), however, a considerable number of young cod were captured in autumn, yet every one of these was used as food and was saleable. In the open offshore water very few young plaice are procured, almost all being of considerable size; but in inshore water, *e.g.* in such bays as St. Andrews, vast numbers of small plaice may be captured with a naturalist's trawl (*i.e.* one with a small mesh), and considerable numbers with the ordinary trawl of either sailing or steam trawler, one of the latter in 1884 having about sixty boxes as its catch. Though the very young plaice are abundant at the tidal margin, yet no graduated lines, indicating an increase in size as we proceed outwards, seem to occur, very small forms being found in the outer lines of St. Andrews Bay as well as those approaching low-water mark.[1]

In steam-trawling for profit, the condition of the captured young fishes depends on the length of time the trawl has been down, the state of the sea, and the condition of the bottom. Thus, if the trawl has been at work for five hours the younger fishes are often dead, and, if not, would probably die if replaced in the water, whereas when the trawl of a sailing boat has been down only an hour the majority would probably live if returned to the sea. If the sea be rough, the pitching of the vessel in hauling causes the bag of the net and its load of fishes to strike the side of the ship, and thus the snouts of the fishes are broken and many killed. In the same way soft muddy ground is fatal to the fishes in the trawl, just as in a less degree, the soft sand of the beach proves destructive to trout swept down by a spate.

Shrimp-trawling is another method of fishing proportionally more destructive to young fishes than perhaps any other. As carried on, for instance, in the estuary of the Thames by sailing boats near Canvey Island and towards Tilbury Fort, multitudes of small soles, dabs, plaice, bib, whiting, and other forms, *e.g.* unctuous suckers and *Cotti*, are retained by the small-meshed net, and before the sifting of the shrimps is concluded the majority have succumbed. Nor are hand-nets and the larger ones drawn by horses less destructive. All cause a frequent and great drain on the young fishes, especially in some places on such valuable forms as soles, turbot, and brill, while the food procured for the public is small in comparison with the loss of fish-food. There should be no insuperable obstacle to the immediate substitution of these methods by others less wasteful to fish-life. The French shrimp-trap, for instance, as recommended by Prof. Giard and M. Roussin, is a step in the right direction.

The use of the "sweep-net" on sandy shores for procuring sand-eels is followed by the capture of numerous young cod, green cod, gurnards, whiting, trout, turbot, brill, dabs, plaice, flounders, and other forms. The net has wings of 4-inch mesh, and a centre of strong netted curtain-gauze, so that small fishes are secured in hundreds. The net is worked by two men, one in a boat, the other on shore, and is especially destructive in estuaries. The little fishes thus captured escape the trawls of both sailing and steam-vessels.

[1] This appears to differ from the results of the *Garland's* recent work.

The salmon-stake nets, on sandy beaches, secure many small turbot and brill from $5\frac{1}{2}$ inches upwards.

The stow or bag-net for sprats, as used by yawls in estuaries of rivers, is a small-meshed net of great length, fixed to the side of the vessel by the upper beam, and into which immense numbers of young herrings and sprats, and sometimes many sparlings, are swept by the current, besides various round food-fishes, flat fishes, and unsaleable forms, such as *Cotti*, Montagu's suckers, and pipe-fishes, not to allude to an occasional salmon. The captured fishes are now and then used for manure, and much valuable food is thus lost to the community.

The small-meshed sprat-nets (pole-nets) of the Forth are also responsible for great captures of small herrings and sprats for manure, as well as for the destruction of young round fishes, such as cod and whiting. The capture of whitebait in the Thames is another instance of the wholesale destruction of very young fishes.

From the foregoing brief sketch it will be apparent that no special kind of fishing is responsible for the capture of small (immature) fishes, and that legislative measures, to be effectual, must, more or less, cover all. The question, therefore, is beset with difficulties. The prohibition of the landing and sale of such fishes would, of course, shut them out of the market, but it would not prevent their being captured; and while they might be returned to the water as soon as practicable, the mortality, as already indicated, would be considerable. It is difficult to see how, by any modification of apparatus, these small fishes would be enabled to escape capture by liners, trawlers, shrimpers, seine, and other net-fishermen. As recommended to the Trawling Commission of 1884, the mesh of the trawl-net might be enlarged. Thus, for 9 feet at the cod-end, it might have a 2-inch mesh; then, for 12 feet, $2\frac{1}{2}$-inch mesh; next, 3 and $3\frac{1}{2}$-inch mesh; and, finally, a 5-inch mesh towards the beam. The enlargement of the mesh will not altogether prevent the loss of young fishes, but it will diminish it. Moreover, a limit to the time the trawl is down might be considered. The pressure of the larger on the smaller fishes when the bag of the net is hoisted by the derrick, and the swinging of the heavily-laden bag on the side of the ship in rough weather, however, are elements of disaster apparently beyond control at present. If the bag of the net with its fishes could be lifted horizontally into a raft or other apparatus level with the water, much injury to the contents, both young and adult, would be avoided; but the practical difficulties are great. In the other modes of fishing in which young fishes are captured in great numbers, and where restrictive measures are inapplicable, the obstacles would seem to be best met by the modification of apparatus and by the trained intelligence of the master-fisherman.

The question as to what constitutes an immature fish has not hitherto, perhaps, received that strict attention which it merits. In the trawling investigations of 1884 the term "immature" was not used in the strictly scientific sense—that is, in connection with the reproductive organs, though these were examined in all the species. The term, indeed, was purposely employed as nearly synonymous with unsaleable. Recently, Dr. Fulton, the energetic Scientific Secretary to the Fishery Board for Scotland, has had a large number of fishes examined—chiefly by Mr. T. Scott, on board the *Garland*—their sizes and the condition of the reproductive organs carefully noted, and the results, as elaborated by him, are given in a paper about to be issued by the Fishery Board in their Blue-book for 1890.[1] The paper is one of very great interest, and there can be little doubt that the term "immature" ought to be restricted to fishes that have never spawned; and it may thus happen that such may be saleable, *e.g.* in the case of the plaice, brill, turbot, cod, and others. On the other hand, mature food-fishes may be unsaleable from their small size, as in the case of the flounder, dab, and long rough dab, though, as already mentioned, these are important as the food of some of our most valuable fishes. As given by Dr. Fulton the smallest ripe food-fishes procured in the *Garland's* trawl were as follows:—Plaice 12 inches, lemon-dab 8 inches, dab 6 inches, long rough dab 6 inches, flounder 7 inches, craig-fluke (witch) 14 inches, turbot 18 inches, brill 16 inches, sail-fluke 9 inches, haddock 10 inches, whiting 8 inches, cod 20 inches, gurnard 8 inches, and catfish

[1] I have to thank Dr. Fulton for an early proof, issued, by the sanction of the Secretary for Scotland and the Fishery Board, in connection with the International Conference.

(*Anarrhichas*) 20 inches (?). In most cases these small specimens were males, as in the even more remarkable case of the salmon—in which the milt of a parr a few inches long can be utilized for the successful fertilization of the ova of an adult female salmon. There would therefore be grounds for saying that fishes of a less size than the foregoing are immature. From these observations it will be seen that the minimum size of 12 inches for turbot and brill—adopted by the representatives of the sea-fishing industry of the United Kingdom in June of this year—does not err on the side of excess. Further, since the mature males are often so much smaller than the females, it is apparent that the same restrictive size would not be practicable, though the numbers of the mature females are of greater importance for the welfare of the fisheries than those of the males.

While, therefore, many difficulties beset legislative measures for the preservation of the young fishes, there need be no halt in the efforts of the fishery authorities in investigating the deep-sea fishing grounds far from shore; and this should be carried out as far as practicable and as frequently as possible every month of the year. A comparison of the surface, mid-water, and bottom fauna there with that already known to exist in such bays as St. Andrews could not fail to give valuable and interesting data. Besides, the gaps in the life-histories of the post-larval and young stages of many fishes would thus be more or less bridged over. Finally, there can be little doubt of the expediency of at once providing suitable open-air tanks, *e.g.* at St. Andrews, in communication with the tidal water for the study of the post-larval and young stages of food-fishes, especially with regard to their rate of growth. It has yet to be proved also whether it would be best to place the larvæ of valuable fishes, such as turbot, brill, and soles, in the sea, or to keep them till the post-larval or young stages are reached.

W. C. McIntosh.

ESTABLISHMENT OF SCIENCE SCHOLARSHIPS.

We have already called attention to the science scholarships which are being established by the Commissioners for the Exhibition of 1851. The official statement on the subject is as follows:—

In their seventh report to the Crown, presented in July 1889, the Commissioners for the Exhibition of 1851 announced their intention of appropriating an annual sum of not less than £5000 a year to the establishment of scholarships, to enable the most promising students in provincial colleges of science to complete their studies either in those colleges or in the larger institutions of the metropolis, care being taken that these scholarships should be a supplement to, and not in competition with, scholarships already existing through either public action or private endowment.

The decision to restrict the scheme of scholarships to provincial colleges was due to the feeling of the Commissioners that the provinces, which took so large a part in supporting the Great Exhibition of 1851, had a just claim to receive as direct a benefit from the funds derived from that Exhibition as is afforded to the institutions on the Commissioners' Estate at South Kensington, which, although unquestionably of national importance, confer a more immediate advantage on the metropolis.

To assist them in preparing a scheme for the distribution and regulation of the scholarships the Commissioners obtained the services of a committee of eminent men of science—namely, Prof. Garnett, Prof. Huxley, Prof. Norman Lockyer, Sir Henry Roscoe, and Sir William Thomson. To these were added two Commissioners, Mr. Mundella and Sir Lyon Playfair, the latter of whom acted as chairman of the committee.

On the 18th of June last this committee presented the accompanying report on the scope and objects of the scholarships, and it has been adopted by the Commissioners.

The committee then considered the manner in which the scholarships should be distributed. On this point they were bound by the restriction of the present scheme to students in provincial institutions, in which term, however, they suggested that colonial Universities might be comprised. They thought it unnecessary to include in the scheme the Universities of Oxford, Cambridge, and Dublin, in view of the large endowments of those bodies. The committee decided upon the allotment of an annual series of seventeen scholarships in the manner shown by the accompanying list, and the institutions named in the list will be invited to nominate scholars, subject to the conditions laid down in the report of the committee, and provided that they possess scholars worthy of the purposes explained in it.

The present allotment is to be considered experimental and temporary, and the selection now made of institutions to which nominations are offered will be subject to modification in the future, having regard not only to the manner in which the nominations are exercised, but also to the claims of other Universities and colleges which may from time to time be brought under the consideration of the Commissioners.

Provincial and Colonial Universities and Colleges.

Scholarships Annually.		
	1	University of Edinburgh.
	1	University of Glasgow.
Alternately .	1	University of St. Andrews (including University College, Dundee). University of Aberdeen.
	1	Mason College of Science, Birmingham.
	1	Bristol University College.
	1	Durham College of Science, Newcastle.
	1	Yorkshire College of Science, Leeds.
	1	Liverpool University College.
	1	Owens College, Manchester.
	1	Nottingham University College.
	1	Firth College, Sheffield.
Alternately .	1	Aberystwith (University College of Wales). Bangor (University College of North Wales). Cardiff (University College of South Wales).
In each year	2	Belfast, Queen's College. Cork, Queen's College. Galway, Queen's College. Dublin, Royal College of Science.
		Canada:—
Alternately .	1	M'Gill College, Montreal. University of Toronto.
		Australia:—
In each year	2	University of Sydney. University of Melbourne. University of Adelaide. University of New Zealand.
	17	

The following is the first report of the committee for considering the regulation and distribution of the science scholarships:—

The committee have had their attention drawn to the fact that there is a large number of scholarships in the country; that they are increasing at a rapid rate; and, if the Commissioners act on the same lines as those already occupied, it is possible that education will gain little by their action, as the endowment of the Commissioners may interfere with the establishment of new scholarships by private liberality.

Hence it is desirable that the scholarships with which this committee have to deal should be of a higher order than most of those now existing; in fact, their functions should begin where the ordinary educational curriculum ends. This system has been adopted with excellent effects by the French *École pratique des hautes études*.

The committee propose:—(1) That the scholarships shall be of £150 a year in value, and shall be tenable for two years, but in rare instances may be extended to three years by special resolution of the Commissioners. The continuation, each year after the first, shall depend upon the work done in the previous year being satisfactory to the scientific committee which it is suggested shall be appointed by the Commissioners.

(2) That the scholarships shall be limited to those branches of science (such as physics, mechanics, and chemistry) the extension of which is specially important for our national industries.

(3) That the Commissioners shall from time to time select a certain number of provincial and colonial colleges in which special attention is given to scientific education, and give to each

the power of nominating a student of not less than three years' standing to a scholarship, on the condition that he indicates high promise of capacity for advancing science or its applications.

(4) That the Commissioners shall appoint a committee of advice, who will consider and report upon the reasons for which the nominations are made by the respective colleges, and the Commissioners will appoint to the scholarships upon the report of their committee.

(5) That the scholarships when awarded shall be tenable in any University either at home or abroad, or in some other institution to be approved of by the Commissioners. The holder of a scholarship must give an undertaking that he will wholly devote himself to the object of the scholarship, and that he will not hold any position of emolument during its continuance.

Lyon Playfair, Chairman.
Wm. Garnett.
T. H. Huxley.
J. Norman Lockyer.
A. J. Mundella.
Henry E. Roscoe.
William Thomson.

SCIENTIFIC SERIALS.

American Journal of Science, August 1890.—On the cheapest form of light, from studies at the Allegheny Observatory, by S. P. Langley and F. W. Very. The authors have made a long and interesting series of observations, by means of the bolometer and spectroscope, on the light radiated by the fire-fly (*Pyrophorus noctilucus*, Linn.) found in Cuba and elsewhere. It has been previously shown that in all industrial methods of producing light, like the candle, lamp, or gas, more than 99 per cent. of the energy is, as far as illumination goes, wasted; and in sources of higher temperature, like the incandescent light and electric arc, even under the most favourable conditions, an enormous waste is involved. The study of the radiation of the fire-fly demonstrates that it is possible to produce light without heat other than in the light itself; that this is actually effected by nature's processes; and that these are "cheaper," that is, more economical in energy, than any industrial method now known. From the observations and facts given there seems no reason why the light should not one day be produced in the laboratory, and used for industrial purposes.—Contributions to mineralogy, No. 48, by F. A. Genth. Analyses are given of the following minerals: tetradymite, pyrite, quartz (pseudomorphous after stibnite), gold in chromiferous clay from Los Cerillos, New Mexico, zircon, scapolite, garnet, titaniferous garnet, allanite, and lettsomite from Arizona and Utah.—A curious occurrence of vivianite, by Wm. L. Dudley.—Classification of the glacial sediments of Maine, by George H. Stone.—The direct determination of bromine in mixtures of alkaline bromides and iodides, by F. A. Gooch and J. R. Ensign. The method described is as follows: the neutral solution containing the bromide and iodine is diluted to 600 c.c. or 700 c.c., and about 1 c.c. or 1·5 c.c. of strong sulphuric acid, or from 2 c.c. to 3 c.c. of the acid mixed with an equal volume of water, are added; a sufficient amount of sodium or potassium nitrite is then introduced, and the liquid is boiled until the colour has disappeared and the escaping steam no longer gives to red litmus-paper the characteristic colour of iodine. The residual liquid is then treated with excess of silver nitrate, and the precipitated bromide filtered off, dried, and weighed.—Some Lower Silurian graptolites from Northern Maine, by W. W. Dodge.—Siderite-basins of the Hudson River epoch, by James P. Kimball. Some interesting facts bearing on the structural geology of the Taconic area extending to the Hudson River, and on the geology of the whole Taconic region, are brought together and discussed.—On a new variety of zinc sulphide from Cherokee County, Kansas, by James D. Robertson.—Two new meteoric irons, by F. P. Venable. An analysis of a meteorite from Rockingham County, N.C., gave the result: Fe, 87·01; P, 0·04; SiO_2, 0·53; Cl, 0·39; Ni, 11·69; Co, 0·79 = 100·45. Another meteoric iron from Henry County, Vancouver, gave: Fe, 90·54; Cl, 0·35; SiO_2, 0·04; P, 0·13; Co, 0·94; Ni, 7·70 = 99·70.—Notice of some extinct Testudinata, by O. C. Marsh.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, August 18.—M. Duchartre in the chair.—Contribution to the theory of the experiments of M. Hertz, by M. H. Poincaré. After pointing out an error in the calculations of M. Hertz, an attempt is made, starting with Maxwell's fundamental hypotheses, to develop a more rigorous formula for the rate of propagation of the wave in air.—International meteorological tables, presented by M. Mascart. These are of the form finally decided upon by the International Committee at Zurich, 1888.—Order of appearance of the first vessels in the flowers of some *Tragopogon* and *Scorzonera*, by M. A. Trécul.—Experimental tuberculosis, on a mode of treatment and of vaccination, by MM. J. Grancher and H. Martin. The paper describes the result of some experiments on the inoculation of rabbits. The process described affords a more or less complete protection from tuberculosis to the rabbits inoculated.—On a portable electrical safety-lamp, for use in mines, by M. G. Trouvé. The smallest lamp described, supplied with six Planté accumulators (weight 420 grammes), runs for five hours.—Note on a theorem concerning life annuities, by M. A. Quiquet.—Experiments on transversal magnetization by magnets, by M. C. Decharme.—On an electric lighting-apparatus, for examining the strata in borings, by M. G. Trouvé.—Allyl-cyano-succinic ether; new synthesis by means of cyano-succinic ether, by M. L. Barthe. In this synthesis sodium-cyano-succinic ether is treated in alcoholic solution with allyl iodide. The new ether forms a colourless oil, and distils under 35 mm. pressure at 207°–210°.—Methyl cyano-succinate, and methyl cyano-tri-carballylate, by M. L. Barthe. Both these ethers are produced when sodium-methyl cyanacetate is treated with methyl chloracetate.—Researches on butter and margarine, by M. C. Viollette. This paper contains results of ten complete analyses. By the method given an adulteration of pure butter with ten per cent. of margarine can be detected.—Researches on the optical analysis of butters, by the same. The differences between the values of the refractive indices for pure butter and for margarine are sufficient to serve as the basis of an analytical method.—On a characteristic reaction of cocaine, by M. F. da Silva.

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THURSDAY, SEPTEMBER 4, 1890.

THE BRITISH ASSOCIATION.

LEEDS, *Tuesday Evening*.

IT is 32 years since the British Association met at Leeds, this being only its second visit, though it has frequently enough held its meetings in Yorkshire during that period. The President of 1858, Sir Richard Owen, happily still survives, though most of his eminent colleagues of the year have quitted the field—Sir John Herschel, Sir David Brewster, Sir Roderick Murchison Sir Benjamin Brodie, Robert Stephenson, Phillips, Darwin, Nasmyth. But the names of Huxley, Francis Galton, and others then coming into prominence, are found among the list of those who contributed to the proceedings of the meeting. Vast changes have taken place both in Leeds and in science during these 32 years—changes which we need not record here. Leeds during the interval has risen to its present prominence and prosperity mainly through the application of the discoveries of science, and therefore it seems appropriate that this year's President should be one who himself during 30 years has had so much to do with the application of these discoveries. As will be seen, Sir Frederick Abel's address deals largely with his own work in this direction.

So far as can be judged at present, the local authorities have done everything they could to render the meeting a success. All the best buildings in the town have been placed at the disposal of the Association. The Reception Room is spacious and handsome, and, so far as I have seen, the Sectional rooms are all that could be desired. The most thoughtful care has been taken for the comfort and convenience of the visitors. The Sectional Secretaries have seldom been so well housed. Their hotel is just opposite the Reception Room, and not only have well-furnished writing and dining rooms been placed at their disposal, but a handsome billiard room with three tables, on which the Secretaries of Section A hope to work out some abstruse problems. The Guide-book to Leeds, which has been prepared under the editorship of Prof. Miall, is well packed with useful information and guidance, and is handy enough to go into the pocket. A small pamphlet gives full details as to all the arrangements of the meeting, and another all the information about excursions.

As to the general work of the Sections, it is probable that it will be up to the average. So far as I can learn, there is no paper as yet of striking or sensational importance. There will, however, be several discussions that are likely to have useful results. In Section A, for example, there will be a discussion on electrical units, and possibly another on mechanical units and nomenclature. In Section D, again, there will be a discussion on the teaching of botany, in connection with which papers will be read by Dr. F. Oliver, Dr. Scott, and Prof. Marshall Ward, and in which Mr. F. Darwin, Prof. Bower, and others, will take part. A joint meeting will be held, on Monday, of Sections E and F, for the consideration of the important subject of the lands of the globe still available for settlement. One of the most striking papers in Section G will be one by Mr. Read Murphy (a native of Australia), on the Victorian torpedo. The Anthropological Laboratory will be at work this year again, though unfortunately it has been located at a distance from the meeting-room of Section H. Public and private hospitality, it need hardly be said, will be lavish.

INAUGURAL ADDRESS BY SIR FREDERICK AUGUSTUS ABEL, C.B., D.C.L. (OXON.), D.Sc. (CANT.), F.R.S., P.P.C.S., HON. M. INST. C.E., PRESIDENT.

MANY who had the pleasure of listening last year, at Newcastle, to the interesting and instructive address of the President to whom I am a most unworthy successor, could not fail, both by the chief subject of his discourse, and by the circumstance of the official position which he occupies with so much benefit to science and the public, to have their thoughts directed to the illustrious naturalist whose philosophical address delighted the members of the Association and the people of Leeds thirty-two years ago.

More than one-half the period of existence of this Association has passed since Richard Owen presided over its meeting in this town. Alas! what gaps have been created in the ranks of those who then were prominent for activity in advancing its work: the then General Secretary, Sir Edward Sabine; the all-popular Assistant-General Secretary, John Phillips; the Treasurer, John Taylor, now live with us only through their works and the enduring esteem which they inspired. But very few of those who held other prominent positions at that meeting have survived to see the Association reassemble in this town. Whewell, Herschel, Hopkins, the elder Brodie, Murchison, William Fairbairn, all Presidents of Sections in 1858, have long since been removed from among us; and the then President of Section F, Edward Baines, a much-honoured and highly-talented son of the "Franklin of Leeds," whom we had hoped to count among those Vice-Presidents representing the city on this occasion, has recently passed away, in his ninetieth year, after a most honourable and useful career, during which he especially distinguished himself by his successful exertions in the advancement of the great educational movements of his time.

The illustrious President of our last meeting here, concerning whose health the gravest apprehensions were not long since entertained, is happily still preserved to us; still intellectually bright at the ripe age of eighty-six, and still, with the keen pleasure of his early life, following the progress of those branches of scientific research which have constituted the favourite occupations and the arena of many intellectual triumphs of a long career of ardent and successful devotion to the advancement of science.

To not a few of those who have flocked to Leeds to attend the annual gathering of this Association, our present meeting-place is doubtless known chiefly by its proud position as one of the most thriving manufacturing towns of the United Kingdom; of ancient renown, especially in connection with one of the chief industries identified with Great Britain in years past. But this good town of Leeds, whose cloth market was described by Daniel Defoe, one hundred and sixty odd years ago, as "a prodigy of its kind, and not to be equalled in the world," and whose present position in connection with divers of our great industries would have equally excited the enthusiasm of that graphic writer, is famous for other things than its prominent association with manufactures and commerce.

Not many of our great industrial centres can boast of so goodly an array, upon the scroll of their past history, of names of men eminent in the Sciences, the Arts and Manufactures, in Divinity and Letters, and in heroic achievements, such as are identified with Leeds and its immediate vicinity: Thomas, Lord Fairfax, one of the most prominent heroes of the Commonwealth; Smeaton, an intellectual giant among engineers; William Hirst and John Marshall, illustrious examples of the men who by their genius, energy, and perseverance placed Great Britain upon the pinnacle of industrial and commercial greatness which she so long occupied unassailed; Richard Bentley, the eminent classic and divine; John Nicholson, the Airedale poet; John Fowler and Peter Fairbairn, worthy followers in the footsteps of Smeaton; Isaac Milner, weaver and mathematician, afterwards Senior Wrangler, Smith's Prize-man, Jacksonian Professor, President of Queen's College, Vice-Chancellor of Cambridge University, Dean of Carlisle, and a

most illustrious Fellow of the Royal Society; Thoresby, antiquarian and topographer; Benjamin Wilson, painter, and industrious contributor to the development of electrical science; William Hey, the eminent surgeon, and friend and counsellor of Priestley; Sadler, political economist and philanthropist; the brothers Sheepshanks—Richard, the astronomer, and John, the accomplished patron of the arts, and munificent contributor to our national art treasures; Edward Baines, whose conspicuous talents and energy developed a small provincial journal into one of the most powerful public organs of the country; his talented sons, of whom not the least conspicuous and highly respected was the late Sir Edward Baines. I might swell this voluminous list by reference to illustrious members of such families as that of Denison, of Beckett, of Lowther, but the men I have referred to fitly illustrate the remarkable array of worthies whose careers have shed lustre upon the town in or near which they were born. Yet that illustration would be altogether incomplete if I failed to speak of one whose career and works alone would suffice to place Leeds in the foremost rank of those English towns which can claim as their own men whose course of life and whose achievements have secured their pre-eminence among our illustrious countrymen. Needless to say that I refer to Joseph Priestley, born within six miles of Leeds, whose name holds rank among the foremost of successful workers in science; who, by brilliant powers of experimental investigation, rapidly achieved a series of discoveries which helped largely to dispel the shroud of mystery surrounding the art of alchemy, and to lay the foundation of true chemical science. An ardent student of the classics, of Eastern languages, and of divinity, a zealous exponent of theological doctrines which marred his career as divine and instructor, he early displayed conspicuous talents for the cultivation of experimental science, which he pursued with ardour under formidable difficulties. His acquaintance with Franklin probably developed the taste for the study of electric science which led him to labour successfully in this direction and the publication, in 1767, of his valuable work on "The History and Present State of Electricity, with Original Experiments," secured for him a prominent position among the working Fellows of the Royal Society. His connection with Mill Hill Chapel, in 1768, appears to have given rise accidentally to his first embracing the experimental pursuit of what formerly was termed pneumatic chemistry, the foundation of which had been laid by Cavendish's memorable contribution, in 1766, to the Philosophical Transactions, on carbonic acid and hydrogen. Priestley's first publication in pneumatic chemistry, on "Impregnating Water with Fixed Air" (carbonic acid), attracted great attention; it was at once translated into French, and the College of Physicians addressed the Lords of the Treasury thereon, pointing out the advantages which might result from the employment, by men at sea, of water impregnated with carbonic acid gas, as a protective against, or cure for, scurvy.

Six years later Priestley investigated the chemical effects produced on the air by the burning of candles and the respiration of animals, and, having demonstrated that it was thereby diminished in volume and deteriorated, he showed that living plants possessed the power of rendering air, which had been thus deteriorated, once more capable of supporting the combustion of a candle. At about this time Priestley received very advantageous proposals to accompany Captain Cook upon his second expedition to the South Seas; but when about to prepare for his departure he learned from Sir Joseph Banks that objections against his appointment, on account of the great latitude of his religious principles, had been successfully urged by some ecclesiastic member of the Board of Longitude. In 1773 the Royal Society awarded Priestley the Copley Medal for a remarkable paper entitled "Observations on Different Kinds of Air," and in that year he became librarian and literary companion to the Earl of Shelburne (afterwards Marquis of Lansdowne), and thereby secured special advantages in the pursuit of his scientific researches.

With respect to his departure from Leeds, he expressed himself as having been very happy there "with a liberal, friendly, and harmonious congregation, to whom my services (of which I was not sparing) were very acceptable. Here I had no unreasonable prejudices to contend with, so that I had full scope for every kind of exertion; and I can truly say that I always considered the office of a Christian minister as the most honourable of any upon earth, and in the studies proper to it I always took the greatest pleasure." During the next five years he published as many volumes describing the results of important experiments on air. After investigating the properties of nitric oxide, and applying it to the analysis of air, Priestley, in 1774, discovered and carefully studied oxygen, which he obtained by the action of heat upon the red oxide of mercury. He was the first to prepare and study sulphurous acid, carbonic oxide, nitrous oxide, hydrochloric acid (*marine acid air*), and the fluoride of silicon, and carried out important researches on the properties of hydrogen, and of other gases previously but little known. His great quickness of perception and power of experiment led him to the achievement of many novel and important results; but one cannot help contrasting his somewhat random search after new discoveries with the close logical reasoning and philosophic spirit which guided and pervaded the remarkable researches of him whose departure from amongst us since the last gathering of this Association is so universally deplored—of the great discoverer of the universal law of the conversion of energy, James Prescott Joule. I could not add to the judicious and graceful reference to his work which Sir Henry Roscoe was privileged to make, in the last year of that philosopher's valuable life, when presiding over the recent meeting of the Association in the town which gloried in numbering Joule among its citizens; but I may, perhaps, be permitted to express the sanguine hope that the desire of the scientific world to secure the establishment of an international memorial fitly commemorative of his great life-work may be realized in the most ample manner.

The wide scope of the admirable discourse delivered by Owen in this town thirty-two years ago affords an interesting illustration of the delight which men whose best energies are devoted to the cultivation of one particular branch of science take in the results of the labours of their fellow-workers in other departments, and in their achievements in contributing to the general advancement of our knowledge of Nature's laws and of their operations. It is to this bond of intimate union between all workers in pure science that we owe the instructive reviews of the progress made in different departments of science, with which we have often been presented at our annual gatherings. On the other hand, those men, from time to time selected to fill the distinguished office of President, whose lives have been mainly devoted to the practical utilization of the results of scientific research, and to the extension in particular directions of the consequent resources of civilization, seize with keen pleasure the opportunity afforded them of directing attention to the triumphs achieved in the application, to the purposes of daily life, of the great scientific truths established by such illustrious labourers in the fields of pure science as Newton, Dalton, Faraday, and Joule. The wide and constantly-extending domain of applied science presents, even to the superficial observer, a continually varied scene; not a year passes but some great prize falls to the lot of one or other of its explorers, and some apparently insignificant vein of treasure, struck upon but a few years back, is rapidly opened out by cunning explorers, until it leads to a mine of vast wealth, from which branch out in many directions new sources of power and might.

Among the branches of science in the practical applications of which the greatest strides have been made since the Association met at Leeds in 1858, is electricity. That year witnessed the accomplishment of the first great step towards the establishment of electrical communication between Europe and America, by the laying of a telegraph-cable connecting Newfoundland with Valencia. Through this cable a message of thirty-one words was shortly afterwards transmitted in thirty-five minutes; an achievement which, though exciting great enthusiasm at the time, scarcely afforded promise of the succession of triumphs of ocean-telegraphy which have since surpassed the wildest dreams of the pioneers in the realms of applied electricity.

The development of the electric telegraph constitutes a never-failing subject of the liveliest interest. The experiments made by Stephen Gray, in 1727, of transmitting electrical impulses through a wire 700 feet long; by Watson, twenty years afterwards, of transmitting frictional electricity through many thousand feet of wire, supported by a line of poles, on Shooter's Hill, in Kent; and by Franklin, who carried out a similar experiment at Philadelphia,—although they were followed by many other interesting and philosophical applications of frictional electricity to the transmission of signals—were not productive of really practical results. The work of Galvani and of Volta was more fruitful of an approach to practical telegraphy in the hands of Sömmering and of Coxe, while the researches of

Oersted, of Ampère, of Sturgeon, and of Ohm, and especially the discoveries of volta-electric induction and magneto-electricity by Faraday, paved the way for the development of the electric telegraph as a practical reality by Cooke and Wheatstone in 1837. How remarkable the strides have been in the resources and powers of the telegraphist since that time is demonstrated by a few such facts as these: the first needle-instrument of Cooke and Wheatstone transmitted messages at the rate of four words per minute, requiring five wires for that purpose; six messages are now conveyed by one wire, at ten times that speed, and news is despatched at the rate of 600 words per minute. Duplex working, which more than doubled the transmitting power of a submarine cable, was soon eclipsed by the application of Edison's quadruplex working, which has in its turn been surpassed by the multiplex system, whereby six messages may be sent independently, in either direction, on one wire. When last the British Association met in Leeds, submarine telegraphy had but just started into existence; thirty years later, the accomplished President of the Mechanical Section informed us, at our meeting at Bath, that 110,000 miles of cable had been laid by British ships, and that a fleet of nearly forty ships was occupied in various oceans in maintaining existing cables and laying new ones.

The important practical achievements by which most formidable difficulties have been surmounted, step by step, in the successive attainment of the marvellous results of our day, have exerted an influence upon the advancement, not merely of electrical science, but also of science generally and of its applications, fully equal to that which they have exercised upon the development of commerce and of the intercourse between the nations of the earth.

Thus, the laying of the earliest submarine cables, between 1851 and 1855, led Sir W. Thomson, in conference with Sir George Stokes, to work out the theory of signalling in such cables, by utilizing the mathematical results arrived at by Fourier in his investigation of the propagation of heat-waves. The failure of the first Atlantic cable led to the survey of the bottom of the Atlantic, which was the forerunner of deep-sea explorations, culminating in the work of the *Challenger* Expedition, and opening up new treasures of knowledge scarcely dreamt of when last the British Association met at Leeds. To the difficulties connected with the early attempts at submarine telegraphy, and the determination with which Thomson drove home the lessons learned, we owe the systematic investigations into the causes of the variations in resistance of copper-conductors, and the consequent improvements in the metallurgy of copper, which led to the realization of the high standard of purity of metal essential for the efficient working of telegraphic systems, and also to the extensive utilization of electricity in the production of pure copper. The rare combination of originality in powers of research and perspicuity in mathematical reasoning, with inventive and constructive genius, for which Thomson has so long been pre-eminent, has placed at the disposal of the investigator of electric science, and of the practical electrician, instruments of measurement and record which have been of incalculable value, and which owe their origin to the theoretical conclusions arrived at by him in his researches into the conditions to be fulfilled for the attainment of practical success in the construction and employment of submarine cables. The mirror galvanometer, the quadrant electrometer, the syphon-recorder, and the divided-ring electrometer, are illustrations of the valuable outcome of Thomson's labours; the combination of the last-named instrument with sliding resistance coils has rendered possible the accurate subdivision of a potential difference into 10,000 equal parts. The general use of condensers in connection with cable signalling, due to Varley's application of them for signalling through submerged cables with induced short waves, was instrumental in establishing the fact that all electro-static phenomena are simply the result of starting an electric current of known short duration round a closed circuit. The practical application of the Wheatstone Bridge led to numerous important mathematical investigations, and induced Clerk Maxwell to devise a new mode of applying determinants to the solution of the complicated electrical problems connected with networks of conductors. The necessity for the universal recognition of an electrical unit of resistance led to the establishment, in 1860, of the Electrical Standards Committee of the British Association, whose long succession of important annual reports was instrumental in most important developments of theoretical electricity, and, indeed, served to open up the whole science of electrical measurement. Matthiessen's important investigations of the electrical behaviour of metals and their alloys, and the preparation and properties of pure iron, were the outcome of the commercial demand for a practically useful standard of electrical resistance; while Latimer Clark's practical standard of electromotive force, the mercurous sulphate cell, became invaluable to the worker in pure electrical research. The unit of resistance established by the British Association Committee received, in 1866, most important scientific application at the hands of Joule, who, by measuring the rate of development of heat in a wire of known resistance by the passage of a known current, obtained a new value of the mechanical equivalent of heat. This value differed by about 1·3 per cent. from the most accurate results arrived at by his experiments on mechanical friction, a difference which eventually proved to be exactly the error in the British Association unit of resistance; so that the true value of the unit of resistance, or Ohm, was determined by Joule fifteen years before this result was achieved by electricians. Clerk Maxwell's remarkable electro-magnetic theory of light was put to the test, through the aid of the British Association unit of resistance, by Thomson, in determining the ratio of the electro-magnetic unit to the electro-static unit of quantity. Many other most interesting illustrations might be given of the invaluable aid afforded to purely scientific research by the practical results of the development of electrical science, and of the constant co-operation between the science student and the practical worker. No one could, more fitly than the late Sir William Siemens, have maintained, as he did in his admirable address at our meeting in Southampton in 1882, that we owe most of the rapid progress of recent times to the man of science who partly devotes his energies to the solution of practical problems, and to the practitioner who finds relaxation in the prosecution of purely scientific inquiries. Most assuredly both these classes of the world's benefactors may with equal right lay claim to rank the name of Siemens among those whom they count most illustrious!

In that highly interesting and valuable address delivered little more than a year before his sudden untimely removal from among us, the numerous important subjects discussed by him included not a few which he had made peculiarly his own in the wide range embraced by his enviable power of combining scientific research with practical work. Prominent among these were the applications of electric energy to lighting and heating purposes, and to the transmission of power, to the future development of which his personal labours very greatly contributed.

Siemens referred to the passing of the first Electric Lighting Bill, in the year of his presidency, as being designed to facilitate the establishment of electric installations in towns; but the anxiety of the Government of that day to protect the interests of the public through local authorities, led to the assignment of such power to these over the property of lighting companies, that the utilization of electric lighting was actually delayed for a time by those legislative measures. There can now be no doubt, however, that this delay has really been in the interests of intending suppliers and of users of the electric light, as having afforded time for the further development of practical details, connected with generation and distribution, which was vital to the attainment of a fair measure of initial success. The subsequent important modification of legislation on the subject of electric lighting, together with the practical realization of comparatively economical methods of distribution, the establishment of fairly equitable arrangements between the public and the lighting companies, and the apportionment, so far as the metropolis is concerned, of distinct areas of operation to different competing companies, have combined to place electric lighting in this country at length upon some approach to a really sound footing, and to give the required impetus to its extensive development. Nine companies either are now, or will very shortly be, actually at work supplying, from central stations, districts of London comprising almost the entire western and north-western portions of the metropolis. As regards other parts of England, there are already twenty-seven lighting stations actually at work in different towns, besides others in course of establishment, and many more projected. The town of Leeds has not failed to give serious attention to the subject of utilizing the electric light, and, although no general scheme has yet been adopted, the electricians who now visit this town will rejoice to see many of its public buildings provided with efficient electric illumination.

While the prediction made by Siemens, eight years ago, that electric lighting must take its place with us as a public illuminant,

has thus been already, in a measure, fulfilled, important progress is being continuously made by the practical electrician in developing and perfecting the arrangements for the generation of the supply, its efficient distribution from centres, and its delivery to the consumer in a form in which it can be safely and conveniently dealt with and applied at an outlay which, even now, does not preclude a considerable section of the public from enjoying the decided advantages presented by electric lighting over illumination by coal-gas. Yet our recent progress in this direction, encouraging though it has been, is insignificant as compared with the strides made in the application of electric lighting in the United States, as may be gauged by the fact that, while in America the number of arc lamps in use, in April of this year, was 235,000, and of glow-lamps about three millions, there are at present only about one-tenth the number of the latter, and one hundredth the number of arc lamps, in operation in England.

In some important directions we may, however, lay claim to rank foremost in the application of the electric light; thus, our large passenger-ships and our war-ships are provided with efficient electrical illumination; to the active operations of our Navy the electric light has become an indispensable adjunct; and our system of coast defence, by artillery and submarine mines, is equally dependent, for its thorough efficiency, upon the applications of electricity in connection with range-finding, with the arrangement and explosion of mines, and with the important auxiliary in attack and defence, the electric light, which, while so arranged, at the operating stations, as to be protected against destruction by artillery-fire and difficult of detection by the enemy, is available at any moment for affording invaluable information and important assistance and protection.

Other important applications of the electric light, such as its use as a lighthouse-illuminant, for the lighting of main roads in coal-mines, where its value is being increasingly appreciated, and even for signalling purposes in mid-air, through the agency of captive balloons, are continually affording fresh demonstrations of the value of this particular branch of applied electric science.

At the Electrical Exhibition at Vienna in 1883, where, not long before the lamented death of Siemens, I had the honour of serving as one of his colleagues in the representation of British interests, the progress which had been made in the construction of electrical measuring instruments since the French Exhibition and the Electrical Congress, two years before, was very considerable. The advance in this direction has been enormous since that time; but although the practical result of Thomson's and of Carlew's important work has been to supply us with trustworthy electrical balances and voltmeters, while efficient instruments have also been made by other well-known practical electricians, we have still to attain results in all respects satisfactory in these indispensable adjuncts to the commercial supply and utilization of electric energy.

In connection with this important subject the recent completion of the Board of Trade standardizing laboratory, established for the purposes of arriving at and maintaining the true values of electrical units, and of securing accuracy and uniformity in the manufacture of instruments supplied by the trade for electrical measurements, may be referred to with much satisfaction as a practical illustration of official recognition of the firm root which the domestic and industrial utilization of electric energy has taken in this country.

The achievements of the telephone were referred to by Siemens in glowing terms eight years ago; but the results then attained were but indications of the direction in which telephonic intercommunication was destined speedily to become one of the most indispensable of present applications of electricity to the purposes of daily life. Preece, in speaking at Bath, two years ago, of the advances made in applied electricity, showed that the impediments to telephonic communication between great distances had been entirely overcome; and now, although considerably behind America and France in the use of the telephone, we are rapidly placing ourselves upon speaking terms with our friends throughout the United Kingdom. The operations of the National Telephone Company well illustrate our progress in telephonic intercommunication: that company has now 22,743 exchange lines, besides nearly 5000 private lines; its exchanges number 272, and its call-offices 526. The number of instruments under rental in England has now reached 99,000; but, important as this figure is compared to our use of the telephone a very few years ago, it sinks into insignificance by the side of the number of instruments under rental in America, which at the beginning of the present year had reached 222,430, being an increase of 16,675 over the number in 1889. Only thirteen years have elapsed since the telephone was first exhibited as a practically workable apparatus to members of the British Association at the Plymouth Meeting, and the number of instruments now at work throughout the world may be estimated as considerably exceeding a million.

The successful transmission of the electric current, and the power of control now exercised over the character which electrically-transmitted energy is made to assume, are not alone illustrated by the efficiency of the arrangements already developed for the supply of the electric light from central stations. Siemens dwelt upon this subject at Southampton with the ardent interest of one who had made its development one of the objects of his energetic labours in later years, and also with a prophet's prognostications of its future importance. In speaking of the electric current as having entered the lists in competition with compressed air, the hydraulic accumulator, and the quick-running rope driven by water-power, Siemens pointed out that no further loss of power was involved in the transformation of electrical into mechanical energy than is due to friction, and to the heating of the conducting wires by the resistance they oppose, and he showed that this loss, calculated upon data arrived at by Dr. John Hopkinson and by himself, amounted at the outside to 38 per cent. of the total energy. Subsequent careful researches by the Brothers Hopkinson have demonstrated that the actual loss is now much less than it was computed at in 1885; as much as 87 per cent. of the total energy transmitted being realizable at a distance, provided there be no loss in the connecting leads used.

The Paris Electric Exhibition of 1881 already afforded interesting illustrations of the performance of a variety of work by power electrically transmitted, including a short line of railway constructed by the firm of Siemens, which was a further development of the successful result already attained in Berlin by Werner Siemens in the same direction, and was, in its turn, surpassed by the considerably longer line worked by Messrs. Siemens at the Vienna Exhibition two years later. Various short lines which have since then been established by the firm of Siemens are well known, and one of the latest public acts in the valuable life of Siemens was to assist at the opening of the electric tramway at Portrush, in the installation of which he took an active part, and where the idea, so firmly rooted in his mind from the date of his visit to the Falls of Niagara, in 1876, of utilizing water-power for electrical transmission—a result first achieved on a small scale by Lord Armstrong—was more practically realized than had yet been the case. Since that time Ireland has witnessed a further application of electricity to traction purposes, and of water-power to the provision of the required energy, in the working of the Bessbrook and Newry tramway, while London at length possesses an electric railway, three miles in length, to be very shortly opened, which will connect the City with one of the southern suburbs through a tram subway, and, although including many sharp curves and steep gradients, will be capable of conveying one hundred passengers at a time, at speeds varying from thirteen to twenty-four miles per hour. During the past year a regular service of tramcars has been successfully worked, through the agency of secondary batteries, upon part of one of the large tramways of North London, with results which bid fair to lead to an extensive development of this system of working. The application of electricity to traction purposes has, however, received far more important development in the United States; at the commencement of this year there were in operation in different States 200 electrical tramroads, chiefly worked upon the Thomson-Houston and the Sprague systems, and having a collective length of 1641 miles, with 2346 motor-cars travelling thereon. Further extensions are being rapidly made; thus, one company alone has 39 additional roads, of a collective length of 385 miles, under construction, to be worked through the agency of storage-batteries.

The idea cherished by Siemens, and enlarged upon by him in more than one interesting address, of utilizing the power of Niagara, appears about to be realized, at any rate in part; as a large tract of land has been recently acquired, by a powerful American association, about a mile distant from the Falls, with a view to the erection of mills for utilizing the power, which it is also proposed to transmit to distant towns; and an International Commission, with Sir William Thomson at its head, and with Mascart, Turrettini, Coleman Sellers, and Unwin as members, will carefully consider the problems involved in the execution of this grand scheme.

The application of electric traction to water-traffic, first successfully demonstrated in 1883, is receiving gradual development, as illustrated by the considerable number of pleasure-boats which may now be seen on the Upper Thames during the boating season, and in connection with which Prof. George Forbes proposed, at our meeting last year, that stations for charging the requisite cells, through the agency of water-power, should be established at the many weirs along the river, so as to provide convenient electric coaling-stations for the river pleasure-fleet.

Electrically-transmitted energy was first applied in Germany to haulage work in mines by the firm of Siemens some years ago, and great progress has since been achieved herein on the Continent and in America. Comparatively little has been accomplished in this direction in England; but it is very interesting to note, on the present occasion, that the first successful practical application of electricity in this country to pumping and underground haulage-work was made in 1887, in this neighbourhood, at the St. John's Colliery, at Normanton, where an extensive installation, carried out by Mr. Immisch, so well known in connection with electric launches, is furnishing very satisfactory results in point of economy and efficiency. The gigantic installations existing for the same purposes in Nevada and California afford remarkable illustrations of the work to be accomplished in the future by electrically-transmitted energy.

Among the many subjects of importance studied by Joule with the originality and thoroughness characteristic of his work, was the application of voltaic electricity to the welding and fusion of metals. Thirty-four years ago he published a most suggestive paper on the subject, in which, after dealing with the difficulties attending the operation of welding, and of the interference of films of oxide, formed upon the highly heated iron surfaces, with the production of perfect welds either under the hammer or by the methods of pressure (of which he then predicted the application to large masses of forged iron), he refers to the possibility of applying the calorific agency of the electric current to the welding of metals, and describes an operation witnessed by him in the laboratory of his fellow-labourer, Thomson, of fusing together a bundle of iron wires by transmitting through them, when embedded in charcoal, a powerful voltaic current. Joule afterwards succeeded in fusing together a number of iron wires with the current of a Daniell battery, and in welding together wires of brass and steel, platinum and iron, &c. In discussing the question of the amount of zinc consumed in a battery for raising a given amount of iron to the temperature of fusion, he points out that the same object would probably be more economically attained by the use of a magneto-electric machine, which would allow the heat to be provided by the expenditure of mechanical force, developed in the first instance by the expenditure of heat; and he indicates the possibility of arranging machinery to produce electric currents which shall evolve one-tenth of the total heat due to the combustion of the coal used, so that 5000 grains of coal applied through that agency would suffice for the fusion of one pound of iron. The successful practical realization of Joule's predictions in regard to the application of electric currents, thus developed, to the welding of iron and steel, and to analogous operations, through the agency of the efficient machines devised by Prof. Elihu Thomson, was demonstrated to the members of the Association by Prof. Ayrton at Bath two years ago, and was shown upon a larger scale to visitors at the Paris Exhibition last year, and recently to highly interested audiences in London by our late President, Sir Frederick Bramwell. The latter demonstrated that the production of iron-welds by means of the Thomson machines was accomplished nearly twice as rapidly as by expert craftsmen; the perfection of the welds being proved by the fact that the strength of bars broken by tensile strains at the welds themselves was about 92 per cent. of the strength of the solid metal. At the Crewe Works Mr. Webb is successfully applying one of these machines to a variety of welding-work. The rapidity with which masses of metal of various dimensions are raised in those machines to welding heat is quite under control; the heat is applied without the advent of any impurities, as from fuel, and the speed of execution of the welding operation reduces to a minimum the time during which the heated surfaces are liable to oxidize. With such practical advantages as these, this system of electric welding bids fair to receive many useful applications.

Another very simple system of electric welding, especially applicable to thin iron and steel sheets, hoops, &c., has been cotemporaneously elaborated in Russia by Dr. Bernados, and is already being extensively used. The required heat at the surfaces to be welded is developed by connecting the metal with the negative pole of the dynamo-machine, or a battery of accumulators, the circuit being completed by applying a carbon electrode to the parts to be heated; the reducing power of the carbon is said to preserve the heated metal surfaces from oxidation during the very brief period of heating. This mode of operation appears to have been practised upon a small scale, some years ago, by Sir William Siemens, to whom we also owe the first attempt to practically apply electric energy to the smelting of metals.

In his address in 1882 he referred to some results attained with his small electrical furnace, and pointed out that, although electric energy could, obviously, not compete economically with the direct combustion of fuel for the production of ordinary degrees of heat, the electric furnace would probably receive advantageous application for the attainment of temperatures exceeding the limits (about 1800° C.) beyond which combustion was known to proceed very sluggishly. This prediction appears to have been already realized through the important labours of Messrs. Cowles, who some years ago attacked the subject of the application of electricity to the achievement of metallurgic operations with the characteristic vigour and fertility of resource of our Transatlantic brethren. After very promising preliminary experiments, they succeeded, in 1885, at Cleveland, Ohio, in maturing a method of operation for the production of aluminium-bronze, ferro-aluminium, and silicium-bronze, with results so satisfactory as to lead to the erection of extensive works at Lockport, N.Y., where three dynamo-machines, each supplying a current of about 3000 Amperes, are worked by water-power, through the agency of turbines, each of 500 horse-power, eighteen electric furnaces being now in operation for the production of aluminium alloys. These achievements have led to the establishment of similar works in North Staffordshire, where a gigantic dynamo-machine has been erected, furnishing a current of 5000 Amperes, with an E.M.F. of 50 to 60 volts. The arrangements of the electrodes in the furnaces, the preparation of the furnace-charges (consisting of mixtures of aluminium-ore with charcoal and with the particular granulated metal with which the aluminium is to become alloyed at the moment of its elimination from the ore); the appliances for securing safety in dealing with the current from the huge dynamo-machine, and many other details connected with this new system of metallurgic work, possess great interest. Various valuable copper- and aluminium-alloys are now produced by alloying copper itself with definite proportions of the copper-alloy, very rich in aluminium, which is the product of the electric furnace. The rapid production in large quantities of ferro-aluminium—which presents the aluminium in a form suitable for addition in definite proportions to fluid cast-iron and steel—is another useful outcome of the practical development of the electric furnace by Messrs. Cowles.

The electric process of producing aluminium-alloys has, however, to compete commercially with their manufacture by adding to metals, or alloys, pure aluminium produced by processes based upon the method originally indicated by Oersted in 1824, successfully carried out by Wöhler three years later, and developed into a practical process by H. Ste. Claire Deville in 1854—namely, by eliminating aluminium from the double chloride of sodium and aluminium in the presence of a fluoride, through the agency of sodium. An analogous process, indicated in the first instance by H. Rose—namely, the corresponding action of sodium upon the mineral cryolite, a double fluoride of aluminium and sodium—has also been recently developed at Newcastle, where the first of these methods was applied, upon a somewhat considerable scale, in 1860, by Sir Lowthian Bell, but did not then become a commercial success, mainly owing to the costliness of the requisite sodium. As the cost of this metal chiefly determines the price of the aluminium, technical chemists have devoted their best energies to the perfection and simplification of methods for its production, and the success which has culminated in the admirable Castner process constitutes one of the most interesting of recent illustrations of the progress made in technical chemistry, consequent upon the happy blending of chemical with mechanical science, through the labours of the chemical engineer.

Those who, like myself, remember how, between forty and fifty years ago, a few grains of sodium and potassium were treasured up by the chemist, and used with parsimonious care in an occasional lecture-experiment, cannot tire of feasting their eyes on the stores of sodium-ingots to be seen at Oldbury as the

results of a rapidly and dexterously executed series of chemical and mechanical operations.

The reduction which has been effected in the cost of production of aluminium through this and other processes, and which has certainly not yet reached its limit, can scarcely fail to lead to applications of the valuable chemical and physical properties of this metal so widespread as to render it as indispensable in industries and the purposes of daily life as those well-known metals which may be termed domestic, even although, and, indeed, for the very reason that, its association with many of these, in small proportion only, may suffice to enhance their valuable properties or to impart to them novel characteristics.

The Swedish metallurgist, Wittenström, appears to have been the first to observe that the addition of small quantities of aluminium to fused steel and malleable iron had the effect of rendering them more fluid, and, by thus facilitating the escape of entangled gases, of ensuring the production of sound castings without any prejudicial effect upon the quality of the metal. The excellence of the so-called Mitis castings, produced in this way, appears thoroughly established, and the results of recent important experiments seem to be opening up a field for the extensive employment of aluminium in this direction, provided its cost becomes sufficiently reduced. The valuable scientific and practical experiments of W. J. Keep, James Riley, R. Hadfield, Stead, and other talented workers in this country and the United States, are rapidly extending our knowledge in regard to the real effects of aluminium upon steel, and their causes. Thus it appears to be already established that the modifications in some of the physical properties of steel resulting from the addition of that metal, are not merely ascribable to its actual entrance into the composition of the steel, but are due, in part, to the de-oxidation by aluminium of some proportion of iron-oxide which exists distributed through the metal, and prejudicially affects its fluidity when melted. In the latter respect, therefore, the influence exerted by aluminium, when introduced in small proportions into malleable iron and steel, appears to be quite analogous to that of phosphorus, silicium, or lead when these are added in small proportions to copper and certain of its alloys, the de-oxidation of which, through the agency of those substances, results in the production of sound castings of increased strength and uniformity. It is only when present in small proportion in the finished steel that aluminium increases the breaking strain and elastic limit of the product.

The influence of aluminium, when used in small proportion, upon the properties of grey and white cast-iron, is also of considerable interest, especially its effect in promoting the production of sound castings, and of modifying the character of white iron in a similar manner to silicium, causing the carbon to be separated in the graphitic form; with this difference—that the carbon appears to be held in solution until the moment of setting of the liquid metal, when it is instantaneously liberated, with the result that the structure of the cast metal and distribution of the graphite are perfectly uniform throughout.

The probable beneficial connection of aluminium with the industries of iron and steel naturally directs attention to the great practical importance, in the same direction, which has already been acquired, and promises to be in increasing measure attained, by certain other metals which, for long periods succeeding their discovery, have either been only of purely scientific interest and importance, or have acquired practical value in regard to their positions in a few directions quite unconnected with metallurgy. Thus great interest attaches to the influence of the metals manganese, chromium, and tungsten upon the physical properties of steel and iron.

The name of Mushet is most prominently associated with the history of manganese in its relations to iron and steel. Half a century ago David Mushet carried out very instructive experiments on the influence exerted upon the properties of steel by the presence of manganese; and to Robert Mushet we owe the invaluable experiments leading to his suggestion to use manganese in the production of steel by the Bessemer process, which at once smoothed the path to the marvellously rapid and extensive development of the applications of steel produced by that classic method, and subsequently by the open-hearth or Siemens-Martin process—a development which has recently received its crowning illustration in the completion of one of the grandest of existing triumphs of engineering science and constructive skill—the Forth Bridge.

Robert Hadfield has recently contributed importantly to our knowledge of the relations of manganese to iron. His systematic study of the subject has revealed some very remarkable variations in the physical properties of so-called manganese-steel, according to the proportions of manganese which it contains. Thus, while the existence in steel of proportions ranging from 0·1 up to about 2·75 per cent. improves its strength and malleability, it becomes brittle if that limit is exceeded, the extreme of brittleness being obtained with between 4 and 5 per cent. of manganese; if, however, the percentage is increased to not less than 7, and up to 20, alloys of remarkable strength and toughness are obtained. Castings of high manganese steel, such as wheel-tyres, combine remarkable hardness with toughness. Even if the proportion of manganese is as high as 20 per cent. in a steel containing 2 per cent. of carbon, it can be forged; whereas it is very difficult to forge a steel of ordinary composition containing as much as 2·75 per cent. of carbon. Another remarkable peculiarity of the high manganese-steel is its behaviour when quenched in water. Instead of the heated metal being hardened and rendered brittle by the sudden cooling, like carbon-steel, its tensile strength and its toughness are increased; so that water-quenching is really a toughening process, as applied to the manganese-alloy; and an interesting feature connected with this is that, the colder the bath into which the highly-heated metal is plunged, the tougher is the product. The curious effect of manganese in reducing, and even destroying, the magnetic properties of iron was already noticed by Rinmann nearly 120 years ago; one result of Hadfield's important labours has been to place in the hands of such eminent physicists as Thomson, John Hopkinson, and Reinold, materials for the attainment of most interesting information respecting the electrical and other physical characteristics of manganese-steel. Hopkinson, from experiments with a sample of steel containing 12 per cent. of manganese, estimated that not more than 9 out of the 86 per cent. of the iron composing the mass was magnetic, and he considered that the manganese enters into that which must, for magnetic purposes, be regarded as the molecule of iron, completely changing its properties, a fact which must have great significance in any theory regarding the nature of magnetization. The great hardness of manganese-steel, and the consequent difficulty of dealing with it by means of cutting-tools, constitute at present the chief impediments to its technical applications in many directions; but where great accuracy of dimensions is not required, and where great strength is an essential, it is already put to valuable uses.

The importance of manganese in connection with the metallurgy of iron and steel is in a fair way of finding its rival in that of the metal chromium, the employment of which, as an alloy with steel, was first made the subject of experiment in 1821, by Berthier, who was led by the important experiments of Faraday and Stoddart, then just published, to endeavour to alloy chromium with steel, and obtained good results by fusing steel together with a rich alloy of chromium and iron, so as to introduce about 15 per cent. of the former into the metal. Further small experiments were made the year following, by Faraday and Stoddart, in the same direction; but chrome-steel appears to have been first produced commercially at Brooklyn, N.Y., sixteen years ago. Ten years later its manufacture had become developed in France, and the varieties of chrome-steel produced in the Loire district now receive important and continually-extending applications, on account of their combining comparative hardness with high tenacity, and only little loss in ductility, and of their acquiring great closeness of structure when tempered.

The influence of chromium upon the character of steel differs in several marked respects from that exercised by manganese; thus, chrome-steels weld badly, or not at all, whereas manganese-steels weld very readily, and work under the hammer better than ordinary carbon-steel. Again, the remarkable influence of manganese upon the magnetic properties of steel and iron is not shared by chromium. Chrome-steel has for some time been a formidable rival of the very highest qualities of carbon-steel produced for cutting-tools, and of the valuable tungsten-steel which we owe to Robert Mushet. The great hardness, high tenacity, and exceeding closeness of structure possessed by suitably-tempered steel containing not more than from 1 to 1·5 per cent. of chromium, and from 0·8 to 1 per cent. of carbon, renders this material invaluable for war purposes: cast projectiles, when suitably tempered, have penetrated compound steel and iron plates over nine inches in thickness, such as are used upon armoured ships of war, without even sustaining any important change of form. The proper tempering of these

projectiles necessitates their being produced hollow; their cavities or chambers are only of small capacity, but the charge of violent explosive which they can contain, and which can be set into action without the intervention of fuze or detonating appliance, suffices to tear these formidable punching-tools into fragments as they force their way irresistibly through the armoured side of a ship, and to violently project those fragments in all directions, with fearfully destructive effects. The employment of chromium as a constituent of steel plates used for the protection of ships of war is already being entered upon, and the influence exerted by the presence of that metal in small quantities in steel employed in the construction of guns is also at present a subject of investigation. At Crewe, Mr. F. Webb has for some time past used chromium, with considerable advantage, in the production of high-quality steels for railway requirements.

The practical results attained by the introduction of copper and of nickel as components of steel have also recently attracted much attention. At the celebrated French steel works of M. Schneider, at Creuzot, the addition of a small percentage of copper to steel used for armour-plates and projectiles is practised, with the object of imparting hardness to the metal without prejudice to its toughness. James Riley has found that the presence of aluminium in very small quantities facilitates the union of steel with a small proportion of copper, and that the latter increases the strength but does not improve the working qualities of steel. With nickel, Riley has obtained products analogous in many important respects to manganese steel; the remarkable differences in the physical properties of the manganese alloys, according to their richness in that metal, are also shared by the nickel alloys, some of these being possessed of very valuable properties; thus, it has been shown by Riley that a particular variety of nickel-steel presents to the engineer the means of nearly doubling boiler-pressures, without increasing weight or dimensions. He has, moreover, found the co-existence of manganese in small quantity with nickel in the alloy to contribute importantly to the development of valuable physical properties.

The careful study of the alloys of aluminium, chromium, manganese, tungsten, copper, and nickel, with iron and with steel, so far as it has been carried, with especial reference to the influence which they respectively exercise upon the salient physical properties of those materials, even when present in them in only very small proportions, has demonstrated the importance of a more searching or complete application of chemical analysis, than hitherto practised, to the determination of the composition of the varieties of steel which practical experience has shown to be peculiarly adapted to particular uses. It appears, indeed, not improbable that certain properties of these, which have been ascribed to slight variations in the proportion or the condition of the constituent carbon, or in the amounts of silicium, phosphorus, and manganese which they contain, may sometimes have been due to the presence in minute quantities of one or other of such metals as those named, and to the effects which they produce, either directly, or indirectly by modifying or counteracting the effects of the normal constituents of steel. The important part now played by manganese in steel manufacture is an illustration of the comparatively recent results of research, and of practical work based on research, in these directions, and the effects of the presence in steel of only very small quantities of some of the other metals named are already, as I have pointed out, being similarly understood and utilized.

Such systematic researches as those upon which Osmond, Roberts-Austen, and many other workers have been for some time past engaged, may make us acquainted with the laws which govern the modifications effected in the physical characteristics of metals by alloying these with small proportions of other metals. The suggestion of Roberts-Austen, that such modifications may have direct connection with the periodic law of Mendeleeff, which may furnish explanations of the causes of specific variations in the properties of iron and steel, has been followed up energetically by Osmond, who has experimentally investigated the thermal influence upon iron of the elements phosphorus, sulphur, arsenic, boron, silicium, nickel, manganese, chromium, copper, and tungsten. He regards his results as being quite confirmatory of the soundness of Roberts-Austen's suggestion, as they demonstrate that foreign elements having atomic volumes lower than iron tend to make it assume or preserve the particular molecular form in which it has itself the lowest atomic volume, while the converse is the case for the foreign elements of high atomic volume. An analogous influence was found to be exerted by those two groups of elements upon the permanent magnetization of steel.

Captivating as such deductions are, those who have devoted much attention to the practical investigation of iron, steel, and other metals, cannot but feel that much caution has to be exercised in drawing broad conclusions from the results of such researches as these. Like the investigations recently made with the object of ascertaining the condition in which carbon exists in steel, and the part played by it in determining the modifications in the properties developed in that material by the influence of temperature and of work done upon it, they are surrounded by formidable difficulties, arising from the practical impossibility of altogether eliminating the disturbing influences of minute quantities of foreign elementary bodies, co-existing in the metal operated upon, with those whose effects we desire to study. Certain it is, however, that by acquiring an accurate acquaintance with the composition of varieties of iron and steel exhibiting characteristic properties; by persevering in the all-important work of systematic practical examination of the mechanical and physical peculiarities developed in iron and steel of known composition by their association with one or more of the rarer metals in varied proportions, and by the further prosecution of chemical and physical research in such directions as those which have already been fruitful of most instructive results, such talented labourers as Chernoff, Osmond, Roberts-Austen, Barus and Strouhal, Hadfield, Keep, James Riley, Stead, Turner, and others, cannot fail to contribute continually to the development of improvements equalling in importance those already attained in the production, treatment, and methods of applying cast-iron, malleable iron, and steel, or alloys equivalent to steel in their qualities.

The causes of the variations in the physical properties of steel produced by the so-called hardening, annealing, and tempering processes were for very many years a fruitful subject of experimental inquiry, as well as of theoretical speculation with regard to the condition in which the carbon is distributed in steel, according to whether the metal is hardened or annealed, or in an intermediate, tempered state. Recent researches have made our knowledge in the latter direction fairly precise; as yet, however, we are only on the track of definite information respecting the nature and extent of connection between the physical peculiarities of steel in those different conditions and the established differences in the form and manner in which the carbon is disseminated through it.

The careful systematic study of the modifications developed in certain physical properties of iron and steel by gradual changes of temperature between fusion of the metal and the normal temperature, has shown those modifications to be governed by a constant law, and that at certain critical temperatures special phenomena present themselves. This important subject, which was so clearly brought before the Association last year in the interesting lecture of Roberts-Austen, has been, and is still being pursued by accomplished workers, among whom the most prominent is F. Osmond. The phenomenon of recalescence, or the re-glowing of, or liberation of heat in, iron and steel at certain stages during the cooling process, first noticed by Gore, and examined into by Barrett, appears to be the result of actual chemical combination between the metal and its contained carbon at the particular temperature attained at the time; while the absorption of heat, demonstrated by the arrest in rise of temperature during its continuous application to the metal, is ascribed to the elimination, within the mass, of carbon as an iron-carbide perfectly stable at low temperatures. The pursuit of a well-devised system of experimental inquiry into this subject has led Osmond to propound theories of the hardening and tempering of steel, which are at present receiving the careful study of physicists and chemists, and cannot fail to lead to further important advancement of our knowledge of the true nature of the influence of carbon upon the properties of iron.

Another important subject connected with the treatment of masses of steel, and with the influence exercised upon their physical characteristics by the processes of hardening and tempering, and by submitting them to oft-repeated concussions or vibrations, or frequent or long-continued strains, is the development and maintenance, or gradual disappearance, of internal stresses in the masses—one of the many important subjects to which attention was directed by Dr. Anderson, the Director-General of Ordnance Factories, in his very suggestive address to the Mechanical Section last year. This question is one of

especial interest to the constructor of steel guns, as the powers of endurance of these do not simply depend upon the quality of the material composing them, but are very largely influenced by the treatment which it receives at the hands of the gun-maker. Indeed, the highest importance attaches to the processes which are applied to the preliminary preparation of the individual parts of which the gun is constructed, and to the putting together of these so as to ensure their being and remaining in the physical condition best calculated to assist each other in securing for the structure the power of so successfully resisting the heavy strains to which it has to be subjected, as to suffer little alteration other than that due to the superficial action of the highly-heated products of explosion of the charges fired in the gun. The development of internal strains in objects of steel, especially by the hardening and tempering processes, or by their exposure to conditions favourable to unequal cooling of different parts of the mass, has long been a subject of much trouble and of experimental inquiry in connection with many applications of steel. Systematic experiments of the kind, commenced, about eighteen years ago, by the late Russian general Kalakoutsky, are now being pursued at Woolwich, with the objects of determining the nature and causes of internal stresses in steel gun-hoops and -tubes, and in shells, and of thereby establishing the proper course to be adopted for avoiding, lessening, or counteracting injurious stresses, on the one hand, and for setting up stresses beneficial to the powers of endurance of guns, on the other. One method of experiment pursued, with parts of guns, is to cut narrow hoops off the forgings, after a particular treatment, which are then cut right across at one place, it being observed whether, and to what extent, the resulting gaps open or close. This important subject has also been similarly investigated by my talented old friend and fellow-worker, the President this year of the Mechanical Section, Captain Andrew Noble, whose name in connection with the science and practice of artillery is familiar to us as household words.

The Crimean War taught nations many lessons of gravest import, to some of which Sir Richard Owen took occasion to call attention most impressively in the address delivered here, before the miseries of that war had become past history. The development of sanitary science, to which he especially referred, and which sprang from the bitter experience of that sad epoch, has had its parallel in the development of the science of artillery; but it would indeed be difficult to establish any parallelism between the benefits which even the soldier and the sailor have reaped from the great strides made by both these sciences. The acquisition of knowledge of the causes of the then hopelessness of gallant struggles which medical skill and self-sacrificing devotion made against the sufferings of the victims of battles and of fell diseases, as deadly as the cruellest implements of war; the application of that knowledge to the provision of the blessings of antiseptic treatment of wounds and to the intelligent utilization of disinfectants and of other valuable preventive measures, to the supply of wholesome water, of wholesome food in campaigning, of sensible clothing, and of wholesome air in hospitals, barracks, and ships—these are some few of the benefits which the soldier and the sailor have derived from the development of sanitary science, which was so powerfully stimulated by the terrible lessons learned during the long-drawn-out siege of Sebastopol: and it is indeed pleasant to reflect that there has been, for years past, most wholesome competition between nations in the enlargement of those benefits, and their dissemination among the men whose vocation it is to slay and be slain. The periodical International Congresses on Hygiene and Demography, of which we shall cordially welcome next year's assemblage in London, and whose members will deplore the absence from among them of the veteran Nestor in the science and practice of hygiene, Sir Edwin Chadwick, have afforded conclusive demonstration of the heartiness with which nations are now co-operating with a view to utilize the invaluable results attained by the successful labourers in sanitary science.

What, on the other hand, shall we say of the benefits which sailors and soldiers, in the pursuit of their calling, derive from the ceaseless costly competition amongst nations for supremacy in the possession of formidable artillery, violent explosives, quick-firing arms of deadly accuracy, and fearful engines which, unseen, can work wholesale destruction in a fleet? And what can we say of the benefits acquired by individual countries in return for their continuous, and sometimes ruinous, expenditure in endeavouring to maintain themselves upon an equality with their neighbours in man-killing power? The conditions under which engagements by sea or land will in the future be fought have certainly become greatly modified from those of thirty-five years ago, and the duration of warfare, even between nations in conflict who are on a fair equality of resources, must become reduced; but, as regards the results of a trial of strength between contending forces, similarly equipped, as they now will be, with the latest of modern appliances only varying in detail, these must, after all, depend, as of old, partly upon accident, favoured, perhaps, by a temporary superiority in equipment, partly upon the skill and military genius of individuals, and very much upon the characteristics of the men who fight the battles.

What really can be said in favour of the advances made in the appliances of war—and this is, perhaps, the view which in such a town as Leeds we should keep before our eyes to the exclusion of the dark side of the picture—is, that by continuous competition in the development of their magnitude, diversity, and perfection, the resources of the manufacturer, the chemist, the engineer, the electrician, are taxed to the uttermost; with the very important, although incidental, results, that industries are created or expanded and perfected, trades maintained and developed, and new achievements accomplished in applied science, which in time beneficially affect the advance of peaceful arts and manufactures. In these ways the expenditure of a large proportion of a country's resources upon material which is destroyed in creating destruction does substantially benefit communities, and tends to the accomplishment of such material progress by a country as goes far to compensate its people for the sacrifices which they are called upon to incur for the maintenance of their dignity among nations.

From this point of view, at any rate, it may interest members of the British Association for the Advancement of Science, and for the promotion of its applications to the welfare and happiness of mankind, to hear something of recent advances in one of the several branches of science in its applications to naval and military requirements with which, during a long and arduous official career, now approaching its close, I have become in some measure identified.

Since the meeting of the Association in this town in 1858, the progress which has been made in the regulation of the explosive force of gunpowder, so as to adapt it to the safe development of very high energy in guns presenting great differences in regard to size and to the work which they have to perform, has been most important. The different forms of gunpowder which were applied to war-purposes in this and other countries, until within the last few years, presented comparatively few differences in composition and methods of manufacture from each other, and from the gunpowder of our ancestors. The replacement of smooth-bore guns by rifled artillery, which followed the Crimean War, and the great increase in the size and power of guns, necessitated by the application of armour to ships and forts, soon called, however, for the pursuit of investigations having for their object the attainment of means for variously modifying the action of fired gunpowder, so as to render it suitable for artillery of different calibres whose power could not be effectively, or, in some instances, safely, developed by the use of the only kind of gunpowder then employed in English artillery of all calibres.

The means resorted to in the earlier of these investigations, and adhered to for many years, for controlling the violence of explosion of gunpowder, consisted exclusively in modifying the size and form of the individual masses composing a charge, and of their density and hardness, with the object of varying the rate of burning of those masses in a gun; it being considered that, as the proportions of ingredients generally employed very nearly correspond to those required for the development of the greatest chemical energy by the thoroughly-incorporated materials, the attainment of the desired results should be, if possible, effected rather by modifications of the physical and mechanical characters of gunpowder, than by variations of the proportions and chemical characters of its ingredients.

The varieties of powder from time to time introduced into artillery-service, as the outcome of investigations in this direction, were of two distinct types: the first of these consisted of further developments of the old granulated or corned powder, being produced by breaking up more or less highly-pressed slabs of the material into grains, pebbles, or boulders of approximately uniform size and shape. Gunpowders of this class, ranging in size from about 1000 pieces to the ounce to about 6 pieces to the pound, have performed efficient service, and certain of them are still employed. The character of the other type is based upon the theoretical view that uniformity in the action of a

particular gunpowder, when employed under like conditions, demands not merely identity in regard to composition, but also identity in form, size, density, and structure of the individual masses of which a charge consists. To approach the practical realization of this view, equal quantities of one and the same mixture of ingredients, presented in the form of powder of uniform fineness and dryness, must be submitted to a particular pressure, for a fixed period, in moulds of uniform size, the surrounding conditions and subsequent manufacturing processes being as nearly as possible alike. Practical experience has however shown that uniformity in the ballistic properties of black powder can be even more readily secured by the thorough blending or mixing together of different products of manufacture, presenting some variations in regard to size, density, hardness, or other features, than by aiming at an approach to identity in the characters of the individual grains or masses.

When our attention was first actively directed to the modification of the ballistic properties of powder, the subject had already been to some extent dealt with, in the United States, by Rodman and Doremus, and the latter had proposed the employment, in heavy guns, of charges consisting of large pellets of prismatic form. While this prismatic powder, which was first used in Russia, was being perfected, and extensively applied there as well as in Germany and England, the production of powder-masses more suitable, by the comparatively gradual nature of their explosion, for the very large charges required for the heavy artillery of the present day, was actively pursued in Italy, and by our own Government Committee on Explosives; the outcome of exhaustive practical investigations being the very efficient Fossano powder, or *poudre progressif* of the Italians, and the boulder- and large cylindrical-powders produced at Waltham Abbey.

Researches carried out by Captain Noble and myself, some years ago, with a series of gunpowders, presenting considerable differences in composition, indicated that decided advantages might be secured, for heavy guns especially, by the employment of such a powder as would furnish a comparatively very large volume of gas, its explosion being at the same time attended by the development of much less heat than in the case of ordinary black powder. In the course of these researches much light was thrown upon the causes of the wearing or erosive action of powder-explosions upon the inner surface of the gun, an action which, especially in the larger calibres of artillery, produces so serious a deterioration of the arm that the velocity of projection and accuracy of shooting suffer considerably, the wear being especially great where the products of explosion, while under the maximum pressure, can escape between the projectile and the bore. The great velocity with which the very highly-heated gaseous and liquid (fused solid) products of explosion sweep over the heated surface of the metal gives rise to a displacement of the particles composing the surface of the bore, which increases in extent as the latter becomes roughened, and thus opposes greater resistance; at the same time, the high temperature to which the surface is raised reduces the rigidity of the metal, and its consequent power of resisting the force of the gaseous torrent; and, lastly, some amount of chemical action upon the metal, by certain of the highly-heated, non-gaseous products of explosion, contributes towards an increase in the erosive effects. Experiments made upon a large scale by Captain Noble with powders of different composition, and with other explosives, have afforded decisive evidence that the explosive agent which furnishes the largest proportion of gaseous products, and the explosion of which is attended by the development of the smallest amount of heat, exerts least erosive action.

Some eminent German gunpowder-manufacturers, who were at this time actively engaged upon the production of a suitable powder for heavy guns, directed their attention, not merely to an alteration of the proportions of the ingredients, but also to a modification in the character of charcoal employed; the eventual result was the production of a new prismatic powder, composed of saltpetre in somewhat higher proportion than in normal black powder, and of a very slightly-burned charcoal of reddish-brown colour, quite similar to the *charbon roux* which Violette produced about forty years ago for use in sporting-powder, by the action of superheated steam upon wood or other vegetable matter. This brown prismatic powder (or "cocoa powder") differs from black powder not merely in colour: it burns very slowly in the open air, and in guns its action is comparatively gradual and long-sustained. The products of its explosion are simple. As the powder contains saltpetre in large proportion relatively to the sulphur and charcoal, these become fully oxidized, and a relatively very large amount of water-vapour is produced, partly because of the comparatively high proportion of water in the finished powder, and partly from the large amount of hydrogen in the slightly-charred wood or straw used. The smoke from a charge of brown powder differs but little in volume from that of black powder, but it disperses much more rapidly, owing to the speedy absorption of the finely-divided potassium salts, forming the smoke, by the large proportion of water-vapour through which they are distributed.

This kind of powder has been substituted, with considerable advantage, for black powder in guns of comparatively large calibre, but it soon became desirable to attain even more gradual action in the case of the very large charges required for guns of the heaviest calibres, such as the 110-ton gun, from which shot of about 1800 lbs. weight are propelled by a powder-charge of 960 lbs. Brown powder has, therefore, been modified in composition to suit these conditions; while, on the other hand, a powder intermediate in rapidity of action between black powder and the brown prism powder has been found more suitable than the former for use in guns of moderately large calibre.

The importance which machine-guns and comparatively large, quick-firing guns have assumed in the armament of ships has made it very desirable to provide a powder for them which will produce comparatively little or no smoke, as their efficient employment becomes greatly limited when, after a very few rounds rapidly fired, with black powder, the objects against which it is desired to direct the fire, are more or less completely hidden by the interposed smoke. Hence much attention has of late been directed to the production of smokeless, or nearly smokeless, powders for naval use. At the same time, the views of many military authorities regarding the importance of dispensing with smoke in engagements on land have also created a demand for smokeless powders suitable for field-artillery and for small-arms.

The properties of ammonium-nitrate, of which the products of decomposition by heat are, in addition to water-vapour, entirely gaseous, have rendered it a tempting material to those who have striven to produce a smokeless powder; but its deliquescent character has been a formidable obstacle to its application as a component of a useful explosive agent. By incorporating charcoal and saltpetre in particular proportions with ammonium-nitrate, F. Gaus recently claimed to have produced an explosive material free from the hygroscopic character common to other ammonium-nitrate mixtures, and furnishing only permanently gaseous and volatile, or smokeless, products of explosion. These anticipations were not realized, but they led the talented German powder-maker, Mr. Heidemann, to produce an ammonium-nitrate powder possessing remarkable ballistic properties, and producing comparatively little smoke, which speedily disperses. It yields a very much larger volume of gas and water-vapour than either black or brown powder, and is considerably slower in action than the latter; the charge required to produce equal ballistic results is less, while the chamber-pressure developed is lower, and the pressures along the chase of the gun are higher, than with brown powder. No great tendency is exhibited by it to absorb moisture from an ordinarily dry, or even somewhat moist, atmosphere, but it rapidly absorbs water when the hygroscopic condition of the air approaches saturation, and this greatly restricts its use.

About five years ago reports began to reach us from France of the attainment of remarkable results with a smokeless powder employed with the repeating or magazine rifle then in course of adoption for military service, and of marvellous velocities obtained by the use of this powder, in specially constructed artillery of great length. As in the case of the explosive agent called *Mélinite*, the fabulously-destructive effects of which were much vaunted at about the same time, the secret of the nature of this smokeless powder was well preserved by the French authorities; it is now known, however, that more than one smokeless explosive has succeeded the original, and that the material at present in use with the Lebel repeating rifle belongs to a class of nitro-cellulose or nitro-cotton preparations, of which several have been made the subject of patents in England, and of which varieties are also being used in Germany and other countries.

A comparison between the chemical changes attending the burning or explosion of gunpowder, and of the class of nitro-compounds represented by gun-cotton, at once explains the cause of the production of smoke by the former, and of the

smokelessness of the latter. Whilst the products of explosion of the nitro-compounds consist exclusively of gases and of water-vapour, gunpowder, being composed of a large proportion of saltpetre, or other metallic nitrate, mixed with charred vegetable matter and variable quantities of sulphur, furnishes products of which over 50 per cent. are not gaseous, even at high temperatures, and which are in part deposited as a fused solid—which constitutes the fouling in a firearm—and in part distributed in an extremely fine state of division through the gases and vapours developed by the explosion, thus giving to these the appearance of smoke as they escape into the air.

So far as smokelessness is concerned, no material can surpass *gun-cotton* (or other varieties of nitro-cellulose); but, even if the rate of combustion of the fibrous explosive in a firearm could be controlled with certainty and uniformity, its application as a safe propulsive agent is attended by so many difficulties that the non-success of the numerous early attempts to apply it to that purpose is not surprising. Those attempts, commencing soon after the discovery of gun-cotton, in 1846, and continued many years later in Austria, consisted entirely in varying the density and mechanical condition of employment of the gun-cotton fibre. No difficulty was experienced in thus exercising complete control over the rapidity of burning in the open air; but when the material was strongly confined, as in the bore of a gun, such methods of regulating its explosive force were quite unreliable, as some slight unforeseen variation in its compactness or in the amount and disposition of the air-spaces in the mass, would develop very violent action. Much more promising results were subsequently obtained by me by reducing the fibre to a pulp, as in the ordinary process of making paper, and converting this into highly-compressed, homogeneous masses of the desired form and size. Some favourable results were obtained at Woolwich, in 1867–68, in field-guns, with cartridges built up of compressed gun-cotton variously formed and arranged, with the object of regulating the rapidity of explosion of the charge. But although comparatively small charges often gave high velocities of projection, without any indications of injury to the gun, the uniform fulfilment of the conditions essential to safety proved to be beyond absolute control, even in guns of small calibre; and military authorities not being, in those days, alive to the advantages which might accrue from the employment of an entirely smokeless explosive in artillery, experiments in this direction were not persevered in. At the same time, considerable success attended the production of gun-cotton cartridges for sporting purposes, the rapidity of its explosion being controlled by various methods; very promising results were also attained with the Martini-Henry rifle and a lightly-compressed pulped gun-cotton charge, of pellet-form, the uniform action of which was secured by simple means.

A nearly smokeless sporting-powder had, in the meantime, been produced by Colonel Schultze, of the Prussian Artillery, from finely-divided wood, converted after purification into a mildly explosive form of nitro-cellulose, and impregnated with a small portion of an oxidizing agent. Subsequently this powder was produced in a granular form, and rendered considerably more uniform in character, and less hygroscopic; it then closely resembled the well-known E.C. sporting-powder, which consists of a nitro-cotton reduced to pulp, incorporated with the nitrates of potassium and barium, and converted into grains through the agency of a solvent and a binding material. Both these powders produce very little smoke compared with black powder, but do not compete with the latter in regard to accuracy of shooting, when used in military arms.

In past years both camphor and liquid solvents have been applied to the hardening of the surfaces of granulated or compressed masses of gun-cotton and of this class of its preparations, with a view to render them non-porous. In some smokeless powders of French, German, Belgian, and English manufacture, acetic ether and acetone have been also used, not merely to harden the granules or tablets of the explosive, but also to convert the nitro-cellulose, in the first instance, into a more or less gelatinous condition, so that it can readily be incorporated with other components and rolled, or spread into sheets, or pressed into moulds, or squirted into wires, rods, or tubes, while still in a plastic state. When the solvent has afterwards been removed, the hardened, horn-like, or somewhat plastic product is cut up into tablets, or into strips or pieces of suitable dimensions, for conversation into charges or cartridges.

Another class of smokeless powder, similar in physical characteristics to these nitro-cellulose powders, but containing nitro-glycerine as an important component, has been originated by Mr. Alfred Nobel, the well-known inventor of dynamite, and bears resemblance in its physical characteristics to another of his inventions, called blasting-gelatine, one of the most interesting of known violent explosive agents. When one of the lower products of nitration of cellulose is impregnated with the liquid explosive, nitro-glycerine, it gradually loses its fibrous nature, becoming gelatinized while assimilating the liquid; and the resulting product almost possesses the characters of a compound. This preparation, and certain modifications of it, have acquired high importance as blasting-agents more powerful than dynamite, and are possessed of the valuable property that their prolonged immersion in water does not separate from them any appreciable proportion of nitro-glycerine. The nitro-glycerine powder first produced by Mr. Nobel was almost perfectly smokeless and developed very high energy, accompanied by moderate pressures at the seat of the charge, but it possessed certain practical defects, which led to the development of several modifications of that explosive and various improvements in manufacture. The relative merits of this class of smokeless powder, and of various kinds of nitro-cellulose powder, are now under careful investigation in this and other countries, and several more or less formidable difficulties have been met with in their application, in small-arms especially; these arise in part from the comparatively great heat they develop, which increases the erosive effects of the products of explosion, and in part from the more or less complete absence of solid products. The surfaces of the barrel and of the projectile being left clean, after the firing, are in a condition favourable to their close adhesion while the bullet is propelled along the bore, with the consequent establishment of very greatly increased friction. The latter difficulty has been surmounted by more than one expedient, but always at the cost of absolute smokelessness.

Our knowledge of the results obtained in France and Germany with the use of smokeless powders in the new rifles and in artillery is somewhat limited; our own experiments have demonstrated that satisfactory results are attainable with more than one variety of them, not only in the new repeating-arm of our infantry, but also with our machine-guns, with field-artillery, and with the quick-firing guns of larger calibre which constitute an important feature in the armament of our Navy. The importance of ensuring that the powder shall not be liable to undergo chemical change detrimental to its efficiency or safety, when stored in different localities where it may be subject to considerable variations of temperature (a condition especially essential in connection with our own naval and military service in all parts of the world), necessitates qualities not very easily secured in an explosive agent consisting mainly of the comparatively sensitive nitro-compounds to which the chemist is limited in the production of a smokeless powder. It is possible, therefore, that the extent of use of such a material in our ships, or in our tropical possessions, may have to be limited by the practicability of fulfilling certain special conditions essential to its storage without danger or possible deterioration. If, however, great advantages are likely to attend the employment of a smokeless explosive, at any rate for certain Services, it will be well worth while to adopt such special arrangements as may be required for securing these without incurring special dangers; this may prove to be especially necessary in our ships of war, where temperatures so high as to be prejudicial even to ordinary black powder, sometimes prevail in the magazines, consequent mainly upon the positions assigned to them in the ships, but which may be guarded against by measures not difficult of application.

The Press accounts of the wonderful performances of the first smokeless powder adopted by the French—which, it should be added, were in some respects confirmed by official reports of officers who had witnessed experiments at a considerable distance—engendered a belief that a very great revolution in the conduct of campaigns must result from the introduction of such powders. It was even reported very positively that noiselessness was one of the important attributes of a smokeless powder, and highly-coloured comparisons have, in consequence, been drawn in Service-periodicals, and even by some military authorities, between the battles of the past and those of the future; the terrific din caused by the firing of the many guns and the roar of infantry-fire, in heavy engagements, being supposed to be reduced to noise so slight that distant troops would fail to know in what direction their comrades were engaged, and that sentries and outposts would no longer be able to warn their comrades of the approaching foe by the discharge of their rifles. Military

journals of renown, misled by such legendary accounts, chiefly emanating from France, referred to the absence of noise and smoke in battles as greatly enhancing the demands for skill and courage, and as surrounding a fight with mystery. The absence of recoil when a rifle was fired with smokeless powder was another of the marvels reported to attend the use of these new agents of warfare. It need scarcely be said that a closer acquaintance with them has dispelled the credit given to such of the accounts of their supposed qualities as were mythical, and a belief in which could only be ascribable to a phenomenal combination of credulity with ignorance of the most elementary scientific knowledge.

The extensive use which has been made in Germany of smokeless or nearly smokeless powder in one or two special military displays has, however, afforded interesting indications of the actual change which is likely to be wrought in the conditions under which engagements on land will be fought in the future, provided these new explosives thoroughly establish and maintain their position as safe and reliable propelling agents. Although the powder adopted in Germany is not actually smokeless, the almost transparent film of smoke produced by independent rifle-firing with it is not visible at a distance of about 300 yards; at shorter distances it presents the appearance of a puff from a cigar. The most rapid salvo-firing by a large number of men does not have the effect of obscuring them from distant observers. When machine-guns and field-artillery are fired with the almost absolutely smokeless powder which we are employing, their position is not readily revealed to distant observers by the momentary vivid flash of flame and slight cloud of dust produced.

There now appears little doubt that in future warfare belligerents on both sides will alike be users of these new powders; the screening or obscuring effect of smoke will therefore be practically absent during engagements between contending forces, and while, on the one hand, the very important protection of smoke, and its sometimes equally important assistance in manœuvres, will thus be abolished, both combatants will, on the other hand, secure the advantages of accuracy of shooting and of the use of individual fire, through the medium of cover, with comparative immunity from detection. Such results as these cannot fail to affect, more or less radically, the principles and conditions under which battles have hitherto been fought. With respect to the naval service, it is especially for the quick-firing guns, so important for defensive purposes, that a smokeless powder has been anxiously looked for; by the adoption of such a powder as has during the past year been elaborated for our artillery, should experience establish its reliability under all Service conditions and its power to fulfil all reasonable requirements in regard to stability, these guns will not only be used by our ships under conditions most favourable to their efficiency, but their power will also be very importantly increased.

The ready and safe attainment of very high velocities of projection through the agency of these new varieties of explosive agents, employed in guns of suitable construction, would appear at first sight to promise a very important advance in the power of artillery; the practical difficulties attending the utilization of these results are, however, sufficiently formidable to place, at any rate at present, comparatively narrow limits upon our powers of availing ourselves of the advantages in ballistics which they may present. The strength of the gun-carriages and the character of the arrangements used for absorbing the force of recoil of the gun, need considerable modifications, not easy of application in some instances; greater strength and perfection of manufacture are imperative in the case of the hollow projectiles or shells to be used with charges of a propelling agent by the firing of which in the gun they may be submitted to comparatively very severe concussions; the increased friction to which portions of the explosive contents of the shell are exposed by the more violent setting back of the mass may increase the possibility of their accidental ignition before the shell has been projected from the gun; the increase of concussion to which the fuze in the shell is exposed may give rise to a similar risk consequent upon an increased liability to a failure of the mechanical devices which are applied to prevent the igniting arrangement, designed to come into operation only upon the impact or graze of the projected shells, from being set into action prematurely by the shock of the discharge; lastly, the circumstance that the rate of burning of the time-fuze which determines the efficiency of a projected shrapnel shell is materially altered by an increase in the velocity of flight of the shell, also presents a source of difficulty.

The fallibility of even the most simple forms of fuze, manufactured in very large numbers, although it may be remote, must always engender a feeling of insecurity, when shells are employed containing an explosive agent of the class which, in recent years, it has been sought, by every resource of ingenuity, combined with intimate knowledge of the properties of these explosives, to apply as substitutes for gunpowder in shells, on account of their comparatively great destructive power.

One of the first uses, for purposes of warfare, to which it was attempted to apply gun-cotton, was as a charge for shells. But even when this was highly-compressed, and accurately fitted the shell-chamber, with the intervention only of a soft packing between the surfaces of explosive and of metal, to guard against friction between the two upon the shock of the discharge, no security was attainable against the ignition of the comparatively sensitive explosive by friction established within its mass at the moment when the shell is first set in motion. By the premature explosion of a shell charged with gunpowder, no important injury is inflicted upon the gun, but a similar accidental ignition of a gun-cotton charge must almost inevitably burst the arm. The earlier attempts to apply gun-cotton as a bursting-charge for shells were several times attended by very disastrous accidents of this kind; but the fact, afterwards discovered, that wet compressed gun-cotton, even when containing sufficient water to render it quite uninflammable, can be detonated through the agency of a sufficiently powerful charge of fulminate of mercury, or of a small quantity of dry gun-cotton embedded within it, has led to the perfectly safe application of gun-cotton in shells, provided the fuze, through the agency of which the initiative detonating agent in the shell comes into operation, is secure against any liability to premature ignition when the gun is fired. Many successful experiments have been made with shells thus charged with wet gun-cotton, which is now recognized as a formidable destructive agent applicable in shells with much less risk of casualty than attends the use of many other of the violent explosive bodies which it has become fashionable, in professional parlance, to designate as "high explosives."

Many devices and arrangements, more or less ingenious and complicated, have been schemed, especially in the United States, for applying preparations of the very sensitive liquid, nitro-glycerine, such as dynamite and blasting-gelatine, as charges for shells. Some of these consist in subdividing the charge by more or less elaborate methods; in others the shell is also lined with some soft elastic packing-material, and paddings of similar material are applied in the head and the base of the shell-chamber, with the object of reducing the friction and concussion to which the explosive is exposed when the projectile is first set in motion. Such arrangements obviously reduce the space available for the charge in the shell, and the best of them fail to render these explosives as safe to employ as wet gun-cotton. In order to avoid exposing shells loaded with such explosives to the concussion produced when propelling them by a powder-charge, compressed air has been applied as the propelling agent, and guns of special construction and very large dimensions, from which shells containing as much as 500 lbs. of gun-cotton or dynamite are projected through the agency of compressed air, have recently been elaborated in the United States, where great expectations are entertained of the value, for war-purposes, of these so-called pneumatic guns.

A highly ingenious device for utilizing a class of very powerful explosives in shells, without any risk of accident to the gun, was not long since brought forward by Mr. Grüsen, the well-known armour-plate and projectile manufacturer of Magdeburg. It consisted of a thoroughly efficient arrangement for applying the fact, first demonstrated by Dr. Sprengel, that mixtures of nitric acid of high specific gravity with solid or liquid hydrocarbons, or with the nitro-compounds of these, are susceptible of detonation, with development of very high energy. The two agents, of themselves non-explosive—nitric acid and the hydrocarbon, or its nitro-product—are separately confined in the shell; when it is first set in motion by the firing of the gun, the fracture of the receptacle containing the liquid nitric acid is determined by a very simple device; the two substances are then free to come into contact, and their very rapid mixture is promoted by the rotation of the shell, so that, almost by the time that it is projected from the gun, its contents, at first quite harmless, have become converted into a powerfully explosive mixture, ready to come into operation through the action of the fuze. Although safety appears assured by this system, the comparatively complicated nature of the contrivance, and the loss of space in the

shell thereby entailed, place it at a disadvantage, especially since some other very violent explosive agents have come to be applied with comparative safety in shells.

Between four and five years ago intelligence first reached us of marvellously destructive effects produced by shells charged with an explosive agent which the French Government was elaborating. The reported results surpassed any previously recorded in regard to violently destructive effects and great velocity of projection of the fragments of exploded shells, and it was asserted that the employment of this new material, Mélinite, was unattended by the usual dangers incident to this particular application of violent explosive agents, an assertion scarcely consistent with accounts which soon reached us of several terrible calamities due to the accidental explosion of shells loaded with Mélinite.

Although the secret of the precise nature of Mélinite has been extremely well preserved, it transpired ere long that extensive purchases were made in England, by or for the French authorities, of one of the many coal-tar derivatives which for some years past has been extensively manufactured for tinctorial purposes, but which, although not itself classed among explosive bodies until quite lately, had long before been known to furnish, with some metals, more or less highly explosive combinations, some of which have been applied to the production of preparations suggested as substitutes for gunpowder.

The product of destructive distillation of coal from which, by oxidation, this material is now manufactured, is the important and universally-known antiseptic and disinfectant, carbolic acid, or phenol. Originally designated carbazotic acid, the substance now known as picric acid was first obtained in small quantities as a chemical curiosity by the oxidation of silk, aloes, &c., and of the well-known blue dye indigo, which thus yielded another dye of a brilliant yellow colour. To the many who may regard this interesting phenol-derivative as a material concerning the stability and other properties of which we have little knowledge it will be interesting to learn that it has been known to chemists for more than a century. It was first manufactured in England for tinctorial purposes by the oxidation of a yellow resin (*Xanthorrhœa hastilis*), known as Botany Bay gum. Its production from carbolic acid was developed in Manchester in 1862, and its application as a dye gradually extended, until, in 1886, nearly 100 tons were produced in England and Wales.

Although picric acid compounds were long since experimented with as explosive agents, it was not until a very serious accident occurred, in 1887, at some works near Manchester where the dye had been for some time manufactured, that public attention was directed in England to the powerfully explosive nature of this substance itself. The French authorities appear, however, to have been at that time already engaged upon its application as an explosive for shells. It is now produced in very large quantities at several works in Great Britain, and it has been extensively exported during the last four years, evidently for other than the usual commercial purposes. Large supplies of phenol, or carbolic acid, have, at the same time, been purchased in England for France, and lately for Germany, doubtless for the manufacture of picric acid, very extensive works having been established for its production in both those countries. It has been made the subject of experiment by our military authorities, and its position has been well established as a thoroughly stable explosive agent, easily manufactured, comparatively safe to deal with, and very destructive when the conditions essential for its detonation are fulfilled.

The precise nature of Mélinite appears to be still only known to the French authorities: it is asserted to be a mixture of picric acid with some material imparting to it greater power; but accounts of accidents which have occurred even quite recently in the handling of shells charged with that material appear to show that, in point of safety or stability, it is decidedly inferior to simple picric acid. Reliable as the latter is in this respect, its employment is, however, not unattended with the difficulties and risks which have to be encountered in the use, in shells, of other especially violent explosives. Future experience in actual warfare can alone determine decisively the relative value of violent explosive agents, like picric acid or wet gun-cotton, and of the comparatively slow explosive, gunpowder, for use in shells; it is certain, however, that the latter still presents distinct advantages in some directions, and that there is no present prospect of its being more than partially superseded as an explosive for shells.

With regard to submarine mines and locomotive torpedoes, such as those marvels of ingenuity and constructive skill, the Whitehead and Brennan torpedoes, the important progress recently made in the practical development of explosive agents has not resulted in the provision of a material which equals wet compressed gun-cotton in combining with great destructive power the all-important essential of safety to those who have to deal with these formidable weapons, and to man the small vessels which have to perform the very hazardous service of attacking ships of war at short distances by means of locomotive torpedoes.

Although the subject of the development of explosive force for purposes of war has of late received from workers in applied science, from seekers of patentable inventions, and even from the public generally, a somewhat predominating share of attention, considering that we congratulate ourselves upon the enjoyment of a period of profound peace, yet the production of new explosive agents for mining and quarrying purposes, which present or lay claim to points of superiority over the well-established blasting-agents, has been by no means at a standstill. For many years the main object sought to be achieved in this direction was to surpass, in power or adaptability to particular classes of work, the well-known preparations of nitro-glycerine and gun-cotton, which, during the past twenty years, have been formidable competitors and, in many directions, absolutely successful rivals, of black powder. It is both interesting and satisfactory to note, however, that this object has of late, and especially since the publication of the results of labours of English and foreign Commissions on the causes of mine-accidents, been prominently associated with endeavours to solve the important problems of combining, in an explosive agent, efficiency in point of power with comparative non-sensitiveness to explosion by friction or percussion, and of securing its effective operation with little or no accompaniment of projected flame. Safety-dynamites, flameless explosives, water-cartridges, and other classes of materials and devices connected with the getting of coal, the quarrying of rock, or the blasting of minerals, have claimed the attention of those who guide the miner's work; in some of these directions the practical results obtained have been beyond question important, and, indeed, conclusive as regards the great diminution of risks to which men need be exposed in those coal-mines where the ordinary use of explosives, although not altogether inadmissible, may at times be attended with danger. It is to be feared that those results are still far from receiving the amount of application which might reasonably be hoped for; but, at any rate, there are, among the extensive mining districts where the employment of explosives in connection with the getting of coal cannot be dispensed with, several of importance where the use of gunpowder has almost entirely given place to the adoption of blasting-agents or methods of blasting, the employment of which is either not, or only very exceptionally, attended by the projection of flame or incandescent matter into the air where the shot is fired.

The mining public is especially indebted to German workers for much of the success which has been obtained in this direction, and also to the eminent French authorities, Mallard and Le Chatelier, for their thorough theoretical and practical investigations bearing upon the prevention of accidental ignition of fire-damp during blasting operations. Having arrived at the conclusion that fire-damp- and air-mixtures are not ignited by the firing of explosive preparations which develop by their detonation temperatures lower than 2220° C., they found that ammonium-nitrate, although in itself susceptible of detonation, does not develop a higher temperature than 1130° C., while the temperature of detonation of nitro-glycerine and gun-cotton are, respectively, 3170° and 2636°. The admixture of that salt with nitro-glycerine or gun-cotton in sufficient proportion to reduce the temperature of detonation to within safe limits allows, therefore, of the employment of those explosive agents in the presence of fire-damp mixtures without risk of accident, and this fact has led to the effective use of such mixtures as safe blasting-agents in coal.

Those who have been content to labour long and arduously with the objects steadily in view of advancing our knowledge of the causes of mine-accidents and of developing resources and measures for removing or combating those causes, can cherish the conviction that recent legislation in connection with coal-mines, based upon the results of those labours, has been already productive of decided benefits to the miner, even although it has fallen short of what might reasonably have been

hoped for as an outcome of the very definite results and conclusions arrived at by the late Royal Commission on Accidents in Mines (in the recent much-lamented death of whose universally respected chairman, my late esteemed friend and colleague, Sir Warington Smyth, the scientific world has sustained the loss of an ardent worker, and the miner, of an invaluable friend).

The fearful dangers arising from the accumulation of inflammable dust in coal-mines, and the equality of mine-dust with fire-damp in its direful power of propagating explosions, which may sometimes even be, in the first instance, established chiefly or entirely through its agency, have now been long recognized as beyond dispute; and it is satisfactory to know that permission to fire shots in mine-workings which are dry and dusty has, by recent legislation, been made conditional upon the previous laying of the dust by effective watering. In some mining districts, moreover, the purely voluntary practice has been extensively adopted by mine-owners of periodically watering the main roads in dry and dusty mines, or of frequently discharging water-spray into the air in such roads, which must tend greatly to reduce the possible magnitude of the disastrous results of a fire-damp or dust explosion in any part of the mine-workings.

The encouragement given to the application of the combined resources of ingenuity, mechanical skill, and knowledge of scientific principles, through the elaborate, but thoroughly practical comparative trials to which almost every variety of safety-lamp has, during the last few years, been submitted by competent and conscientious experimenters, has resulted in the provision of lamps to the hand of the miner which combine the essential qualities of safety, under the most exceptionally severe conditions, with good illuminating power, simplicity of construction, lightness, and moderate cost. Very important progress has also been made, since the first appointment of the late Accidents in Mines Commission, towards the provision of thoroughly serviceable and safe portable electric lamps for use in mines. Of those which have already been in the hands of the miners, several have fairly fulfilled his requirements as regards size, weight, and illuminating power of sufficient duration; but much still remains to be accomplished with respect to durability, simplicity, thorough portability, and cost, before the self-contained electric lamp can be expected to compete successfully with the greatly improved miners' lamps which are now in use, or available.

The recent legislation in connection with mines is certainly deficient in any sufficiently decisive measure for excluding from mine-workings certain forms of lamps which, while fairly safe in the old days of sluggish ventilation, are unsafe in the rapid air-currents now frequently met with in mines; it is, however, very satisfactory to know that the strong representations on this subject made by the late Commission, combined with the force of example and with the conclusive demonstration of the superiority of other lamps, by exhaustive experiments, have led within the last two years to the very general abandonment of the unprotected Davy, Clanny, and Stephenson lamps in favour, either of simple, safe modifications of these, or of other safe and efficient lamps, and that one possible element of danger to the miner has thus been eliminated, at any rate in many districts. In one important respect recent improved legislation has failed to effect a most desirable change—namely, in the substitution of safety-lamps for naked lights in workings where small local accumulations of fire-damp are discovered from time to time. There appears little doubt that one of the three fearful explosions which have occurred within the last twelve months—the explosion at Llanerch Colliery, near Pontypool—was caused by the continued employment of naked lights in a mine where inspection constantly revealed the presence of fire-damp. This, and two other terrible disasters, at Mossfield Colliery, in Staffordshire, at Morfa Colliery, near Swansea, which have occurred since the last meeting of the Association, may have seemed to weaken the belief that the operation of the recent Mines Regulation Act, which was based upon some of the results of seven years' arduous labour of the late Mines Commission, must have resulted in very substantial improvement in the management of mines and in the conduct of work by the men. Happily, however, there is a consensus of opinion among those most competent to judge—*i.e.* the Government Mine Inspectors—that very decided benefits have already accrued from the operation of the new Act. Although far from embodying all that the experienced mine-owners, miners, and scientific workers upon that Commission, as well as practical authorities in Parliament, concurred in regarding as reasonably adaptable, from the results of observation and experiment, to the furtherance of the safer working of mines, this Act does include measures, precautionary and preventive, of undeniable utility, well-calculated to lessen the dangers which surround the miner, and to add to his personal comfort underground. We may hope, moreover, that the operation of the Act is paving the way to more comprehensive legislation in the near future; for it can scarcely be doubted, by the light of recent sad experience, that there are directions in which both masters and men still hesitate to adopt, of their own free will, measures or regulations, methods of working or appliances and precautions, which are calculated to be important additional safeguards against mine-accidents, and which are either left untouched, or only hesitatingly and imperfectly dealt with in the recent enactments.

My labours upon the late Mines Commission represent only one of several subjects in connection with which it has been my good fortune to have opportunities of rendering some slight public service in directions contrasting with one of the main functions of my career, by endeavouring to apply the results of scientific research to a diminution of the risks to which particular classes of the community, or the public at large, are exposed—of being sufferers by explosions, the results of accidents or other causes.

During the pursuit of bread-winning vocations, and even in ordinary domestic life, the conditions, as well as the materials, requisite for determining more or less disastrous explosions are often ready to hand, and their activity may be evoked at any moment through individual heedlessness or through pure accident. Steam, or gases confined under pressure, volatile inflammable liquids, combustible gases, or finely-divided inflammable solids, are now all well recognized as capable of assuming the character of formidable explosive agents; but with respect to the three last-named, it is only of late that material progress has been made towards a popular comprehension and appreciation of the conditions conducive to danger, and of those by the fulfilment of which danger may be avoided. Thus, the causes of explosions in coal-laden ships, together with the occurrence of spontaneous ignition in coal-cargoes, another fruitful source of disaster, were made the subject of careful inquiry some years ago by a Royal Commission, upon which I had the pleasure of working with the late Dr. Percy, whose invaluable labours for the advancement of metallurgic science will always be gratefully remembered. The light thrown by that inquiry upon the causes of those disasters, and upon the conditions to be fulfilled for guarding against the accumulations of fire-damp, gradually escaping from occlusion in coal, and of heat, developed by chemical changes occurring in coal-cargoes, has unquestionably led to an important reduction of the risks to which coal-laden ships are exposed. Subsequent official inquiries and experimental investigations, in which I took part with the late Sir Warington Smyth and some eminent naval officers, consequent upon the loss of H.M.S. *Doterel* through the accidental ignition of an explosive mixture of petroleum spirit-vapour and air (and other calamities in war-ships originating with the gradual emission of fire-damp from coal), have resulted in the adoption of efficient arrangements for ventilating all spaces occupied by, and contiguous to, the large supplies of fuel which these vessels have to carry.

The thorough investigation, by Rankine and others, of the causes of explosions in flour-mills, which in years past were so frequent and disastrous, has secured the adoption of efficient measures for diminishing the production, and the dissemination through channels and other spaces in the mills, of explosive mixtures of flour-dust and air, and for guarding against their accidental ignition. The numerous terrible accidents caused by the formation and accidental ignition of explosive mixtures of inflammable vapour and air in ships carrying cargoes of petroleum stored in barrels or in tanks, have, by the investigations to which they have given rise, led to the indication of effective precautionary measures for guarding against their recurrence. Again, the many distressing accidents, frequently fatal, which have attended the domestic use of those valuable illuminants, petroleum and mineral oils of kindred character, have been made the subject of exhaustive investigations, which have demonstrated that these disasters may readily be prevented by the employment of lamps of proper construction, and by the observance of very simple precautions by the users of them; and a

recent official inquiry which I have conducted with Mr. Boverton Redwood has furnished most gratifying proof that very substantial progress has been made within the last few years by lamp-manufacturers in the voluntary adoption of such principles of construction as we had experimentally demonstrated to be essential for securing the safe use of mineral oils in lamps for lighting and heating purposes, the employment of which has, within a brief period, received enormous extension in this and other countries.

The creation and rapid development of the petroleum industry has, indeed, furnished one of the most remarkable illustrations which can be cited of industrial progress during the period which has elapsed since the British Association last met in Leeds. One year after that meeting, viz. on August 28, 1859, the first well, drilled in the United States with the object of obtaining petroleum, was successfully completed, and the rate of increase in production in the Pennsylvania oil-fields during the succeeding years is shown by the following figures:—

In 1859, 5000 barrels (of forty-two American gallons) were produced In the following year the production increased to 500,000 barrels; while in the next year (1861) it exceeded 2,000,000 barrels, at which figure it remained, with slight fluctuations, until 1865. The supply then continued to increase gradually, until, in 1870, it reached nearly 6,000,000 barrels; while in 1874 it amounted to nearly 11,000,000 barrels. In 1880 it amounted to over 26,000,000 barrels, and in 1882 it reached 31,000,000. Since then the supply furnished by the United States has fallen somewhat, and last year it amounted to 21,500,000 barrels. The production of crude petroleum in the Pennsylvanian fields, large as it has been, has not, however, kept pace with the consumption, for we find that the accumulated stocks, which on December 31, 1888, amounted to over 18,000,000 barrels, had become reduced to about 11,000,000 barrels at the close of last year. At this rate the surplus stock above ground will have vanished by the end of the current year. In addition to the petroleum raised in Pennsylvania, there is now a very large production in the State of Ohio; but this has not as yet been employed as a source of lamp-oil; it is, however, transported by pipe line in great quantities to Chicago, for use as liquid fuel in industrial operations.

A few years after the development of the United States petroleum-industry, the production of crude petroleum in Russia also began to extend very rapidly. For more than 2500 years, Baku, on the borders of the Caspian Sea, has been celebrated for its naphtha springs, and for the perpetual flames of the Fire Worshippers, fed by the marvellous subterranean supplies of natural gas. To a limited extent neighbouring nations appear to have availed themselves of the vast supplies of mineral oil at Baku during the past thousand years. By the thirteenth century the export of the crude oil had already become somewhat extensive, but the production of petroleum from it by distillation is of comparatively recent date. In 1863 the supplies of petroleum from the Baku district amounted to 5018 tons; they increased to somewhat more than double during the succeeding five years. In 1869 and following three years the production reached about 27,000 tons annually, and in 1873 it was about 64,000 tons; three years later, 153,000 tons were produced, and in the following five years there was a steady annual increase, until, in 1882, the production amounted to 677,269 tons; in 1884 it considerably exceeded 1,000,000 tons, and last year it had reached the figure of about 3,300,000 tons. The consumption of crude petroleum as fuel for locomotive purposes has, moreover, now assumed very large proportions in Russia, and many millions of gallons are annually consumed in working the vast system of railways on both sides of the Caspian Sea.

The imported refined petroleum used in this country in lamps for lighting, heating, and cooking, was exclusively American until within the last few years, but a very large proportion of present supplies comes from Russia. The imports of kerosene into London and the chief ports of the United Kingdom during 1889 amounted to 1,116,205 barrels of United States oil, and 771,227 barrels of Russian oil. During the same period the outturn of mineral oil for use in lamps by the Scottish shale oil companies probably amounted to about 500,000 barrels.

Another important feature connected with the development of the petroleum industry is the great extent to which the less volatile products of its distillation have replaced vegetable and animal oils and fats for lubricating purposes in this and other countries. The value of petroleum as a liquid fuel and as a source of gas for illuminating purposes has, moreover, been long since recognized, and it is probable that one outcome of the attention which is now being given to the hitherto unworked deposits of petroleum in the East and West Indies, South America, and elsewhere, will be a very large increase in its application to these purposes. In the East Indies there are vast tracts of oil-fields in Burmah, Baluchistan, Assam, and the Punjab. The native Rangoon oil industry is one of great antiquity, although the oil was only used in the crude condition until about thirty-five years ago, at which time Dr. Hugo Müller, with the late Warren De la Rue, whose many-sided labours and generous benefactions have so importantly contributed to the advancement of science, made valuable researches on the products furnished by crude oil imported from Rangoon. The resources of the oil-fields of Upper Burmah, especially of the district of Yenangyoung (or *creek of stinking water*), have since then been developed by British enterprise, and have attained to considerable importance since our annexation of Upper Burmah.

The great extension of the petroleum trade is gradually leading to very important improvements in the system of transport of the material over water and on land. Until recently this has been carried out entirely in barrels and tin cases; the consequent great loss from leakage and evaporation, accompanied by risk of accident, is now becoming much reduced by the rapidly-increasing employment of tank-steamers, which transport the oil in bulk. Tank railway-waggons have for some time past been in use in Russia, and there is a prospect of these and of tank-barges being adopted here for the distribution of the oil; while in London, the practice is already spreading gradually of distributing supplies to tradesmen from tank road-waggons. Some considerable doubt as to whether the risk of accident has not rather been altered in character than actually reduced by the new system of transport, has not unnaturally been engendered in the public mind by the occurrence within a comparatively short period of several serious disasters during the discharge of cargoes from tank-vessels. The memorable explosion which took place, in October 1888, on board the *Ville de Calais*, in Calais Harbour, with widespread destructive effects, was followed by a similarly serious explosion in the *Fergusons*, at Rouen, last December, and, more recently, by a fire of somewhat destructive character at Sunderland, resulting from the discharge into the river of petrolem-residues from a ship's tanks. In all these cases the petroleum was of a nature to allow inflammable vapour to escape readily from the liquid, so that an explosive mixture could be rapidly formed by its copious diffusion through the air. No similar casualty has been brought to notice as having happened to tank-ships carrying petroleum oil of which the volatility is in accordance with our legal requirements, and this points to the prudence of restricting the application of the tank system to the transport and distribution of such petroleum as complies with well-established conditions of safety.

Another most remarkable feature connected with the development of the petroleum industry is presented by the utilization, within the last few years, of the vast supplies of natural inflammable gas furnished by the oil-fields.

In America this remarkable gas-supply was for a long time only used locally, but, before the close of 1885 its conveyance to a distance by pipes, for illuminating and heating purposes, had assumed large proportions; one of the companies in Pittsburgh having alone laid 335 miles of pipes of various sizes, through which gas was supplied equivalent in heating value to 3,650,000 tons of coal per annum. Since then the consumption in and around Pittsburgh has probably been at least tripled. At the close of 1886 six different companies were conveying natural gas by pipes to Pittsburgh from 107 wells; 500 miles of pipe, ranging in diameter from 30 inches to 3 inches, were used by these companies, 232 miles of which were laid within Pittsburgh itself. The Philadelphia Company, the most important of these associations, then owned the gas supply from 54,000 acres of land situated on all the anticlinals around Pittsburgh, but drew its supplies only from Tarentum and the Murraysville field. It supplied, in 1886, 470 factories and about 5000 dwellings within the city, besides many factories and dwellings in Alleghany, and in numerous neighbouring villages. The average gas-pressure at the wells, when the escape is shut off, is about 500 lbs. per square inch, and in the case of new wells this pressure is very greatly exceeded. In order to minimize the danger from leakage the gas-pressure in the city is reduced to a maximum of 13 lbs., and is regulated by valves at a number of stations under the control of a central station. The usual pressure in the larger

lines is from 6 to 8 lbs., while in the low-pressure lines it does not exceed 4 to 5 ounces.

The effect of the change from coal-gas to natural gas upon the atmosphere over Pittsburgh has been most marked: formerly the sky was constantly obscured by a canopy of dense smoke; now the atmosphere is clear, and even white paint may with impunity be employed for the house fronts.

The very rapid development of the employment of natural gas is not confined to the neighbourhood of Pittsburgh; it is used for heating purposes in the cities of Buffalo, Erie, Jamestown, Warren, Olean, Bradford, Oil City, Titusville, Meadville, Youngstown, and perhaps twenty more towns and villages in Pennsylvania and North-western New York. In North-western Ohio, the cities of Toledo and Sandusky, the towns of Findlay, Lima, Tiffin, Fostoria, and others in that section are also supplied with natural gas; a pipe line has moreover been recently laid to Detroit, Mich., and it is estimated that in these localities 36,131,669,000 cubic feet of the gas were consumed during last year, displacing 1,802,500 tons of coal. To the south-west of Pittsburgh there are many smaller places which consume natural gas; it also occurs in considerable quantity, and is being utilized, in Indiana (whence an account has recently reached us of a terrific subterranean explosion of the gas); and it is at the present time contemplated to carry a natural gas-supply to Chicago.

The utilization of the natural gas of the Russian oil-fields, although of very ancient date, has hitherto not been extensive, neither does the magnitude of the supply appear to bear comparison with that of the Pennsylvanian district.

A form of gaseous fuel which has long been known to technical chemists and metallurgists, but which has of late attracted considerable attention, especially in connection with the recent interesting work relating to its applications pursued by Mr. Samson Fox, of Leeds, has become, within the last four years, a competitor, in the United States, both of the natural gas of Pennsylvania and of coal-gas. Since Felix Fontana first produced so-called water-gas in 1780, by passing vapour of water over highly-heated fuel, many methods, differing chiefly in small details, have been proposed for carrying out the operation, with a view to the ready and cheap production of the resulting mixture of hydrogen and carbonic oxide, and numerous technical applications of water-gas have been suggested from time to time, with no very important results, excepting as regards its use for lighting-purposes. Being of itself non-luminous, its utilization in this direction is accomplished, either by mixing it with a highly luminous gas, or by causing a hydrocarbon vapour to be diffused through it; or the non-luminous flame, produced by burning it in the air, is made to raise to incandescence some suitably prepared solid substance, such as magnesia, lime, a zirconium salt, or platinum, whereby bright light is emitted. The objection to its employment as an illuminant for use in buildings, to which great weight is attached by us, and rightly, as sad experience has shown—viz. that, as it consists, to the extent of about one-half its volume, of the highly poisonous gas carbonic oxide, the atmosphere in a confined space may be rendered irrespirable by a small accidental contamination with water-gas, by leakage or otherwise, not detectable by any odour—appears to constitute no great impediment to its employment in the United States, as it is now manufactured for illuminating and heating purposes by a large proportion of their gas-works, being in some places employed in admixture with a highly luminous coal-gas, in others rendered luminous by the alternative methods mentioned. It is stated that about three-fourths of the illuminating gas now supplied to the cities of New York, Brooklyn, Philadelphia, Jersey, St. Paul, and Minneapolis, is carburetted water-gas; in Chicago the entire supply now consists of this gas, and Boston will also soon be supplied exclusively with it. The use of water-gas for metallurgic work does not appear to be contemplated in the United States, but it is especially to such applications of the gas that much attention has been devoted here in Leeds; and although some eminent experts are sceptical regarding the attainment of advantages, especially from an economical point of view, by the employment of this form of gaseous fuel, especially after practical experience in the same direction acquired in Germany, the technical world must feel grateful to Mr. Fox for his work in this direction, affording, as it does, an interesting illustration of the qualities of perseverance and energy which, when combined with sound knowledge, often achieve success in directions that have long appeared most unpromising—qualities which have been characteristic of many pioneers in industrial progress in this country.

Leeds has been especially fortunate in the possession of such pioneers, who, when competition brought about great changes in the particular trade through which, for many generations, this city chiefly enjoyed prosperity and high renown, developed its power and resources in new directions, from which success soon flowed in continually increasing measure. The rapid rise of Leeds to its present high position in industrial prosperity and national importance most probably dates from the period when its chief staple industry began to experience serious rivalry, in its own peculiar achievements, on the part of other districts of the kingdom and of other countries. From early days a flourishing centre of one of the provinces of Great Britain most richly endowed with some of Nature's best treasures, Leeds could scarcely have failed, through the energy, acute intelligence, and powerful self-reliance especially characteristic of the men of Yorkshire, to rapidly acquire fresh renown in connection with industries which either were new to the town and district, or had been pursued in comparatively modest fashion, and which have combined to place the Leeds of to-day upon a higher pinnacle of commercial prosperity, power, and influence than her patriotic citizens of old could ever have dreamt of.

An examination into the present educational resources of Leeds places beyond any doubt the fact that her present prosperity in commerce and industries is in no small degree ascribable to the paramount importance long since attached here to the liberal provision of facilities for the diffusion of knowledge among the artisan- and industrial-classes, and especially for the acquisition of a sound acquaintance with the principles of the sciences and their applications to technical purposes, with particular reference to the prominent local industries, by all grades of those who pursue or intend to pursue them. There is, probably, no town in the kingdom more amply provided with efficient elementary and advanced schools for both sexes, while the special requirements of the artisan are efficiently met by the prosperous School of Science and Technology. The resources of the Yorkshire College provide, in addition, a combination of thorough scientific education with really practical training in the more important local industries; indeed, during the sixteen years of its continually-progressive work, this institution has acquired so widespread a reputation that students come from abroad to reap the advantages afforded by the unrivalled textile and dyeing departments of the Leeds College. The keen competition now existing between these departments and the corresponding branches of the much younger but most vigorous sister College at Bradford, can only conduce to the further development of both, and to their thorough maintenance up to the requirements of the day.

The very important pecuniary aid afforded to these establishments, and to a number of other technical schools in Yorkshire, by one of the most important of the ancient Companies of the City of London, the Clothworkers, affords an interesting illustration of the good work in the cause of education performed by those Guilds, and, especially of late years, by means of their flourishing Institute for the advancement of technical education, which, through its two great instructional establishments in London, and through the operation of its system of examinations throughout the country, extending now even to the colonies, has afforded very important aid towards eradicating the one great blot upon our national educational organization. To have been first in the field in practically developing a far-reaching scheme for the advancement of technical education in this country must continue to be a source of pride to the City of London and its ancient Guilds in time to come, when the operation of efficient legislation, supported and extended by patriotic munificence and by the hearty co-operation of associations of earnest and competent workers in the cause, shall have placed the machinery and resources for the technical instruction of the people upon a footing commensurate with our position among nations.

The remarkable address delivered by Owen here in 1858, wherein the condition, at that time, of those branches of natural science which he had made particularly his own was most comprehensively reviewed, included some especially interesting observations on the importance to the cultivation and progress of the natural sciences, and to the advancement of education of the masses in this country, of providing adequate space and resources for the proper development of our national Museum of Natural History; and it cannot but be a source of great satisfaction and pride to him to have lived to witness the thoroughly successful realization of the objects of his own

indefatigable strivings and powerful advocacy in that direction. Comprehensive as were the views adopted by Owen regarding the scope and possible extension of that Museum, it may, however, be doubted whether they ever embraced so extensive a field as was presented for our contemplation by his successor last year, when he told us that a natural history museum should, in its widest and truest sense, represent, so far as they can be illustrated by museum-specimens, all the sciences which deal with natural phenomena, and that the difficulties of fitly illustrating them have probably alone excluded such subjects as astronomy, physics, chemistry, and physiology, from occupying departments in our national Museum of Natural History.

The application, in its broadest signification, of the title, Natural History Museum, may doubtless be considered to include, not only illustrations and examples of the marvellous works of the Creator and of the results of man's labours in tracing their intimate history and their relations to each other, but also illustrations of the means employed, and of the results attained, by man in his strivings to fathom and unravel the laws by which the domains of Nature are governed. But the reason why representative collections, illustrative of the physical sciences, do not form part of our national Natural History Museum, has, I venture to think, scarcely been correctly ascribable to any difficulty of organizing fit illustrations of methods of investigation, of the attendant appliances, and of the results obtained by experimental research; it appears, rather, to exist in the fact that physical science has hitherto had no share in such a combination of circumstances as has been favourable to the good fortunes and advancement of the natural sciences, and as is analogous to those which, from time to time, give rise to the provision of increased accommodation for our national art treasures. Our present national Science Collection, which has, indeed, had a struggle for existence, does not owe the development it has hitherto experienced to any such moral pressure as has been several times exercised in the case of our art collections, by the munificence of individuals, with the result of securing substantial aid from national resources; its gradual increase in importance has been due to the untiring perseverance of men of science, and of a few prominent influential and public-spirited authorities, in keeping before the public the lessons taught by careful inquiries, such as those intrusted to the Royal Commission on Scientific Instruction, into the opportunities afforded for the cultivation of science and the development of its applications, in other countries, as compared with those provided here.

The success of the efforts made in 1875 by a committee thoroughly representative of every branch of experimental science, to bring together in London an international loan collection of scientific apparatus, and the widespread interest excited by that collection, led the President of the Royal Society, in union with many distinguished representatives of science, to lay before our Department of Education a proposal to establish a national museum of pure and applied science, including the Museum of Inventions, which had already existed since 1860 as a nucleus of a science museum, the establishment whereof had formed part of the original scheme of the Science and Art Department. The Loan Collection of 1876 did, in fact, and in consequence of the urgent representations then made, first put into practical shape the long-cherished desire of men of science to see an institution arise in England similar to the Conservatoire des Arts et Métiers of France, and it became the starting-point of the national collection, representative of the several branches of experimental science, which has been undergoing slow but steady development since that time, patiently awaiting the provision of a suitable home for its contents. This collection, which illustrates not only the means whereby the triumphs of research in experimental science have been and are achieved, but also the methods by which these departments of science are taught, yields, small as it is, to none of our national museum-treasures in interest and importance.

In yet another way did that Loan Collection become illustrious: one of the most interesting features connected with it was the organization of a series of important conferences and explanatory lectures, serving to illustrate, and also greatly to enhance, its value, and affording most invaluable demonstration of the way in which such collections must exercise direct influence upon the advancement of science and upon the diffusion of scientific knowledge. These lectures and conferences demonstrated the wisdom of the suggestion made by the illustrious representative of associated science in Leeds eighteen years previously, that public access to museums should be combined with the delivery of lectures emphasizing and amplifying the information afforded by their contents. The example there set of thoroughly utilizing for instructional purposes, and for the advancement of science, a collection illustrative of the physical sciences, has since been followed by the Science and Art Department; illustrative lectures connected with the existing nucleus of a national science collection, have been delivered from time to time, and the objects in the collection are constantly utilized in the courses of instruction of the adjoining Normal School of Science.

Although the national importance of thoroughly representative and continously-maintained science collections has long been manifest, not only to all workers in science, but also to all who have cared to inquire, even superficially, into the influence of the cultivation of science upon the industrial and commercial prosperity of the country, the labours of a Royal Commission, and of successive Committees, in demonstrating the necessity for the provision of adequate accommodation for such collections, and for their support upon the basis of that afforded to the natural history collections, have been very long in bearing fruit. However, lovers of science, and those who have the prosperity of the country near at heart, have at length cause for rejoicing at the acquisition by the nation of a site in all respects suitable and adequate for the accommodation of the science collections, which, as soon as appropriate buildings are provided for their reception, will not fail, in comprehensiveness and completeness, to become worthy of a country which has been the birthplace of many of the most important discoveries in science, and of a people who have led the van among all nations in making the achievements of science subservient to the advancement of industries and commerce.

The site selected as the permanent home of our national Science Collections is immediately in rear of the Natural History Museum, and faces the stately edifice, now rapidly progressing towards completion, for the erection of which, as an Imperial memorial of the Queen's Jubilee, funds were provided by voluntary contributions from every portion of the Empire and every class in the Empire's nations. The Imperial Institute, the conception of which we owe to His Royal Highness the Prince of Wales, occupies a central position among buildings devoted to the illustration and cultivation of pure and applied science and of the arts—*i.e.* the Normal School of Science, the Technical College of the City and Guilds of London, the National Schools of Art, the Science Museum, the South Kensington Museum, and the Royal College of Music; to which we may ere long see added a National Gallery of representative British Art. A more fitting location could scarcely be conceived for this pre-eminently national institution, which has for its main objects the comprehensive and continuously progressive illustration—of the practical applications of the vast resources presented by the animal, vegetable, and mineral kingdoms to industries and the arts; of the extent, and the progressive opening up, of those resources in all parts of the Empire; of the practical achievements emanating from the results of scientific research; and of the utilization of the arts for the purposes of daily life. With the attainment of these objects it will be the function of the Imperial Institute to combine the continuous elaboration of systematic measures tending to stimulate progress in trades and handicrafts, and to foster a spirit of emulation among the artisan and industrial classes. Another branch of the Institute's work, upon which it is already engaged, is the systematic collection of data relating to the natural history, commercial geography, and resources of every part of the Empire, for wide dissemination, together with all current information bearing upon the commerce and industries of the Empire and of other countries, which can be comprised under the head of Commercial Intelligence. The achievement of these objects should obviously tend to maintain intimate intercourse, relationship, and co-operation between the great home and colonial centres of commerce, industries, and education, and to enhance importantly our power of competing successfully in the great struggle, in which nations are continuously engaged, for supremacy in commercial and industrial enterprise and prosperity.

To the elaboration of the practical details of a system of operation calculated to secure the objects I have indicated, eminent public-spirited men are now devoting their best energies, with the sanguine expectation of realizing the hope cherished by the Royal Founder of the Imperial Institute, that this memorial of the completion, by our beloved Sovereign, of fifty years of a wise and prosperous reign, is destined to be one of the most important bulwarks of this country, its colonies and depend-

encies, by becoming a great centre of operations, ceaselessly active in fostering the unity, and developing the resources, and thus maintaining and increasing the power and prosperity, of our Empire.

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. T. E. THORPE, B.SC., PH.D., F.R.S., TREAS.C.S., PRESIDENT OF THE SECTION.

LEEDS has one most notable association with chemistry of which she is justly proud. In the month of September 1767, Dr. Joseph Priestley took up his abode in this town. The son of a Yorkshire cloth-dresser, he was born, in 1733, at Fieldhead, a village about six miles hence. His relatives, who were strict Calvinists, on discovering his fondness for books, sent him to the academy at Daventry to be trained for the ministry. In spite of his poverty and of certain natural disadvantages of speech and manner, he gradually acquired, more especially by his controversial and theological writings, a considerable influence in Dissenting circles. A pressing invitation and the offer of one hundred guineas a year induced him to accept an invitation to take charge of the congregation of Mill Hill Chapel here. He was already known to science by his "History of Electricity," and the effort was made to attach him still more closely to its cause by the offer of an appointment as naturalist to Cook's second expedition to the South Seas. But, thanks to the intervention of some worthy ecclesiastics on the Board of Longitude who had the direction of the business, and who, as Prof. Huxley once put it, "possibly feared that a Socinian might undermine that piety which in the days of Commodore Trunnion so strikingly characterized sailors," he was allowed to remain in Leeds, where, as he tells us in his "Memoirs," he continued six years, "very happy with a liberal, friendly, and harmonious congregation," to whom his services (of which he was not sparing) were very acceptable. "In Leeds," he says, "I had no unreasonable prejudices to contend with, and I had full scope for every kind of exertion."[1]

We have every reason to feel grateful to the "worthy ecclesiastics," since their action indirectly occasioned Priestley to turn his attention to chemistry. The accident of living near a brewery led him to study the properties of "fixed air," or carbonic acid, which is abundantly formed in the process of fermentation, and which at that time was the only gas whose separate and independent existence had been definitely established. From this happy accident sprang that extraordinary succession of discoveries which earned for their author the title of the Father of Pneumatic Chemistry, and which were destined to completely change the aspect of chemical theory and to give it a new and unexpected development.

I have been led to make this allusion to Priestley, not so much on account of his connection with this place as for the reason that, as it seems to me, there has been a disposition to obscure his true relation to the marvellous development of chemical science which made the close of the last century memorable in the history of learning. Our distinguished fellow-worker, M. Berthelot, the Perpetual Secretary of the French Academy, has recently published, under the title of "La Révolution Chimique," a remarkable book, written with great skill, and with all the charm of style and perspicacity which invariably characterizes his work, in which he claims for Lavoisier a participation in discoveries which we count among the chief scientific glories of this country. From the eminence of M. Berthelot's position in the world of science, his book is certain to receive in his own country the attention which it merits, and as it is issued as one of the volumes of the Bibliothèque Scientifique Internationale, it will probably obtain through the medium of translations a still wider circulation. I trust that I shall not be accused of being unduly actuated by what Mr. Herbert Spencer terms "the bias of patriotism" in deeming the present a fitting occasion on which to bring these claims to your notice with a view of determining how far they can be substantiated.

All who are in the least degree familiar with the history of chemical science during the last hundred years will recognize, as I proceed, that the claims which M. Berthelot asserts on behalf of his illustrious predecessor are not put forward for the first time. Explicitly made, in fact, by Lavoisier himself, they were uniformly and consistently disallowed by his contemporaries. M. Berthelot now seeks to support them by additional evidence, and to strengthen them with new arguments, and asks us thereby to clear the memory of Lavoisier from certain grave charges which lie heavily on it. You have doubtless anticipated that these claims have reference to Lavoisier's position in relation to the discovery of oxygen gas and the determination of the non-elementary nature of water.

The substance we now call oxygen—a name we owe to Lavoisier—was discovered by Priestley on August 1, 1774; he obtained it, as every schoolboy knows, by the action of heat upon the red oxide of mercury. We all remember the characteristically ingenuous account which Priestley gives of the origin of his discovery. M. Berthelot sees in it merely the evidence of the essentially empirical character of his work. "Priestley," he says, "the enemy of all theory and of every hypothesis, draws no general conclusion from his beautiful discoveries, which he is pleased, moreover, not without affectation, to attribute to chance. He describes them in the current phraseology of the period with an admixture of peculiar and incoherent ideas, and he remained obstinately attached to the theory of phlogiston up to his death, which occurred in 1804" (p. 40). Such a statement is calculated to give an erroneous idea of Priestley's merit as a philosopher. That the implication it contains is wholly opposed to the real spirit of his work might be readily shown by numerous quotations from his writings. Perhaps this will suffice:—"It is always our endeavour, after making experiments, to generalize the conclusions we draw from them, and by this means to form a *theory* or system of principles to which all the *facts* may be reduced, and by means of which we may be able to foretell the result of future experiments." This quotation is taken from the concluding chapter of his "Experiments and Observations on Different Kinds of Air," in which he actually seeks to draw "general conclusions" concerning the constituent principles of the various gases which he himself made known to us, and to show the bearing of these conclusions on the doctrine of phlogiston. That he was content to rest in the faith of Stahl's great generalization, even to the end, is true, and the fact is the more remarkable when we recall the absolute sincerity of the man, his extraordinary receptivity, and, as he says of himself, his proneness "to embrace what is generally called the heterodox side of almost every question." If it is argued that this merely shows Priestley's inability to appreciate theory, it must be at least admitted that there is no proof that he was inimical to it. His position is clearly evident from the concluding words of the section of his work from which I have already quoted:—"This doctrine of the composition and decomposition of water has been made the basis of an entirely new system of chemistry, and a new set of terms has been invented and appropriated to it. It must be acknowledged that substances possessed of very different properties may, as I have said, be composed of the same elements in different proportions and different modes of combination. It cannot, therefore, be said to be absolutely *impossible* but that water may be composed of these two elements or any other. But then the supposition should not be admitted without *proof*; and if a former theory will sufficiently account for all the *facts* there is no occasion to have recourse to a new one, attended with no peculiar advantage (*loc. cit.*, p. 543). . . . I should not feel much reluctance to adopt the *new doctrine*, provided any new and stronger evidence be produced for it. But though I have given all the attention that I can to the experiments of M. Lavoisier, &c., I think that they admit of the easiest explanation on the *old system*" (*loc. cit.*, p. 563).

The fact that Priestley was the first to consciously isolate oxygen is not contested by M. Berthelot, although he is careful to point out, what is not denied, that the exact date of the discovery depends on Priestley's own statement, and that his first publication of it was made in a work published in London in 1775. It was known before Priestley's famous experiment that the red oxide of mercury, originally formed by heating the metal in contact with air, would again yield mercury by the simple action of heat and without the intervention of any reducing agent. Bayen, six months before the date of Priestley's discovery, had observed that a gas was thus disengaged, but he gave no description of its nature, contenting himself merely by pointing out the analogy which his experiments appeared to possess to those of Lavoisier on the existence of an elastic fluid in certain substances. Afterwards, when the facts were established, Bayen drew attention to

[1] Leeds still enjoys one of the fruits of Priestley's insatiable power of work in her admirable Proprietary Library. He seems to have suggested its formation, and was its first honorary secretary.

his earlier experiments, and claimed, not only the discovery of oxygen, but all that Lavoisier deduced from it. "But," says M. Berthelot, in reference to this circumstance, "his contemporaries paid little heed to his pretensions, nor will posterity pay more" ("La Révolution Chimique," p. 60).

M. Berthelot, however, does not dismiss Lavoisier's claims to a participation in the discovery in the same summary fashion. On the contrary, whilst not explicitly claiming for him the actual isolation, in the first instance, of oxygen, the whole tenor of his argument is to extenuate, and even to justify, his demand to be regarded as an independent discoverer of the gas. He begins by asserting that Lavoisier had already a presentiment of its existence in 1774, and he quotes, in support of this assumption, an abstract from Lavoisier's memoir, published in December 1774, in the *Journal de Physique* of the Abbé Rozier: "This air, deprived of its fixable portion (by metals during calcination), is in some fashion decomposed, and this experiment would seem to afford a method of analyzing the fluid which constitutes our atmosphere, and of examining the principles of which it is composed. . . . I believe I am in a position to affirm that the air, as pure as it is possible to suppose it, free from moisture and from every foreign substance, far from being a simple body, or element, as is commonly thought, should be placed, on the contrary, . . . in the group of the mixtures, and perhaps even in that of the compounds."

M. Berthelot further asserts that Lavoisier was at this time the first to recognize the true character of air, and he expresses his belief that it is probable that he would himself have succeeded in isolating its constituents if the path of inquiry had been left to him alone. It is no disparagement to Lavoisier's prescience to say that there is nothing in these lines, nor in the memoir of the repetition of Boyle's experiments on the calcination of tin to which they refer, to show that Lavoisier had made any advance beyond the position of Hooke and Mayow. It has been more than once pointed out that the chemists of the seventeenth century understood the true nature of combustion in air mnch better than their brethren of the last quarter of the eighteenth century. Hooke, in the "Micrographia," and Mayow, in his "Opera Omnia Medicophysica," indicated that combustion consists in the union of something with the body which is being burnt; and Mayow, both by experiment and inference, demonstrated in the clearest way the analogy between respiration and combustion, and showed that in both processes one constituent only of the air is concerned. He distinctly stated that, not only is there increase of weight attending the calcination of metals, but that this increase is due to the absorption of the same *spiritus* from the air that is necessary to respiration and combustion. Mayow's experiments are so precise, and his facts so incontestable, that, as Chevreul has said, it is surprising that the truth was not fully recognized until a century after his researches (*vide* Watts's "A Dictionary of Chemistry," by Morley and Muir, Art. "Combustion," p. 242).

It is now necessary to examine Lavoisier's claims rather more closely and in the light of M. Berthelot's book. A *résumé* of his work "On the Calcination of Tin" was given by Lavoisier to the Academy in November 1774, but the complete memoir was not deposited until May 1777. A careful comparison of an abstract of what was stated to the Academy in November 1774, contributed by Lavoisier himself, in December 1774, to the *Journal de Physique* of the Abbé Rozier, makes it evident that very substantial additions were made to the communication before it was finally printed in the *Mémoires de l'Académie des Sciences*. The possibility of this is allowed by M. Berthelot. He says:—"A summary communication, often given *vivâ voce* to a learned Society, such as the Academy of Sciences of Paris or the Royal Society of London, would immediately call forth verifications, ideas, and new experiments, which would develop the range and even the results of such communication. The original author, when printing his memoir, would in return —and for this he is hardly blamable—embody these additional results and later interpretations. It thus becomes most difficult to assign impartially to each his share in a rapid succession of discoveries" (*loc. cit.*, p. 58).

But although, as we shall see, Lavoisier was certainly aware of Priestley's great discovery, no allusion is made to the gas, nor to Priestley's previous work on the other constituent of air, which is printed in the Philosophical Transactions for 1772, and for which he was awarded the Copley Medal by the Royal Society. It is simply impossible to believe that Lavoisier could have been uninfluenced by this work. Indeed, we venture to assert that the full and clear recognition of the non-elementary nature of the air which he eventually made was based upon it. It is noteworthy that in the early part of his memoir he states his opinion that the addition, not only of powdered charcoal, but of any phlogistic substance to a metallic calx is attended with the formation of fixed air. It is certain that at this period he had not only not consciously obtained any gas resembling Priestley's dephlogisticated air from any calx with which he had experimented, but that none of his experiments had afforded him any idea that the gas absorbed during calcination was identical with it.

At Easter 1775, Lavoisier presented a memoir to the Academy "On the Nature of the Principle which Combines with Metals during Calcination." This was "*relu le* 8 *août*, 1778." To the memoir there is a note stating that the first experiments detailed in it were performed more than a year before; those on the red precipitate were made by means of *a burning-glass* in the month of November 1774, and were repeated in the spring of 1775 at Montigny in conjunction with M. Trudaine. In this paper Lavoisier first distinctly announces that the principle which unites with metals during their calcination, which increases their weight, and which transforms them into calces, is nothing else "than the purest and most salubrious part of the air; so that if that air which has been fixed in a metallic combination again becomes free, it reappears in a condition in which it is eminently respirable, and better adapted than the air of the atmosphere to support inflammation and the combustion of substances" ("Œuvres de Lavoisier," official edition, vol. ii. p. 123). He then describes the method of preparing oxygen by heating the red oxide of mercury, and compares its properties with those of fixed air. There is, however, no mention of Priestley, nor any reference to his experiments. It can hardly be doubted that in this memoir Lavoisier intended his readers to believe that he was "the true and first discoverer" of the gas which he afterwards named oxygen. This is borne out by certain passages in his subsequent memoir "On the Existence of Air in Nitrous Acid; *lu le* 20 *avril*, 1776, *remis en décembre* 1777." He had occasion incidentally to prepare the red oxide of mercury by calcining the nitrate, and says that he obtained from it a large quantity of an air "much purer than common air, in which candles burnt with a much larger, broader, and more brilliant flame, and which in no one of its properties differed from that which *I* had obtained from the calx of mercury, known as *mercurius præcipitatus per se*, and which Mr. Priestley had procured from a great number of substances by treating them with nitric acid."

In another part of this memoir he says that "perhaps, strictly speaking, there is nothing in it of which Mr. Priestley would not be able to claim the original idea; but as the same facts have conducted us to diametrically opposite results, I trust that, if I am reproached for having borrowed my proofs from the works of this celebrated philosopher, my right at least to the conclusions will not be contested." M. Berthelot remarks on the irony of this passage: we may infer from it that the friends of the English chemist had not been altogether idle. In his memoir "On the Respiration of Animals," read to the Academy in 1777, he again appears to admit the claim of Priestley to at least a share in the discovery: "It is known from Mr. Priestley's and my experiments that *mercurius præcipitatus per se* is nothing but a combination," &c. In several subsequent communications Priestley's name is mentioned in very much the same connection, until we come to the classical memoir "On the Nature of the Acids," when it is said: "I shall henceforth designate the dephlogisticated air, or the eminently respirable air, . . . by the name of the *acidifying principle*, or, if it is preferred to have the same signification under a Greek word, by that of the '*principe oxygine*.'"

In none of the memoirs after that of Easter 1775 is the claim for participation more than implied; it is made explicitly for the first time in the paper "On the Method of Increasing the Action of Fire," printed in the *Mémoires de l'Académie* for 1782, and in these words:—"It will be remembered that at the meeting of Easter 1775 I announced the discovery, which I had made some months before with M. Trudaine,[1] in the laboratory at Montigny, of a new kind of air, up to then absolutely unknown, and which we obtained by the reduction of *mercurius præcipitatus per se*. This air, which Mr. Priestley discovered at very nearly the same time as I, and I believe even before me, and which he had procured mainly from the combination of minium

[1] M. Trudaine de Montigny died in 1777.

and of several other substances with nitric acid, has been named by him *dephlogisticated air*."

In the "Traité Elémentaire de Chimie" the claim for participation is again asserted in these words: "This air, which Mr. Priestley, Mr. Scheele, and I discovered at about the same time . . ."

Now, there is no question that Lavoisier knew of the existence of oxygen some months before he made the experiments with the burning-glass of M. Trudaine at Montigny, *for the simple reason that Priestley had already told him of it*. Priestley left Leeds in 1773 to become the librarian and literary companion of Lord Shelburne, and in the autumn of 1774 he accompanied his lordship on to the Continent, and spent the month of October in Paris. Lavoisier was famous for his hospitality; his dinners were celebrated; and Priestley, in common with every foreign *savant* of note who visited Paris at that period, was a welcome guest. What followed is best told in Priestley's own words:—"Having made the discovery [of oxygen] some time before I was in Paris, in the year 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people of the city were present, saying that it was a kind of air in which a candle burnt much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Mrs. Lavoisier as much as any, expressed great surprise. I told them I had gotten it from *precipitate per se* and also from *red lead*. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plombe rouge*, which was not understood till Mr. Macquer said I must mean *minium*."

In his account of his own work on dephlogisticated air, given in his "Observations," &c., 1790 edition, he further says, vol. ii. p. 108: "Being at Paris in the October following [the August of 1774], and knowing that there were several very eminent chemists in that place, I did not omit the opportunity, by means of my friend Mr. Magellan,[1] to get an ounce of *mercurius calcinatus* prepared by Mr. Cadet, of the genuineness of which there could not possibly be any suspicion; and, at the same time, I frequently mentioned my surprise at the kind of air which I had got from this preparation to Mr. Lavoisier, Mr. le Roy, and several other philosophers, who honoured me with their notice in that city, and who, I dare say, cannot fail to recollect the circumstance."

If any further evidence is required to prove that Lavoisier was not only not "the true and first discoverer" of oxygen, but that he has absolutely no claim to be regarded even as a later and independent discoverer, it is supplied by M. Berthelot himself. Not the least valuable portion of M. Berthelot's book, as an historical work, is that which he devotes to the analysis of the thirteen laboratory journals of Lavoisier, which have been deposited, by the pious care of M. de Chazelles, his heir, in the archives of the Institute. M. Berthelot has given us a synopsis of the contents of almost every page of these journals, with explanatory remarks and dates when these could be ascertained. As he well says, these journals "are of great interest because they inform us of Lavoisier's methods of work and of the direction of his mind—I mean the successive steps in the evolution of his private thought." On the fly-leaf of the third journal is written "*du* 23 *Mars*, 1774, *au* 13 *février*, 1776." From p. 30 we glean that Lavoisier visited his friend M. Trudaine at Montigny about ten days after his conversation with Priestley, and repeated the latter's experiments on the marine acid and alkaline airs (hydrochloric acid gas and ammonia). He is again at Montigny some time between February 28 and March 31, 1775, and repeats not only Priestley's experiments on the decomposition of mercuric oxide, presumably by means of M. Trudaine's famous burning-glass, but also his observations on the character of the gas. The fly-leaf of the fourth journal informs us that it extends from February 13, 1776, to March 3, 1778. On p. 1 is an account of experiments made February 13 on "précipité *per se* de chez M. Baumé," in which the disengaged gas is spoken of as "*l'air déphlogistique de M. Prisley*" (*sic*). Such a phrase in a private notebook is absolutely inconsistent with the idea that at this time Lavoisier considered himself as an independent discoverer of the gas. How he came to regard himself as such we need not inquire. Nor is it necessary to occupy your time by any examination of the arguments by which

[1] Prof. Grimaux ("Lavoisier," p. 51), says: "Un de ses [Lavoisier's] amis qui habitait Londres, Magalhaens ou Magellan, de la famille du célèbre navigateur, lui envoyait tous les mémoires sur les sciences qui paraissaient en Angleterre, et le tenait au courant des découvertes de Priestley."

M. Berthelot, with the skill of a practised advocate, would seem to identify himself with the case of his client. We would do him the justice of recognizing the difficulty of his position. He seeks to discharge an obligation of which the acknowledgment has been too long delayed. The Académie des Sciences a year ago awoke to the sense of its debt of gratitude to the memory of the man who had laboured so zealously for its honour, and even for its existence, during the stormy period of which France has just celebrated the centenary, and out of the *éloge* on Lavoisier which M. Berthelot, as Perpetual Secretary, was commissioned to deliver, has grown "La Révolution Chimique." To write eulogy, however, is not necessarily to write history. We cannot but think that M. Berthelot has been hampered by his position, and that his opinion, or at least the free expression of it, has been fettered by the conditions under which he has written. We imagine we discern between the lines the consciousness that, to use Brougham's phrase, the brightness of the illustrious career which he eulogizes is dimmed with spots which a regard for historical truth will not permit him wholly to ignore.

Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention is that not only did Lavoisier effect the overthrow, but he also discovered the facts. In other words, he has not only a claim to a participation in the discovery of oxygen, but he is also "the true and first discoverer" of the non-elementary nature of water. This second claim is directly and explicitly stated. Although it is supported by a certain ingenuity of argument, we venture to think that we shall be able to show it has no greater foundation in reality than the first.

Members of the British Association, who are at all familiar with its history, will recall the fact that this is not the first occasion on which the attempt to transfer "those laurels which both time and truth have fixed upon the brow of Cavendish" has had to be resisted. At the Birmingham meeting of 1839 the Rev. W. Vernon Harcourt, who then presided, devoted a large portion of his address to an able and eloquent vindication of Cavendish's rights. The attack came then as now from the Perpetual Secretary of the French Academy, and the charges were also formulated then, as now, in an *éloge* read before that learned body. The assailant was M. Arago, who did battle, not for his countryman, Lavoisier, whose claims are dismissed as "pretensions," but on behalf of James Watt, the great engineer, who was one of the foreign members of the Institute.

It is not my wish to trouble you at any length with the details of what has come to be known in the history of scientific discovery as the Water Controversy—a controversy which has exercised the minds and pens of Harcourt, Whewell, Peacock, and Brougham in England; of Brewster, Jeffrey, Muirhead, and Wilson in Scotland; of Kopp in Germany; and of Arago and Dumas in France. This controversy, it has been said, takes its place in the history of science side by side with the discussion between Newton and Leibnitz concerning the invention of the Differential Calculus, and that between the friends of Adams and Leverrier in reference to the discovery of the planet Neptune. Up to now it has practically turned upon the relative merits of Cavendish and Watt. M. Berthelot is the first French *savant* of any note who has seriously put forward the claims of Lavoisier, his countryman and predecessor Dumas having deliberately rejected them.

At the risk of wearying you with detail, I am under the necessity of restating the facts in order to make the position clear. Some time before April 18, 1781, Priestley made what he called "a random experiment" for the entertainment of a few philosophical friends. It consisted in exploding a mixture of inflammable air (presumably hydrogen) and common air, contained in a closed glass vessel, by the electric spark, in the manner first practised by Volta in 1776. The experiment was witnessed by Mr. John Warltire, a lecturer on natural philosophy and a friend of Priestley, who had rendered him the signal service of giving him the sample of the mercuric oxide from which he had first obtained oxygen. Warltire drew Priestley's attention to the fact that after the explosion the sides of the glass vessel were bedewed with moisture. Neither of the experimenters attached any importance to the circumstance at the time, Priestley being of opinion that the moisture was pre-existent in the gases, as no special pains were taken to dry them. Warltire, however, conceived the notion that the experiment would afford the means of determining whether heat was ponderable or not, and hence he was led to repeat it, firing the mixture in a copper vessel for

greater safety. The results of these observations are contained in Priestley's "Experiments and Observations on Air," vol. v. 1781, App., p. 395.

At this period Cavendish was engaged on a series of experiments "made, as he says, principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed" (Cavendish, Phil. Trans. 1784, p. 119). On the publication of Priestley's work he repeated Waltire's experiment, for, he says, as it "seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." The series of experiments which Cavendish was thus induced to make, and which he made with all his wonted skill in quantitative work, led him some time in the summer of 1781 to the discovery that a mixture of two volumes of the inflammable air from metals (the gas we now call hydrogen) with one volume of the dephlogisticated air of Priestley combine together under the influence of the electric spark, or by burning, to form the same weight of water. If Cavendish had published the results of these observations at or near the time he obtained them, there would have been no Water Controversy. But in the course of the trials he found that the condensed water was sometimes acid, and the search for the cause of the acidity (which incidentally led to the discovery of the composition of nitric acid) occasioned the delay. The main result that a mixture of two volumes of inflammable air and one volume of dephlogisticated air could be converted into the same weight of water was, however, communicated to Priestley, as he relates in a paper in the Phil. Trans. for 1783. Priestley was at this time interested in an investigation on the seeming convertibility of water into air, and he was led to repeat Cavendish's experiments, some time in March 1783, on what was apparently the converse problem. Priestley, however, made a fatal blunder in the repetition. With the praiseworthy idea of obviating the possibility of any moisture in the gases, he prepared the dephlogisticated air from nitre, and the inflammable air by heating what he calls "perfectly made charcoal" in an earthenware retort. At this time, it must be remembered, there was no sharp distinction between the various kinds of inflammable air: hydrogen, sulphuretted hydrogen, marsh gas and olefiant gas, coal gas, the vapours of ether and turpentine, and the gas from heated charcoal, consisting of a mixture of carbonic oxide, marsh gas, and carbonic acid, were indifferently termed "inflammable air." Priestley attempted to verify Cavendish's conclusion on the identity of the weight of the gases used with that of the water formed; but his method in this respect, as in his choice of the inflammable air, was wholly defective, and could not possibly have given him accurate results. It consisted in wiping out the water from the explosion vessel by means of a weighed piece of blotting-paper and determining the increase of weight of the paper. He says, however:—"I always found as near as I could judge the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for this purpose; the result was such as to afford a strong presumption that the air was reconverted into water, and therefore that the origin of it had been water." These results, together with those on the conversion of water into air, were communicated towards the end of March 1783 by Priestley to Watt, who began to theorize upon them, and then to put his thoughts together in the form of a letter to Priestley, dated April 26, 1783, and which he requested might be read to the Royal Society on the occasion of the presentation of Priestley's memoir. In this letter Watt says:—"Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red-hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. This water is then the only remaining product of the process, and *water*, *light*, and *heat* are all the products. *Are we not then authorized to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light, &c.?*"

This letter, although shown to several Fellows of the Society, was not publicly read at the time intended. Priestley, before its receipt, had detected the fallacy of his experiments on the seeming conversion of water into air, and as much of the letter was concerned with this matter Watt requested that it should be withdrawn. Watt, however, as he tells Black[1] in a letter dated June 23, 1783, had not given up his theory as to the nature of water, and on November 26, 1783, he restated his views more fully in a letter to De Luc. In the meantime, Cavendish, having completed one section of his investigation, sent in a memoir to the Royal Society, which was read on January 15, 1784, in which he gives an account of his experiments, and announces his conclusion "that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston." Watt thereupon requested that his letter to De Luc should be published, and it was accordingly read to the Royal Society on April 29, 1784. Which of the two—Cavendish or Watt—is, under these circumstances, to be considered as "the true and first discoverer" of the compound nature of water is the question which has been hitherto the main subject of the Water Controversy.

Let us now consider the matter as it affects Lavoisier. In 1783, Lavoisier had publicly declared against the doctrine of phlogiston, or rather, as M. Dumas puts it, "against the crowd of entities of that name which had no quality in common except that of being intangible by every known method" ("Leçons sur la Philosophie Chimique," p. 161). How completely Lavoisier had dissociated himself from the theory may be gleaned from his memoir of that year. "Chemists," he says, "have made a vague principle of phlogiston which is not strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire and sometimes it is fire combined with the earthly element; sometimes it passes through the pores of vessels and sometimes they are impenetrable to it: it explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form every moment."

But Lavoisier had merely renounced one fetich for another. At the time that he penned these lines he was as much under the thraldom of *le principe oxygine* as the most devoted follower of Stahl was in the bondage of phlogiston. The idea that the calcination of metals was but a slow combustion had been fully recognized. M. Berthelot tells us that, as far back as the March of 1774, Lavoisier had written in his laboratory journal:—"I am persuaded that the inflammation of inflammable air is nothing but a fixation of a portion of the atmospheric air, a decomposition of air. . . . In that case in every inflammation of air there ought to be an increase of weight, and he tried to ascertain this by burning hydrogen at the mouth of a vessel from which it was being disengaged. In the following year he asks, What remains when inflammable air is burnt completely? According to the theory by which he is now swayed it should be an acid, and he made many attempts to capture this acid. In 1777 he and Bucquet burnt six pints of the inflammable air from metals in a bottle containing lime-water, in the expectation that fixed air would be the result. And in 1781 he repeated the experiment with Gengembre, with the modification that the oxygen was caused to burn in an atmosphere of hydrogen, but not a trace of any acid product could be detected. Of course there must have been considerable quantities of water formed in these experiments, but Lavoisier was preoccupied with the conviction that oxidation meant acidification, and its presence was unnoticed, or, if noticed, was unheeded. Macquer, in 1776, had drawn attention to the formation of water during the combustion of hydrogen in air, but Lavoisier has stated that he was ignorant of that observation. What was it then that put him on the right track? We venture to think that M. Berthelot has himself supplied the answer. He says (p. 114):—"Rumours of Cavendish's trials had spread throughout the scientific world during the spring of 1783. . . . Lavoisier, always on the alert as to the nature of the products of the combustion of hydrogen, was now in such position that the slightest hint would enable him to comprehend its true nature. He hastened to repeat his trials, as he had the right to do, never having ceased to occupy himself with a question which lay at the very heart of his doctrine."

"On the 24th of June, 1783," continues M. Berthelot, "he repeated the combustion of hydrogen in oxygen, and he obtained a notable quantity of water without any other product, and he

[1] Watt, "Correspondence," p. 31.

concluded from the conditions under which he had worked that the weight of the water formed could not be other than equal to that of the two gases which had formed it. The experiment was made in the presence of several men of science, among whom was Blagden, a member of the Royal Society of London, who on *this occasion* recalled the observations of Cavendish (*qui rappela à cette occasion les observations de Cavendish*)."

On the following day Lavoisier published his results. The following is the official minute of the communication taken from the register of the sittings of the Académie des Sciences:—

Meeting of Wednesday, June 25, 1783.

MM. Lavoisier and De Laplace announced that they had lately repeated the combustion of Combustible Air with Dephlogisticated Air; they worked with about 60 *pints of the airs, and the combustion was made in a closed vessel: the result was very pure water.*

The cautious scribe who penned that minute did not commit himself too far. M. Berthelot, however, regards it as the first certain date of publication, established by authentic documents, in the history of the discovery of the composition of water; "a discovery," he adds, "which, on account of its importance, has excited the keenest discussion."

You will search in vain through the laboratory journals, as given by M. Berthelot, for any indications either of experiments or reflections which would enable you to trace the course of thought by which Lavoisier was guided to the truth. There is absolutely nothing on the subject until in the eighth volume (25 *mars*, 1783, *au février* 1784), and on p. 63 we come to the experiment of June 24, and we read:—"In presence of Messieurs Blagden, of [name illegible], de Laplace, Vandermonde, de Fourcroy, Meusnier, and Legendre, we have combined in a bell-jar dephlogisticated air and inflammable air drawn from iron by means of sulphuric acid, &c.... The amount of water may be estimated at 3 drachms: the amount which should have been obtained was 1 ounce 1 drachm and 12 grains. Thus we must suppose that there was a loss of two-thirds of the amount of the air or that there has been a loss of weight."

And this is the experiment which, according to M. Berthelot, enabled Lavoisier to conclude that "the weight of the water formed could not be other than equal to that of the two gases which had formed it"! It is on this single experiment, hurriedly and imperfectly done, that Lavoisier's claim to the discovery of the compound nature of water is based! M. Berthelot objects to the assumption that it was hurriedly done. He says, on p. 114: "Lavoisier caused a new apparatus to be made, with a couple of tubes and two reservoirs for the gases, an arrangement which would require a certain amount of time to put together; this circumstance proves that it could not have been an improvised trial." To what extent it was improvised will be seen immediately.

Now although the laboratory journals do not in this case "inform us of Lavoisier's methods, and of the direction of his mind, . . . the successive steps in the evolution of his private thought," we have other means of ascertaining how he arrived at his knowledge. The method was simplicity itself: he *was told of the fact, and his informant was none other than Cavendish's assistant, Blagden.*

Cavendish's memoir was published in 1784. Before it was struck off its author caused the following addition to be made: "During the last summer also a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water." This addition, as I have had the opportunity of verifying by an inspection of the original MSS. in the archives of the Royal Society, was made in the handwriting of Cavendish's assistant and amanuensis, Blagden.

When Lavoisier's memoir appeared, it was found to contain the following reference to this circumstance:—"It was on the 24th June that M. de Laplace and I made this experiment in presence of MM. le Roi, Vandermonde, and several other Academicians, and of Mr. Blagden, the present Secretary of the Royal Society of London. The latter informed us (*ce dernier nous apprit*) that Mr. Cavendish had already tried, in London, to burn inflammable air in closed vessels, and that he had obtained a very sensible quantity of water."

This reference was so partial, and its meaning so ambiguous, that Blagden addressed the following letter to Crell to be published in his *Chemische Annalen* (Crell's *Annalen*, 1786, vol. i. p. 58).

It is so direct and conclusive that I offer no apology for giving it almost entire:—[1]

"I can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made was communicated to Mr. Lavoisier. The following is a short statement of the history:—

"In the spring of 1783, Mr. Cavendish communicated to me, and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us that out of them he must draw the conclusion that dephlogisticated air was nothing else than water deprived of its phlogiston; and, *vice versâ*, that water was dephlogisticated air united with phlogiston. About the same time the news was brought to London that Mr. Watt, of Birmingham, had been induced by some observations to form a similar opinion. Soon after this I went to Paris, and in the company of Mr. Lavoisier and of some other members of the Royal Academy of Sciences I gave some account of these new experiments and of the opinions founded upon them. They replied that they had already heard something of these experiments, and particularly that Dr. Priestley had repeated them. They did not doubt that in such manner a considerable quantity of water might be obtained, but they felt convinced that it did not come near to the weight of the two species of air employed, on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject; but, as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations, to repeat the experiment, on a somewhat larger scale, as early as possible. This desire he complied with on June 24, 1783 (as he relates in the latest volume of the Paris memoirs). From Mr. Lavoisier's own account of his experiment, it sufficiently appears that at that period he had not yet formed the opinion that water was composed of dephlogisticated and inflammable airs, for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced anything contrary to truth; but it can still less be denied that he concealed a part of the truth; for he should have acknowledged that I had, some days before, apprised him of Mr. Cavendish's experiments, instead of which the expression 'il nous apprit' gives rise to the idea that I had not informed him earlier than that very day. In like manner Mr. Lavoisier has passed over a very remarkable circumstance—namely, that the experiment was made in consequence of what I had informed him of. He should likewise have stated in his publication not only that Mr. Cavendish had obtained 'une quantité d'eau très sensible,' but that the water was equal to the weight of the two airs added together. Moreover, he should have added that I had made him acquainted with Messrs. Cavendish and Watt's conclusions—namely, that water, and not an acid, or any other substance, arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions opened the way to Mr. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish, only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. . . . The course of all this history will clearly convince you that Mr. Lavoisier (instead of being led to the discovery by following up the experiments which he and Mr. Bucquet had commenced in 1777) was induced to institute again such experiments, solely by the account he received from me, and of our English experiments; and that he really discovered nothing but what had before been pointed out to him to have been previously made out and demonstrated in England."

[1] Mr. Muirhead's translation. *Vide* Watt, "Correspondence," "Composition of Water," p. 71.

To this letter, reflecting so gravely on his honour and integrity, Lavoisier made no reply. Nor did Laplace, Le Roi, Vandermonde, or any one of the Academicians concerned, vouchsafe any explanation. *De non apparentibus et de non existentibus eadem est ratio.* No explanation appeared, because none was possible. M. Berthelot ignores this letter, which is the more remarkable, since reference is made to it in more than one of the publications which he tells us he has consulted in the preparation of his account of the Water Controversy. If he knew of it he must regard it either as unworthy of an answer or as unanswerable.

It would be heaping Ossa on Pelion to adduce further evidence from letters of the time of what Lavoisier's contemporaries thought of his claims. *De mortuis nil nisi bonum.* I would much more willingly have dwelt upon the virtues of Lavoisier, and have let his faults lie gently on him; but I have felt it incumbent on me on this occasion to make some public answer to M. Berthelot's book, and in no place could that answer be more fittingly given than in this town, which saw the dawn of that work out of which these grand discoveries arose. It may be that much of what I have had to say is as a twice-told tale to many of you. I trust I need make no apology on that account. The honour of our ancestors is in our keeping, and we should be unworthy of our heritage and false to our trust if we were slow to resent or slack to repel any attempt to rob them of that glory which is their just right and our proud boast.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY A. H. GREEN, M.A., F.R.S., PROFESSOR OF GEOLOGY IN THE UNIVERSITY OF OXFORD, PRESIDENT OF THE SECTION.

THE truth must be told; and this obliges me to confess that my contributions to our stock of geological knowledge, never very numerous, have of late years been conspicuously few, and so I have nothing to bring before the Geelogical Section that can lay any claim to be the result of original research.

In fact, nearly all my time during the last fifteen years has been taken up in teaching. This had led me to think a good deal about the value of geology as an educational instrument, and how its study compares with that of other branches of learning in its capability of giving sinew and fibre to the mind, and I have to ask you to listen to an exposition of the notions that have for a long time been taking shape bit by bit in my mind on this subject.

I am not going to enter into the question, handled repeatedly and by this time pretty well threshed out, of the relative value of natural science, literature, and mathematics, as a means of educational discipline; for no one who is lucky enough to know a little of all three will deny that each has an importance of its own, and its own special place in a full and perfect curriculum. The question which is the most valuable of the three I decline to entertain, on the broad general ground that "comparisons are odorous," and for the special reason that the answer must depend on the constitution of the mind that is to be disciplined. I might quite as reasonably attempt to lay down that a certain diet which suits my constitution and mode of life must agree equally well with all that hear me.

I need scarcely say that nothing would induce me, if it could possibly be helped, to say one word that might tend to disparage the pursuit to which we are all so deeply attached. But I cannot shut my eyes to the fact that, when geology is to be used as a means of education, there are certain attendant risks that need to be carefully and watchfully guarded against.

Geologists, and I do not pretend myself to be any better than the rest of them, are in danger continually of becoming loose reasoners. I have often had occasion to feel this, and I recall a scene which brought it home to me most forcibly. At a gathering, where several of our best English geologists were present, the question of the cause of changes of climate was under discussion. The explanation which found most favour was a change of the position of the axis of rotation within the earth itself; and this, it was suggested, might have been brought about by the upheaval of great bodies of continental and mountainous land where none now exist, and an accompanying depression of the existing continents or parts of them. That such a redistribution of the heavier material of the earth would result in some shifting of the axis of rotation admits of no doubt. The important question is, How much? What degree of rearrangement of land and sea would be needed to produce a shift of the amount required? It is purely a question of figures, and the necessary calculations can be made only by a mathematician. I ventured to suggest that some one who could work out the sum should be consulted before a final decision was arrived at, for I knew perfectly well that not one of the company present could do it. But if I say that my advice met with scant approval, I should represent very inadequately the lack of support I met with. The bulk of those present seemed quite content with the vague feeling that the thing could be done in the way suggested, and there was a general air of indifference as to whether the hypothesis would stand the test of numerical verification or not.

I could bring many other similar instances which seem to me to justify the charge I have ventured to make; but it will be more useful to inquire what it is that has led to a failing, which, if it really exist, must be a source of regret to the whole brotherhood of hammerers.

The reason, I think, is not far to seek. The imperfection of the Geological Record is a phrase as true as it is hackneyed. No more striking instance of its correctness can be found than that furnished by the well-known mammalian jaws from the Stonesfield slate. The first of these was unearthed about 1764; others, to the number of some nine, between then and 1818. The rock in which these precious relics of the beginning of mammalian life occur has been quarried without intermission ever since; it has been ransacked by geologists and collectors without number; many of the quarrymen know a jaw when they see it, and are keenly alive to the market value of a specimen; but not one of these prized and eagerly-sought-after fossils has turned up during the last seventy years.

Then, again, how many of the geological facts which we gather from observation admit of diverse explanation. Take the case of *Eozoon Canadense*. Here we have structures which some of the highest authorities on the Foraminifera assure us are the remains of an organism belonging to that order; other naturalists, equally entitled to a hearing, will have it that these structures are purely mineral aggregates simulating organic forms. And hereby hangs the question whether the limestones in which the problematical fossil occurs are organic, or formed in some other and perhaps scarcely explicable way.

And this after all is only one of the countless uncertainties that crowd the whole subject of invertebrate palæontology. In what a feeble light have we constantly to grope our way when we attempt the naming of fossil Conchifers for instance. The two species *Gryphæa dilatata* and *G. bilobata* furnish an illustration. Marked forms are clearly separable, but it is easy to obtain a suite of specimens, even ftom the Callovian of which the second species is said to be specially characteristic, showing a gradual passage from one form into the other. And over and over again the distinctions relied upon for the discrimination of species must be pronounced far-fetched and shadowy, and are, it is to be feared, often based upon points which are of slender value for classificatory purposes. In the case of fossil plants the last statement is notoriously true, and yet we are continually supplied with long lists of species which every botanist knows to be words and nothing more, and zonal divisions are based upon these bogus species and conclusions drawn from them.

It is from data such as have been instanced, scrappy to the last degree, or from facts capable of being interpreted in more than one way, or from determinations shrouded in mist and obscurity, that we geologists have in a large number of cases to draw our conclusions. Inferences based on such incomplete and shaky foundations must necessarily be very largely hypothetical. That this is the character of a great portion of the conclusions of geology we are all ready enough to allow with our tongue—nay, even to lay stress upon the fact with penned or spoken emphasis. But it is open to question whether this homage at the shrine of logic is in many cases anything better than lip-service; whether we take sufficiently to heart the meaning of our protestations, and are always as alive as our words would imply to the real nature of our inferences.

A novice in trade, scrupulously honest, even morbidly conscientious to begin with, if he lives among those who habitually use false scales, runs imminent risk of having his sense of integrity unconsciously blunted and his moral standard insensibly lowered. A similar danger besets the man whose life is occupied in deducing tentative results from imperfectly ascertained facts. The living, day by day, face to face with approximation and conjecture must tend to breed an indifference to

accuracy and certainty, and to abate that caution and that wholesome suspicion which make the wary reasoner look well to his foundations, and resolutely refuse to sanction any superstructures, however pleasing to the eye, unless they are firmly and securely based.

If I am right in thinking that the mental health of the geologist of matured experience and full-grown powers is liable to a disorder of the kind I have indicated, how much greater must the risk be in the case of a youth, in whom the reasoning faculty is only beginning to be developed, when he approaches the study of geology! And does it not seem at first sight that that study could scarcely be used with safety as a tool to shape his mind, and so train his bent that he shall never even have a wish to turn aside either to the right hand or to the left from the strait path that leads through the domain of sound logic?

That it is hazardous, and that evil may result from an incautious use of geology as an educational tool, I entertain no doubt. The same may, indeed, be said of many other subjects, but I feel that it is specially true in the case of geology. But I should be guilty of that very haste in drawing conclusions against which I am raising a warning word, if I therefore inferred that geology can find no place in the educational curriculum.

To be forewarned is a proverbial safeguard, and those who are alive to a danger will cast about for a means of guarding against it. And there are many ways of neutralizing whatever there may be potentially hurtful in the use of geology for educational ends. It has been said that the right way to make a geologist is not to teach him any geology at all to begin with. To send him first into a laboratory, give him a good long spell at observations and measurements requiring the minutest accuracy, and so saturate his mind with the conception of exactness that nothing shall ever afterwards drive it out. If a plan like this be adopted, it is easy to pick out such kinds of practical work as will not only breed the mental habits aimed at, but will also stand him in good stead when he goes on to his special subject. Goniometrical measurements and quantitative analysis will serve the double purpose of inspiring him with accurate habit of thought, and helping him to deal with some of the minor problems of geology. And I cannot hold that this practice of paying close attention to minute details will necessarily unfit a man for taking wider sweeps and more comprehensive views later on. That habit comes naturally to every man who has the making of a geologist in him directly he gets into the field. Put such a man where a broad and varied landscape lies before him, teach him how each physical feature is the counterpart of geological structure, and breadth of view springs up a native growth. I do not mean to say that the plan just suggested is the only way of guarding against the risk I have been dwelling upon. There are many others. This will serve as a sample to show what I think ought to be aimed at in designing the geological go-cart. And any such mind-moulding leads, be assured, not to hesitancy and doubt, but to conclusions, reached slowly it may be, but so securely based that they will seldom need reconstruction.

There is another aspect of the question. The uncertainties with which the road of the geologist are so thickly strewn have an immense educational value, if only we are on our guard against taking them for anything better than they really are. Of those stirring questions which are facing us day by day and hour by hour, none perhaps is of greater moment than the discussion of the value of the evidence on which we base the beliefs that rule our daily life. A man who is ever dealing with geological evidence and geological conclusions, and has learned to estimate these at their real value, will carry with him, when he comes to handle the complex problems of morals, politics, and religion, the wariness with which his geological experience has imbued him.

Now I trust the prospect is brightening. Means have been indicated of guarding against the danger which may attend the use of geology as an educational instrument. Need I say much to an audience of geologists about the immense advantages which our science may claim in this respect? In its power of cultivating keenness of eye it is unrivalled, for it demands both microscopic accuracy and comprehensive vision. Its calls upon the chastened imagination are no less urgent, for imagination alone is competent to devise a scheme which shall link together the mass of isolated observations which field work supplies; and if, as often happens, the fertile brain devises several possible schemes, it is only where the imaginative faculty has been kept in check by logic that the one scheme that best fits each case will be selected for final adoption. But, above all, geology has its home, not in the laboratory or study, but *sub Jove*, beneath the open sky; and its pursuit is inseparably bound up with a love of Nature, and the healthy tone which that love brings alike to body and mind.

And what does the great prophet of Nature tell us about this love?

> "The boy beholds the light and whence it flows;
> The man perceives it die away,
> And fade into the light of common day."

Will it not, then, be kind to encourage the boy to follow a pursuit which will keep alive in him a joy which years are too apt to deaden; and will not the teaching of geology in schools conduce to this end? Geology certainly should be taught in schools, and for more prosaic reasons, of which the two following are, perhaps, the most important. Geography is essentially a school subject, and the basis of all geographical teaching is physical geography. This cannot be understood without constant reference to certain branches of geology. Again, how many are the points of contact between the history of nations, the distribution and migrations of peoples, and the geological structures of the lands they have dwelt in or marched over.

But geology is not an easy subject to teach in schools. The geology of the ordinary text-book does not commend itself to the boy-mind. The most neatly-drawn sections, nay, even the most graphic representations of gigantic and uncouth extinct animals, come home to the boy but little, because they are pictures and not things. He wants something that he can handle and pull about; he does not refuse to use his head, but he likes to have also something that will employ his hands at the same time.

The kind of geology that boys would take to is outdoor work; and, of course, where it can be had, nothing better could be given them. A difficulty is that field work takes time and filches away a good deal of the intervals that are devoted to games. Still cross-country rambles and scrambling about quarries and cliffs are not so very different from a paper-chase; and if the teacher will only infuse into the work enough of the fun and heartiness which come so naturally in the open air, he need not despair of luring even the most high-spirited boy, every now and then, away from cricket and football.

But there are localities not a few—the Fen country, for instance—where it is scarcely possible to find within manageable distance of the school the kind of field-geology which is within the grasp of a beginner. But even here the teaching need not be wholly from books. The best that can be done in such cases is to make object-lessons indoors its basis. For instance, give a lad a lump of coarsish sandstone; let him pound it and separate by elutriation the sand grains from the clay; boil both in acid, and dissolve off the rusty coating that colours them; ascertain by the microscope that the sand grains are chips and not rounded pellets, and so on. All such points he will delight to worry out for himself; and, when he has done that, an explanation of the way in which the rock was formed will really come home to him. Or it is easy to rig up contrivances innumerable for illustrating the work of denudation. A heap of mixed sand and powdered clay does for the rock denuded; a watering-can supplies rain; a trough, deeper at one end than the other, stands for the basin that receives sediment. By such rough apparatus many of the results of denudation and deposition may be closely imitated, and the process is near enough to the making of mud-pies to command the admiration of every boy. It is by means like these that even indoor teaching of geology may be made life-like.

I need not dwell upon the great facts of physical geology which have so important a bearing on geography and history; but I would, in passing, just note that these too often admit of experimental illustration, such for instance as the well-known methods of imitating the rock-folding caused by earth-movements. I would add that wherever, in speaking of school teaching, I have used the word "boy," that word must of course be taken to include "girl" as well.

In conclusion I should like to give you an outline of the kind of course I endeavour to adopt in more advanced teaching in the case of students who are working at other subjects as well, and can give only a part of their time to geology. During the first year the lectures and book-work should deal with physical geology. In the laboratory the student should first make the acquaintance of the commoner rock-forming minerals, the means of recognizing them by physical characters, blowpipe tests, and

the simpler methods of qualitative analysis, and may then go on to work at the commoner kinds of rocks and the elements of microscopic petrography. During the summer months I would take him into the field, but not do more than impress upon him some of the broader aspects of outdoor work, such as the connection between physical feature and geological structure.

During a second year stratigraphical geology should be lectured upon and studied from books, and so much of animal morphology as may be necessary for palæontological purposes should be mastered. The practical work would lie mainly among fossils, with a turn every now and again at mineralogy and petrology to keep these subjects going. Out of doors I would not yet let the student attempt geological mapping, but would put into his hands a geological map and descriptions of the geology of his neighbourhood, and he would be called upon to examine in minute detail all accessible sections, collect and determine fossils, and generally see how far he can verify by his own work the observations of those who have gone before him.

Indoor work during the third year would be devoted to strengthening and widening the knowledge already gained. Out of doors the student should attempt the mapping of a district by himself. It will be well, if there is any choice in the matter, to select one in which the physical features are strongly marked.

This sketchy outline must serve to indicate the notions that have grown up in my mind on the subject now before us, and the methods I have been led to adopt in the teaching of geology. I trust that they may be suggestive, and may call forth that kindly and genial criticism with which the brotherhood of the hammer are wont to welcome attempts, however feeble, to strengthen the corner-stones and widen the domain of the science we love so well, and to enlarge the number of its votaries.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

British Association Procedure.

I AM glad to see a letter from my colleague, Prof. Armstrong, on the subject of the procedure of the British Association. I am not disposed to take an exaggerated view of the harm that may arise from the mild excitement and dissipation which seem to be inseparable from gatherings of this kind; but I do not regard with satisfaction the prospect of annulling half the good effect of my much-needed rest and annual dose of fresh air, by spending a week in crowded rooms in the middle of a great town. The disinclination to run this risk increases, and the risk itself seems to increase, when the date fixed for the meeting is selected in such an unfortunate way as to cut in two the summer holiday of many members, and especially of those who are teachers, whether in school, college, or university.

Having been on two separate occasions concerned in making preparations for the reception of the Association, I know something of the circumstances which have to be considered. One of the most important points is the provision of suitable rooms for reception and Sectional business. These are very commonly obtained in colleges or schools, and cannot be placed at the disposal of the Association till the holidays begin. But all schools or colleges break up before the end of July, and the last days of July or the first days of August would be the most convenient time to the great majority of those who form the most numerous and active attendants at the meetings. The weather at that time is also more likely to be favourable to garden parties and excursions than at any time late in August or in September. That, at least, is my own opinion as to the time of meeting.

Then as to the work of the Sections. As a Sectional Secretary I have read papers (other people's) at 3 o'clock in the afternoon to an audience consisting of a Vice-President impatient to follow the President to lunch, two reporters who were not listening, and my wife making signals of distress from a back bench. As a Sectional President I have sat at the same hour, luncheonless and weary, while a paper which seemed as long and as discursive as the story of the Ancient Mariner, was droned forth by the author to an audience of about three persons fidgeting like the belated wedding guest. I wonder whether this sort of thing is supposed to be of any use to anybody.

The change which I proposed, and which was in part embodied in a recommendation from Section B to the Council, consisted in altering the hour for Committees from 10 to 9.30, and beginning general business at 10 instead of 11; the Section to close at 2.

The only objection I heard to 9.30 was that some members might be lodged at a distance, and find it difficult to attend so early. I never could see much except laziness at the bottom of this objection. The only other that occurred to me was that possibly sometimes the Committee business would occupy more than half an hour. But this difficulty, even if real, chiefly arises from the practice to which Prof. Armstrong has referred, of making the Committees so large.

This practice serves no obvious purpose except that of advertising a certain number of people who like to see their names in print. I believe the demand for election upon Sectional Committees would be considerably reduced if the names of the Sectional Committees *were no longer printed*. It would be quite sufficient for the purposes of business to give in the Journal only the names of the officers.

I think, further, that something should be done to reduce the cost of a meeting to the town visited by the Association. The gorgeousness of the entertainments given, and the demands made upon provincial pockets, have become so extravagant that none but wealthy or ambitious towns can face the luxury of a visit of the British Association. WILLIAM A. TILDEN.

Birmingham, September 1.

WHILST I entirely agree with Prof. Armstrong as to the desirability of reform in regard to most of the matters to which he has called attention, I would like to point out that in one respect the large Sectional Committees have perhaps served a useful purpose. Nowhere are the older and younger men of science brought so extensively into direct contact with each other as at the meetings of these Committees, and hence they have served perhaps more than anything else to introduce the younger provincial men to their older and younger brethren of the metropolis and to each other.

If it be admitted therefore that a chief object of the Association is that its members shall meet, I think, speaking as a provincial, that there is much to be said in favour of the retention of moderately large Sectional Committees; though no doubt the introduction of such reforms as would tend to discourage the presence on them of those who are out of place would add to their usefulness in every way. W. A. SHENSTONE.

Clifton, September 2.

Fine Group of Sun-spots.

THIS morning I saw a very large cluster of spots in the sun's northern hemisphere, and nearly at mid-transit across the disc. The group is elongated east and west, and there is a fine spot at each of the extremities. The length of the group is about 113,000 miles; it exhibits a very complicated structure, and I have made a drawing of it with some difficulty, owing to the rapid changes it is undergoing in detail. A 3-inch refractor, power 90, defines the object well, and reveals many peculiarities in its form. Though I have termed it a group of spots, it might with propriety be called a single spot, for it is connected with wisps of penumbra, and chains of small spots, which altogether represent an extensive area of disturbance.

On looking at the sun with the eye simply protected with tinted glass, I see the group of spots distinctly, and it would form quite a conspicuous appearance to the naked eye should the sun rise or set in a fog during the next day or two.

I ascertained by frequent scrutiny during the first half of the present year that the sun's spots were usually very small and fugitive, and the present fine display of *maculæ* is therefore all the more worthy of observation and record.

Bristol, August 31. W. F. DENNING.

Organic Colour.

IN considering the causes of bright colouring in animals and plants, I think the physical meaning of colour has not been sufficiently regarded.

All dull colours, such as browns, olives, plums, &c., mean that vibrations of every wave-length in the white sunlight are absorbed almost entirely, a very small proportion being reflected. A deep red colour means that there is a less proportion of the longest waves absorbed; a deep violet, that there is a less proportion of the shortest waves absorbed; and a full green, that the absorption is less in the intermediate wave-lengths. These are the primary hues; but in objects which reflect the brilliant secondary hues—scarlets, yellows, blues, and pinks—the chief absorption is confined to a small area in the spectrum, a large proportion of the light being reflected.

There are, then, three distinct stages of coloration, viz. (1) that in which all wave-lengths are absorbed; (2) that in which absorption ceases in respect to about one-third of the spectrum; (3) that in which absorption ceases in respect to about two-thirds of the spectrum.

These three stages are progressive, and in the direction of progress from chaos to unity; from a condition of the protoplasm in which molecular elements of very diverse vibrating capacity are mixed up together, to a condition in which the capacities of these elements have become greatly simplified.

When we speak of an organism arriving at maturity, we imply that it began its career in a state of immaturity, and that it gradually progresses to the condition of maturity. In what that condition consists, or what fundamental changes have taken place, it may not be easy to say; but it is surely true, as a rule, that organisms in an early and immature state are comparatively dull in colour, and do not put on their brightest hues until the period of maturity, indicating that one of the characteristics of maturity is the simplification of the vibrating capacity of the molecules. If this be really a law of Nature, it is a far-reaching one, and will account for much. F. T. MOTT.

Leicester.

On the Soaring of Birds.

I HAVE thought that this habit can be explained as follows; at least as regards rooks, which I have often noticed soaring in flocks, especially in the spring, and I think usually in warm cloudy weather.

An upward convection current of warm air is established over some area. The birds stretch out their wings, and if the upward velocity of the current should happen to be just equal to the velocity with which a bird with outstretched wings would sink through still air (the "terminal velocity"), the bird would be supported; but if it were somewhat greater, the bird would be raised upwards. In that case he inclines his wings so that the resolved part of the pressure on the under side of the wings carries him forward at a uniform level. But this movement, being rectilinear, would take him outside the warm column which he is enjoying. A centripetal force is therefore needed to maintain the circular movement, and this is obtained by tipping the wings, so that the wing which points outwards is raised, and that which points inwards towards the warm column is depressed, as noticed by your correspondent. If the upward velocity of the current is not sufficient to support the bird, an occasional flap with both wings, and the subsequent sinking, supplies the deficiency of upward pressure.

O. FISHER.

IN your issue of August 21 (p. 397) Mr. Magnus Blix gives a very ingenious explanation of the soaring of birds. It appears, however, to me that this explanation rests upon a false basis.

In his illustration, Mr. Blix supposes a bird to be moving in a direction, relative to the wind, at right angles to that of the wind, its absolute velocity, therefore, being greater than that of the wind. He then supposes the bird, by movement of wing-plane, to change its direction to one opposite to that of the wind, and assumes that its absolute velocity, in the new direction, will be equal to the absolute velocity in the old.

Now it is probably true that a bird can change its direction without sensible loss of *velocity relative to the air*, but any velocity it may have, in virtue of the motion of the air, must remain as a component of the new velocity in the same direction as before, however the bird may change the direction of its wing-plane.

Now the supposed bird, in changing its direction at c, would still have the component of velocity due to the wind acting in direction ef as before. Its velocity relative to the wind, therefore, from c to d would be the original velocity at a (diminished in its passage from a to c); its absolute velocity the difference of the two velocities.

If this objection hold good, Mr. Blix's theory seems to be no longer an explanation. C. O. BARTRUM.

19 Well Walk, Hampstead, August 26.

Occurrence of a Crocodile on Cocos Islands.

DURING a recent visit to Cocos Islands Mr. Ross showed me the skull of a crocodile of small size which had appeared about a year previously on the islands. It was first seen by a native Cocosian, who reported that he had seen something between a lizard and a log of wood in the sea. It then reappeared upon another island and destroyed a number of ducks, and was eventually shot by Mr. Ross. The distance from Java, the nearest land, is fully 700 miles. It is remarkable that this animal should have swum so far, and managed eventually to strike this small patch of land in the middle of the ocean. I do not know another record of a big reptile travelling so far. Mr. Ross tells me that bamboo-rafts sometimes drift to Cocos, and perhaps it managed to help itself along on one of these.

The whole seas here, but especially the Straits of Sunda and Malacca Straits, are full of drift-fruits, seeds, sticks, stems of Nipa and Pandanus; and between the Straits of Sunda and Cocos, large patches of pumice rolled lumps and dust can be seen, the relics of the destruction of Krakatão.

H. N. RIDLEY.

Botanic Gardens, Singapore, August 6.

Helix nemoralis and hortensis.

I SHOULD be very pleased if some of the various conchological readers of NATURE would kindly furnish me with their records of these two shells. The questions I specially want to ask concerning them are as follows:—What varieties (with band-formulæ) have they found? What number of each variety and band-variation have they taken? What is the environmental condition of the localities where they have found them, as regards plant-life and geological formation? And, in addition, I want the records (and this is a special point) from separate and distinct hedges or banks.

J. W. WILLIAMS.

57 Corinne Road, Tufnell Park, N.

Mr. Williams's "British Fossils."

IN my review of Mr. Williams's "British Fossils," published in NATURE of August 28 (p. 412), I notice a slip on my part in regard to eclogite. I should have said that whereas this rock is stated to consist of red garnets and hornblende, it is usually described as being composed of red garnets and one of the pyroxenes, such as omphacite or smaragdite, or both.

Since writing the review I have come to the conclusion that the twice repeated term "dermoid types" is intended for "demoid types"; a term used in the second edition of Phillips's "Manual of Geology." THE REVIEWER.

August 29.

A Remarkable Rainbow.

I HAVE just seen a very remarkable rainbow. It was plus 60° in height, and thin. The sunset was lurid, with a mock sun to the south of the real one. D. MACGILLIVRAY.

Oxford, August 25.

NOTES.

ON Sunday, August 17, M. Janssen ascended to the Grands Mulets, and next day he reached a hut called the Cabane des Bosses, which an Alpinist, M. Vallot, of Paris, has erected at a point about 400 metres below the summit of Mont Blanc. According to the Paris correspondent of the *Times*, the second day's journey was made in a sledge, drawn and pushed by twenty-two guides. Tuesday, Wednesday, and Thursday M. Janssen spent in a part of the hut which M. Vallot has fitted up as a scientific laboratory. On Friday, as the weather was very clear, M. Janssen had his sledge dragged up to the summit of the mountain to complete his observations. At the ridge of the

Bosses, which is almost vertical, and bordered on both sides by beds of snow ready to fall in avalanches at the slightest motion, the guides begged him to leave the sledge. He did so, but after taking five or six steps he fell exhausted on the snow and had to return to the sledge. He went back to the Grands Mulets the same day, and on the following Sunday he reached the Hôtel de Mont Blanc, and rejoined Mme. and Mdlle. Janssen, who had watched all his movements through a telescope. The results obtained by M. Janssen on this occasion confirm those to which he was led by his previous observations at the Grands Mulets.

THE medical profession loses much by the death of Dr. James Matthews Duncan, F.R.S. He died of heart disease at Baden-Baden, on Monday, September 1. He was born at Aberdeen in 1826.

WE regret to have to record the death of Prof. Carnelly. He died suddenly on August 27, at the age of 38. He had held the chair of chemistry at Firth College, and at the Dundee University College; and two years ago he was appointed Professor of Chemistry at Aberdeen.

ANOTHER death which we are sorry to have to record is that of Miss North, who died on Saturday, August 30, at her residence, Mount House, Alderley, Wotton-under-Edge, Gloucestershire, after a prolonged illness.

ORAZIO SILVESTRI, the distinguished chemist and vulcanologist, died at Catania on August 17. He was fifty-five years of age. In 1863 he was appointed to the professorship of chemistry at the University of Catania, whence he was transferred, in 1874, to a corresponding chair at the University of Turin. Afterwards he returned to Catania, where he became professor of mineralogy, geology, and vulcanology. Prof. Silvestri was an enthusiatic student of Mount Etna, and carried on many important investigations during the eruptions of 1865, 1869, 1879, 1883, and 1886. Through his efforts an astronomical and meteorological observatory has been constructed on Etna at a height of 3000 metres.

THE British Pharmaceutical Association held its twenty-seventh annual meeting in Leeds on Tuesday and Wednesday. The chair was occupied by Mr. Charles Umney. The attendance was unusually numerous.

THE Sanitary Institute had a most successful Congress at Brighton last week. Among the presidential addresses was one on "Geology in its relation to hygiene, as illustrated by the geology of Sussex," by Mr. W. Topley, F.R.S., President of the Section for Chemistry, Meteorology, and Geology. The discussions at the various meetings did much to foster the interest of the public in the laws of public health; and we should have been glad to devote more attention to the proceedings but for the pressure on our space due to the meeting of the British Association.

THIS week the International Congress of Agriculture and Forestry is holding a series of meetings at Vienna. There are delegates from Great Britain and many other countries. The proceedings began on Monday evening with a reception given by the organizing committee. On Tuesday the opening address was delivered by Count Christian Kinsky, President of the Diet of Lower Austria. The final sitting will be held on Saturday.

THE fourth annual series of vacation science courses at Edinburgh was brought to a close last Saturday with an excursion to Melrose and Abbotsford. These courses corresponded to the second part of the Oxford summer gathering, and were remarkably successful. A similar series is being organized for the winter months, and will be specially adapted to "the educational requirements of teachers."

ON August 27 and 28 earthquake shocks were felt along the Danube valley from Amstetten to Grein in Lower Austria. The seismic movement on August 28 lasted ten minutes, and was accompanied by a disturbance of the river, the water rising into long lines of waves similar to those caused by the paddle-wheel of a steamer.

THE Caucasus papers relate an interesting case of globular lightning which was witnessed by a party of geodesists on the summit of the Böhul Mountain, 12,000 feet above the sea. About 3 p.m., dense clouds of a dark violet colour began to rise from the gorges beneath. At 8 p.m., there was rain, which was soon followed by hail and lightning. An extremely bright violet ball, surrounded with rays which were, the party says, about two yards long, struck the top of the peak. A second and a third followed, and the whole summit of the peak was soon covered with an electric light which lasted no less than four hours. The party, with one exception, crawled down the slope of the peak to a better sheltered place, situated a few yards beneath. The one who remained was M. Tatosoff. He was considered dead, but proved to have been only injured by the first stroke of lightning, which had pierced his sheepskin coat and shirt, and burned the skin on his chest, sides, and back. At midnight the second camp was struck by globular lightning of the same character, and two persons slightly felt its effects.

A STUDY of five years' thunderstorms (1882–86) on the Hungarian plain has been recently made by M. Hegyfoky. We note the following points in his paper (communicated to the Hungarian Academy). The days of thunderstorm were those on which thunder was observed, and they formed 16·4 per cent. of all days from April to September. The air-pressure on those days sank about 2 mm. under the normal, morning and evening. The less the pressure, the greater the probability of thunderstorm. The temperature (estimated by the maximum thermometer) was higher than that of all days of the season indicated; and the moisture and cloudiness were similarly in excess. The wind blew about mid-day more softly, and in the evening more strongly than usual. It went round, as a rule, from the south-east by the south to the west and north-west. The clouds came oftener than usual from the south-east and south-west quadrants; so that the centre was generally north of the station. Nearly half of the season's rainfall was on days of thunderstorm. Hail fell on 11 days, on one of which there was no thunderstorm. There were most thunderstorms in June (59 out of 199). The June of 1886 had as many as 26. The commencement of a thunderstorm (first thunder) occurred most often from 2 to 5 p.m. Towards the end of the season the thunderstorms tend to come later in the day. When the pressure falls under the mean of the season (752·4 mm.), the thunderstorms last longer than when it is above the mean. The path was in most cases from south-west or west, and in most cases coincided with that of both lower and upper clouds, but in several cases only with that of the lower or upper. After the first thunder the meteorological elements are usually subject to great changes, most marked as the storm nears the zenith: rain falls, wind rises, and alters quickly in direction, temperature and vapour-pressure fall, relative humidity, cloud, and pressure increase. As the storm withdraws there is a return to the normal. Various other points are considered. The author accepts Sohncke's theory—that the electricity of thunderstorms is due to friction of water-drops on ice.

THE Meteorological Council have just published a series of observations made at Sanchez (Samaná Bay), St. Domingo, in the years 1886–88. They were made chiefly by the late Dr. W. Reid, Medical Officer of the Samaná and Santiago Railway Company. The Council, recognizing the value of the observations, which

were taken with much care, and for a locality for which they are very scarce, determined to publish them in detail. The monthly means and summaries have been calculated in the Meteorological Office and added in a convenient form at the end of the volume. From these it is seen that the maximum shade temperature was 96°·5 in September 1887, and the minimum 58°·5 in March 1888. The rainfall varied considerably in the different years—as much as 26 inches. The greatest daily fall was 6 inches in April. The sunshine recorded in 1888 amounted to an average of 6·7 hours daily. Dr. Reid remarks, with regard to the wind, that in about 19 days out of 20 there is a light breeze in the west at 6h. a.m., which continues till about 8h. a.m., then a short calm, then a light breeze from about south, which veers round to east or east-south-east by 10 a.m., and there continues till about 4 p.m., when it remains calm till next morning. Only three gales are recorded during the three years, and these all occurred in 1886.

THE *Pall Mall Gazette* has issued in its "Extra" series a charming story of a dog. It is called "Teufel the Terrier: the Life and Adventures of an Artist's Dog," and is "told by J. Yates Carrington, and edited by Charing Cross." The tale is admirably illustrated, and will give much pleasure to all who study the ways of dogs, and appreciate their intelligence and sense of fun.

PART 23 of Cassell's "New Popular Educator" has been published. Besides the woodcuts in the text, there is a coloured plate illustrating electric discharges in rarefied gases.

MESSRS. GEORGE L. ENGLISH AND CO., of Philadelphia and New York, have published the fifteenth edition of their catalogue of the minerals which they have for sale. There has been in America, they say, a "very great increase in the demand for mineral specimens."

THE proposed creation of Universities in France will soon give rise to much animated discussion in the French Senate. Meanwhile, the Ministry of Public Instruction has prepared a return showing the number of students who at present attend the different French faculties. The total is 16,587, of whom 15,316 are Frenchman and 1271 foreigners, as against only 9863 fifteen years ago. Of this total 5843 students attend the faculty of medicine, 4570 that of law, 1834 that of literature, 1590 that of pharmacy, 1276 that of science, and 101 that of Protestant theology. Rather more than half of these (8653) are students of the different Paris faculties, and of the 1271 foreign students 1078 are in Paris. There are 989 Europeans (313 Russians, 159 Roumanians, and 121 Turks), 201 Americans (of whom 173 come from the United States), 68 Africans (of whom 51 are Egyptians), 12 Asiatics, and 1 Australian. The great majority of these foreigners are studying medicine; 907 belong to that faculty, while 240 are studying law, 58 science, 39 pharmacy, 24 literature, and 3 Protestant theology.

THE *Japan Weekly Mail* in a recent issue notices the publication of a kind of Japanese folk-lore journal, called the *Fuzoku Gwahōo*, the object of which is to collect and record important and curious Japanese national customs. Japanese customs, old and new, are classified by the new journal under seven heads—namely, customs that concern (1) human beings, (2) animals and plants, (3) dress and ornaments, (4) food and beverages, (5) buildings, (6) furniture and coins, (7) miscellaneous. For the illustrations resort is had to old pictures. Every number contains an essay on some interesting custom, with allusions to authorities on the subjects treated.

THE Government of India, it is reported, has decided to discontinue the annual grant hitherto devoted to search for, and purchase of, rare Sanskrit manuscripts, but the decision will not take effect until 1892. A regular staff of native searchers have been employed during the past ten years, and these have visited most of the large temples throughout India, examining and cataloguing the vast collections of works hoarded up there. The private libraries of several native gentlemen have been likewise carefully sifted, and their contents recorded. Of the manuscripts thus examined, no fewer than 2400 have been purchased by the Government, and rendered accessible to the public at Bombay and Calcutta. The most valuable "finds" have included numerous old Jain manuscripts, now being submitted to the scrutiny of competent scholars in Bombay. Although the search and purchase grants are to cease, the Indian Government has agreed to continue the allowance of Rs. 9000 per annum for the publication of texts and translations of the Sanskrit and Persian works discovered.

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Chrysothrix sciurea* ♀) from Guiana, presented by Mrs. Osgood; two Chinese Alligators (*Alligator sinensis*) from China, presented by Mr. D. C. Jansen; a Great-billed Touracou (*Corythaix macrorhyncha*) from West Africa, a Wonga-wonga Pigeon (*Leucosarcia picta*) from Australia, a Madagascar Love Bird (*Agapornis cana*) from Madagascar, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on September 4 = 20h. 55m. 52s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4627	—	—	20 57 10	+54 7
(2) G.C. 4628	—	Pale blue.	20 58 9	−11 48
(3) 3 Aquarii	5	Reddish-yellow.	20 41 56	− 5 21
(4) β Capricorni	3	Yellowish-white.	20 14 17	−15 10
(5) α Capricorni	4	White.	20 11 0	−12 53
(6) D.M. + 32° 3522 ...	8	Red.	19 36 44	+32 22
(7) V Cygni	Var.	Red.	20 37 46	+47 45

Remarks.

(1) The G.C. description of this nebula is: "Considerably bright; large; elongated in the direction 45° or thereabouts; barely resolvable." The spectrum appears to have been observed only by Dr. Huggins, who recorded:—"One bright line only was distinctly seen, of apparently the same refrangibility as the brightest of the nitrogen lines. This bright line appeared by glimpses to be double. Possibly this appearance was due to the presence near it of a second line. The faintness of the light did not permit the slit to be made sufficiently narrow for the determination of this point." This is an observation well worth repeating, as it may possibly throw some light on the origin of the chief nebula line. The magnesium fluting near λ 500 has been suggested, on various grounds, as the origin of the chief line, and this consists of a rhythmical series of flutings with well-defined edges towards the red end of the spectrum. It may be that the second faint line seen by Dr. Huggins was the second maximum of the compound fluting, but unfortunately he does not state whether it was more or less refrangible than the brighter line.

(2) It appears to be generally agreed that this is one of the finest specimens of planetary nebulæ in the heavens. Lassell saw it as an elliptic ring with a star in the centre. Dr. Huggins and Lieutenant Herschel each recorded three bright, sharp, and distinct lines in its spectrum, and Prof. Winlock suspected a fourth. The spectrum of this nebula might perhaps be advantageously observed in connection with that of the previous nebula. As the temperature of nebulæ indicating hydrogen is probably lower than that of nebulæ in which the hydrogen lines are absent, there is reasonable ground for supposing that the fluted appearance of the chief line (assuming it to be due to

magnesium) will be most obvious when the hydrogen lines are not seen. A comparison of the two spectra with the same instruments under similar conditions will therefore be valuable.

(3) This comparatively bright star of Group II. has not yet been observed in sufficient detail, Dunér simply stating that the bands 2-8 are wide and dark. For purposes of classification it is also necessary to know whether the bands in the blue or those in the red are most intense.

(4 and 5) These are stars of the solar type and of Group IV. respectively (Konkoly). The usual observations are required in each case.

(6) Dunér describes the spectrum of this star as one of Group VI., consisting of three zones, of which the blue is also pretty bright. The principal bands are very dark, and the secondary bands 4 and 5 (λ 589 and 576) were also occasionally seen. The brightness of the blue zone varies very considerably in stars of this group, and, moreover, does not depend upon the magnitude of the star. It probably therefore depends upon temperature. The associated phenomena are well worth investigation.

(4) This interesting variable will reach a maximum about September 6. The observations of the magnitude at maximum are a little discordant, but there can be no doubt that it changes considerably, the extremes being 6·8 and 9·5, whilst the minimum is a prolonged one of about magnitude 13. The spectrum is one of Group VI., showing very little blue light. Continuous spectroscopic observations will be very valuable in connection with Mr. Lockyer's theory of the cause of variability in stars of this group.

A. FOWLER.

VARIABLE STARS NEAR THE CLUSTER 5 M.—At the June meeting of the Royal Astronomical Society, Mr. A. A. Common, F.R.S., exhibited some photographs of the cluster 5 Messier, taken with his 5-foot telescope at Ealing. Four photographs had been taken on April 22, May 9, May 15, and June 9, with exposures of 25, 45, 66, and 45 minutes respectively. The plate taken on May 15—that is, the one with the longest exposure—contains five stars not shown on those taken before and after that date. The presence of these five stars was not due to longer exposures because they were all brighter than the 10th magnitude, whereas stars of at least the 12th magnitude were seen on all the plates. A great difference was also observed in the apparent magnitudes of many of the stars near the cluster.

Prof. E. C. Pickering notes (*Astronomische Nachrichten*, No. 2986) that an examination of the photographs of this region taken at Harvard College Observatory proves beyond doubt that the star about 9" or 10" south preceding the cluster varies between 9·76 and 11·6 magnitude, and that the south component of the wide pair just following the cluster varies between 9·3 and 12·2 magnitude.

NEW ASTEROIDS.—A new minor planet (295), of the 13th magnitude, was discovered by Dr. Palisa, at Vienna, on August 17; and another, (296), by Mr. Charlois, at Nice, on August 19. The latter was found near the position of Hera, (103), but because of the difference in magnitude it is thought to be new.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 25.—On a jawbone of a Greenland seal, found by M. Michel Hardy in the grotto of Raymonden.—Observations of the Denning (July 23, 1890) Comet, made at the Paris Observatory, by M. G. Bigourdan.—Observations of the new planet Palisa (Vienna, August 17, 1890), made at the Paris Observatory, by Mdlle. D. Klumpke.—Elements and ephemerides of the planet (294), discovered at the Nice Observatory, July 15, 1890, by M. Charlois.—On two forms of electrical gyroscopes, one serving to show the movement of the earth, and the other for the rectification of the marine compass, by M. G. Trouvé. The two instruments are similarly constructed, but the latter is heavier, and so hung as to be free from the various causes of disturbance always present on board. It is able to correct the compass with certainty, since its axis of rotation remains fixed in space, however long it is necessary to prolong the observation.—On the respiration of the grasshopper, by M. Ch. Contejean. The abdomen is chiefly concerned with the respiratory movements. Stimulation of the nervous system by applying induced electric currents causes an obvious acceleration in the breathing.—New researches on the production of light by animals and vegetables, by M. Raphael Dubois. The author concludes that the production of light in animal organisms is due to the transformation of the colloidal protoplasmic granulations into crystalloidal granulations, under the influence of a respiratory phenomenon.—On the presence of the carboniferous formation in Brittany, by M. P. Lebesconte. This paper contains a list of the fossils obtained from some newly-discovered fossil-bearing strata in the carboniferous limestones at L'Ille-et-Vilaine in Quenon.—On the storm of August 18, 1890, at Dreux, by M. Léon T. de Bort. In its local and destructive character this storm showed many analogies with the tornadoes of the United States.—Notes were also submitted by M. Chapel, on the coincidence of atmospheric disturbances with the meeting with the Perseids; by M. van Heyden, on the height of the atmosphere; and by M. E. Mathieu-Plessy, on a new base obtained by heating ammonium nitrate, possibly nitramide, $NO_2 . NH_2$.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Principia; or, the Three Octaves of Creation: Rev. A. Kennion (E. Stock).—Transactions and Proceedings of the New Zealand Institute, 1889, vol. xxii.: Sir J. Hector (Trübner).—Paul Nugent, Materialist, 2 vols., 3rd edition: H. F. Hetherington and Rev. H. D. Burton (Griffith and Farran).—Annual Report of the Department of Mines, New South Wales, for the year 1889 (Sydney).—Inorganic Chemistry: Wm. Jago (Longmans).—Wild Flowers of North Wales Coast; R. Darlington (Roper and Drowley).—Wild Flowers of Vale of Llangollen, &c.: R. Darlington (Roper and Drowley).—The Ethical Problem: Dr. P. Carus (Chicago, Open Court Publishing Company).—Report of the French Commission on the Use of Explosives in the Presence of Fire-Damp in Mines (Newcastle-upon-Tyne).—Zur Geschichte der Ältesten Haustiere: Dr. Otto (Breslau).—Untersuchungen über die Physiologischen Wirkungen der Lupetidine und verwandter Körper und deren Beziehungen zu ihrer chemischen Constitution: A. Gürber (Zürich).—Timehri, No. xvii. (Stanford).—Journal of the Chemical Society, August (Gurney and Jackson).

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THURSDAY, SEPTEMBER 11, 1890.

PRINCIPLES OF ORGANIC CHEMISTRY.

Principles of General Organic Chemistry. By Prof. E. Hjelt, Helsingfors. Translated from the Author's German Edition of the original work by J. Bishop Tingle, Ph.D. (London: Longmans, Green, and Co., 1890.)

THIS work is an English translation of the German edition of a book which originally appeared in Swedish, and its object is stated to be "to give in a short and clear form the most important points of general and theoretical organic chemistry." Paraphrasing a statement recently put forward by a well-known reviewer in these columns, we certainly doubt the propriety of translating German books of this kind into English, regarding the ignorance of German by a chemist as inexcusable, if not criminal: in our opinion, indeed, permission to study the science of chemistry should be contingent on proof being given of a competent knowledge of this language. But our objection does not rest alone on this basis: we should not even have recommended the translation of the work from Swedish, as we hold that its study must have a thoroughly demoralizing effect. It is impossible "to give the most important points of general and theoretical organic chemistry," in accordance with the plan adopted by the author, within so narrow a compass; and such a book can only serve the purposes of the crammer. The uselessness of attempting to construct a cottage from plans prepared for a mansion needs no proof, but it is just such an attempt that is made in the book under notice.

The book is divided into three parts. According to the translator, in Part I. the composition, constitution, and classification of organic compounds are discussed and explained as clearly and concisely as possible. Part II. is devoted to illustrating the connection between the constitution of organic compounds and their chief physical properties. Part III. deals with the chemical behaviour of organic compounds. In illustration of the treatment accorded to the various sections, it may be mentioned, however, that the whole subject of "Geometrical Isomerism," one of the most difficult of modern chemical problems, is dismissed in five pages; that optical properties occupy but five and a half; and that only three and a half are devoted to the discussion of specific gravity and specific volume in their relation to constitution.

Some among us contend that the study of chemical science affords logical training of a very high order, but certainly this would not be the opinion of any intelligent person unacquainted with the subject who chanced to read this book. Thus, what can be the value of such wretched mental pabulum as that supplied on pp. 42–43, where, after the briefest possible reference to the van t' Hoff-Le Bel hypothesis, we read, "Two doubly linked carbon atoms would be represented by a figure consisting of two tetrahedra with one edge in common. Two arrangements are possible of substances of the type $Cab = Cab$. Fumaric and maleic acids are examples of such compounds"? Then follow the two conventional double tetrahedron figures, and a few lines further comes the dogmatic assertion, "It can be proved that fumaric acid is constituted like Fig. 2, and maleic acid like Fig. 1. It is not possible here to give a systematic account of the principles upon which the discovery of geometrical isomerism is based." Fancy the effect of studying, let us say, Euclid on such principles, and the kindly reception the lad would meet with who told his master in class that "*it can be proved* that any two sides of a triangle are greater than the third," and whose knowledge went no further; yet this is about the position which a reader of this book would be placed in after perusing its fragmentary sentences. If the student be exceptionally intelligent, and be not satisfied with dogmatic assertions, what must, moreover, be his opinion of his teacher when later on he directs his attention to current literature, and finds that the constitution of fumaric and maleic acids is one of the questions which is being hotly contested among chemists; that it is not proved that either acid has the constitution represented by the figures given; that, in fact, it is pure assumption that such is the case; and that the determination of the constitution of these and similar acids is a problem of peculiar difficulty?

The translator tells us that "No pains have been spared in order to bring the work into harmony with the latest researches, though of course, from the very nature of the case, all controversial matter has been excluded." The first part of the sentence is distinctly misleading, and it is difficult to understand the meaning of the latter. Our methods of determining constitution are admittedly in so many cases imperfect and but roughly approximate; so much depends on individual judgment, and the point of view from which the interpretation is given; that, in discussing constitution and the relation of physical properties to structure, "controversy" cannot be excluded. The advancing student has the right to demand a statement of the arguments for and against, and to nourish him on dogma is to do him a grievous injury: his object being to learn to play the game himself later on, he desires to obtain an insight into its rules and moves, and his only chance of learning methods is to become acquainted with the methods and arguments of previous workers. An illustration is afforded by chapter xi., on heat of combustion and heat of formation, which extends to the inordinate length of two pages and a half. In this chapter reference is made to Thomsen's calculations of the thermal values of the different kinds of bonds between carbon atoms, and his conclusions are put forward in such a manner as to lead the student to suppose that they are based on cogent arguments. The author's preface being dated Helsingfors, February 1887, it is excusable that he should have been impressed by the weight of Thomsen's authority; but it is inexcusable that the translator, three years later, should overlook the criticisms that have been passed on Thomsen's work, and should fail to point out that the conclusions which this chemist based on his thermal studies of carbon compounds are frequently in absolute conflict with those deduced from the study of chemical behaviour. The survival at this date of the strange conclusion that in acetylenic compounds the carbon atoms are held together by less than no affinity clearly shows that common-sense after all is an uncommon sense. The concluding paragraph of chapter xi. is

one which it is perhaps undesirable to pass without remark :—

"All researches prove that unsaturated compounds possess a greater heat of combustion than saturated ones; their heats of formation are therefore less and their energy greater than that of compounds containing carbon atoms linked only by single bonds. The thermal behaviour of unsaturated compounds also shows that the so-called *double bond is a weaker, not a stronger, form of atomic attraction than the simple bond*."

The first of these sentences is a mere statement of fact; the second is an unwarrantable and illogical deduction from the facts, and yet the fallacy which it embodies is very generally overlooked. Chemists are persuaded that the ethylenic form of linkage is not the equivalent of *two* paraffinic linkages, but is considerably weaker; beyond this, however, all is surmise. It is not determined whether or no the carbon atoms in ethylenic compounds are united by more than a single affinity; and as we have no means at present of calculating the thermal equivalent of even a paraffinic linkage, thermal behaviour cannot enable us to judge which is the stronger form of atomic attraction—the paraffinic or the ethylenic. The greater stability of saturated as compared with unsaturated compounds would appear to be due to the greater readiness with which the latter are acted on. To defeat an enemy it is necessary to approach within striking distance; and so it is in affairs chemical. The vulnerable points in saturated compounds are few or limited in extent, but in the case of the unsaturated it is easy for the attacking party—the chemical agent—to effect a lodgment.

Our criticisms thus far have had reference chiefly to Parts I. and II.; but of Part III., which is the more important section of the book and the more novel in plan, we cannot speak in terms much less unfavourable. We can only say: Defend us from the student whose knowledge of the general behaviour of organic compounds has been derived from such a course of study. We wish, in the interests of English chemical students, that the book had remained untranslated.

H. E. A.

THE THEORY OF INTEREST.

Capital and Interest: a Critical History of Economic Theory. By Prof. Eugen von Böhm-Bawerk. Translated by William Smart, M.A. (London: Macmillan and Co., 1890.)

PROF. SMART shares with Mr. James Bonar the honour of introducing to the English public a leader of the important Austrian school of economists. Mr. Bonar, in the *Quarterly Journal of Economics*, transfuses into his own happy style the spirit of Prof. Böhm-Bawerk's theory of value. Prof. Smart translates the same writer's theory of interest, which, to be fully appreciated, should be read in connection with the earlier work. The translation is enhanced by an analysis and a preface, in which the author's theory is, so to speak, "brought down to earth," and adapted, by examples taken from the highway and the market-place, to the comprehension of the wayfaring man. Referring to his own labours, Mr. Smart makes a suggestion which deserves attention :—

"The time I have given to this work may excuse my suggesting that a valuable service might be rendered to the science, and a valuable training in economics given, if clubs were organized, under qualified professors, to translate, adapt, and publish works which are now indispensable to the economic student."

Mr. Smart should be one of these professors; for he has proved himself to be eminently qualified, not only to translate, but to adapt an important work.

One quality of this work, about the excellence of which there can be no question, is the learning with which it abounds. The Austrian economists rival their German neighbours of the exclusively "historical" school in laboriousness of research. He must be a ripe scholar to whom many even of the names, as well as matters, in our author's review of theorists and theories are not new. We shall not expose our own ignorance by mentioning the writers of whom we had never heard before. As an instance of one whose name was not unknown, but whose position in economical history was not sufficiently recognized, may be noticed Salmasius. The average English reader is aware that Salmasius was underrated by Milton and his biographer, Dr. Johnson. But it requires Prof. Böhm-Bawerk's acquaintance with economic literature to realize how much Salmasius contributed to the explosion of the old prejudices against interest. Not only does his doctrine

"indicate an advance, but it long indicates the high-water mark of the advance. . . . There was no essential advance on Salmatius (in respect of the theory of interest) till the time of Smith and Turgot."

J. B. Say, if we remember rightly, has observed that there is not much use in studying the theories of the earlier economists, as they were mostly wrong. Prof. Böhm-Bawerk evidently does not accept this somewhat Philistine conclusion. But we suspect that he does not deny the premiss. For it appears to be the motive of this "Critical History of Economic Theory" to prove that all preceding economists have gone astray, and fallen short of the glory which we fully concede appertains in a special degree to Prof. Böhm-Bawerk as the formulator of the true theory of interest. Now we cannot agree to the negative proposition here implied. Our approbation of Prof. Böhm-Bawerk does not rest upon the censure of his predecessors. Of course it must be admitted that on the theory of interest, as on other economical subjects, a great deal of nonsense has been talked. But—hindered perhaps by the proverbial difficulty of unlearning the lessons of youth—we can hardly believe that the leaders of economic thought, that Ricardo and Senior and J. S. Mill, deserve to be involved in such a sweeping condemnation.

In expressing this doubt we shall shelter ourselves behind the authority of one to whom most readers conversant with economic science will be disposed to defer. In one of the scrupulously weighed notes attached to the epoch-making work which Prof. Alfred Marshall has just published, he thus refers to Prof. Böhm-Bawerk :—

"The question may be raised whether he has not somewhat exaggerated the difference between his own position and that of his predecessors; whether the sharp contrasts which he finds between the doctrines of successive schools really existed, and whether those doctrines

were generally as fragmentary and one-sided as he thinks."

Without attempting to add to words so weighty, we shall follow the excellent example of him who had to speak after Mr. Burke, and shall simply "say ditto" to Prof. Marshall.

It is not be supposed that our remarks are calculated to disparage the truth and importance of Prof. Böhm-Bawerk's own theory of interest. It would argue a very slight acquaintance with economical literature to suppose that the worth of an author's own work is to be measured by the worth of his criticisms upon the work of others. Prof. Böhm-Bawerk is not the less right because those from whom he differs are not wrong. We are not precluded from expressing unqualified admiration of the *positive theory* which forms the sequel to this too "critical" history.

F. Y. E.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Soaring of Birds.

IN NATURE of August 21 (p. 397), a new solution has been offered of the problem of the soaring of birds, which will hardly be accepted as satisfactory; for it is based on the radical misconception that the absolute velocity with which a bird soars, or flies, partly with the air, and partly through the air, can be converted, wholly or in part, into a motion through the air, at a swifter rate than before. This is only possible when the bird passes from one current into another, or the current itself is changed in rate or direction. It is strange that anyone should fail to see that when a body of air is moving uniformly over the land or sea, it can no more sustain a bird flying or gliding within it than if it were motionless.

Referring to your correspondent's first diagram, "It is evident," he says, "that the velocity of the bird at the point *c*, if the initial velocity of the bird and the velocity of the wind are properly adapted" (whatever that may mean) "can be greater than at *a*. He appears to think that the length of the line *a c* is some measure of the velocity; whereas, in fact, this velocity will be the same, or rather greater, indefinitely near to *a* than it is at *c*; the velocity of the bird through the air being then at its maximum, and that of the wind uniform. Nothing is gained by the length of *c a*. A movement in the direction *c d* might be at least as advantageously made close to *a* as further along the line.

It is hardly worth while to press any other objection to the explanation offered. But it may be noted that if the diagrams given are intended to represent the actual facts the soaring bird is being rapidly carried to leeward, which is a misrepresentation. Let me offer what I believe to be an apt illustration. While a billiard ball is rolling across a table, its direction being again and again changed by its rebounds from the cushions, the table is moved forward through space at the rate of about a thousand miles a minute. This movement ought, according to your correspondent, to accelerate or retard the motion of the ball across the table, or at least ought to do so if the two movements were "properly adapted."

REGINALD COURTENAY.

Tean Vicarage, Stoke-on-Trent, September 1.

[P.S.—In NATURE for September 4 (p. 457), just received, are remarks on the soaring of birds, by two of your correspondents, which seem to me to be perfectly just; only I would add that the upward currents of which soaring birds avail themselves are commonly due to obstacles to the uniform motion of the wind. Also, that a soaring bird, if turning abruptly, will in general not merely uplift, but strike once, with the outer wing.]

The Affinities of *Heliopora cærulea*.

OWING to absence from Dublin I have but just seen the correspondence on the affinities of *Heliopora cærulea* in two recent numbers of NATURE (pp. 349, 370). Dr. S. J. Hickson has undoubtedly given the true explanation of Mr. Saville Kent's "curious mistake." Knowing that the polyps of this Alcyonarian had never before been observed in expanded condition, I took every opportunity when in Torres Straits of examining living specimens *in situ*. Only on one occasion was I rewarded, and then I distinctly saw the small extruded polyps with their eight flat fringed tentacles; they were nearly colourless, but had a whitish tinge, in fact, they precisely resembled the polyps of the common *Alcyonium digitatum*. I was unable to sketch them at the time, as my bodily position was unfavourable, and the tide was rising, In no case did the polyps exhibit any sign of vitality when kept in a vessel of water in my laboratory. The only published drawing (so far as I remember) purporting to be the polyp of this form is that given by Quoy and Gaimard, but whatever it may be, it represents neither the polyp itself nor the annelid described by Mr. Saville Kent.

ALFRED B. HADDON.

Royal College of Science, Dublin.

Occurrence of a Crocodile on Cocos Islands.

WITH reference to Mr. Ridley's account of the occurrence of a crocodile on the Cocos Islands, I was quartered in Barbados in the beginning of 1885, when a very fine alligator, over 15 feet in length, was washed on shore. As it was on the point of crawling up the beach it was noticed by a sergeant of engineers and some sappers who shot it, and afterwards exhibited it in the town.

The nearest river the alligator could have come from was the Orinoco, a distance of 300 miles.

This is improbable, as the set of the ocean currents would have sent the alligator much to the west of Barbados if it had come from the Orinoco. It is much more probable the alligator came from the mouth of the Amazon or from the Essequibo, many hundred miles further to the east.

Dr. Mitchell, of Trinidad, told me he had seen an alligator on a small log attacked by sharks in the Gulf of Paria.

A. L. CALDWELL.

A.S. Corps, Chatham, September 6.

THE BRITISH ASSOCIATION.

LEEDS, *Wednesday Morning*.

THE Leeds meeting has been small and quiet, the attendance only numbering over 1700. Happily the weather has been excellent, and brilliant sunshine has cast a glow over the ugliness of the place, and rendered the excursions to the Aire and the Wharfe valleys delightful. The usual social accompaniments of British Association meetings have not been so plentiful as at former meetings, but every one seems satisfied with the hospitality displayed.

In the proceedings of the meeting there have been one or two matters of excitement and some marked successes. Sir Frederick Abel considerately cut down an address which was more suited for the study than the platform. It is universally admitted that no more successful lectures have ever been delivered at an Association meeting than those of Mr. Poulton on Friday, on "Mimicry," and Prof. Boys on Monday, on "Quartz Fibres." The large audiences were really entranced.

It can hardly be said that any paper of high and wide scientific importance has been read this year in any of the Sections. There have, however, been several most important discussions on the reorganization of some of the Sections, which attracted much attention, and several changes are to be made.

Next year's meeting of the Association will take place at Cardiff, when Dr. Huggins will preside. The 1892 meeting will be held at Edinburgh, and the 1893 at Nottingham.

The following is the list of grants to be submitted to the meeting of the General Committee to-morrow:—

A.—Mathematics and Physics.	£
Seismological Phenomena of Japan	10
Electrical Standards	100
Meteorological Observations on Ben Nevis	50
Electrolysis	5
Photographs of Meteorological Phenomena	5
Discharge of Electricity from Points	10
Ultra-Violet Rays of Solar Spectrum	50
Seasonal Variations of Temperature	20
B.—Chemistry.	
Analysis of Iron and Steel	10
Isomeric Naphthalene Derivatives	25
Formation of Haloid Salts	25
Action of Light upon Dyes	20
C.—Geology.	
Erratic Blocks	10
Fossil Phyllopoda	10
The Geological Record	100
Photographs of Geological Interest	10
Lias Beds in Northamptonshire	20
Registration of Type Specimens of British Fossils	10
Volcanic Phenomena of Vesuvius	10
Underground Waters	5
Investigation of Elbolton Cave	25
D.—Biology.	
Marine Biological Association at Plymouth	30
Botanical Station at Peradeniya	50
Improving Deep-sea Tow-net	40
Disappearance of Native Plants	5
Zoology of the Sandwich Islands	100
Zoology and Botany of the West India Islands	100
E.—Geography.	
Normal Tribes of Asia Minor and Northern Persia	30
G.—Mechanical Science.	
Action of Waves and Currents in Estuaries	150
H.—Anthropology.	
New Edition of "Anthropological Notes and Queries"	50
Anthropometric Laboratory	10
North-western Tribes of Canada	200
Habits of Natives of India	10
Corresponding Societies	25
	£1330

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY J. W. L. GLAISHER, SC.D., F.R.S., PRESIDENT OF THE SECTION.

No one who is called upon to preside over this Section can fail to be struck by the range of subjects comprehended within its scope. The field assigned to us extends from the most exact of all knowledge, the sciences of number, quantity, and position, to branches of inquiry in which the progress has been so slight that they still consist of little more than collections of observed facts. This breadth of area has obvious disadvantages, but it is not without some compensating advantages. In these days, when science is so much subdivided, it is well that students of subjects even so diverse as those with which we have to deal should occasionally meet on common ground, and have the opportunity of learning from each other's lips the kind of work in which they are engaged. Wide as is our range, we should remember also how closely knit together in various ways are the more important of our subjects; and in the case of mathematics, astronomy, and physics, besides their actual and historical alliance, a mathematician may be permitted to feel that a special bond of union is created by the mathematical processes and language which are essential for their investigation and expression.

It is, I am afraid, unfortunate for my audience, that my own subject should be at one extreme, not only of those dealt with by our Section, but even of the still greater range covered by the Association. I will endeavour, however, in my remarks to confine myself to a few general considerations relating to pure mathematics, which I hope will not be considered out of place on this occasion.

By pure mathematics I do not mean the ordinary processes of algebra, differential and integral calculus, &c., which every worker in the so-called mathematical sciences should have at his command. I refer to the abstract sciences which do not rest upon experiment in the ordinary sense of the term, their fundamental principles being derived from observations so simple as to be more or less axiomatic. To this class belong the theories of magnitude and position, the former including all that relates to quantity, whether discrete or continuous, and the latter including all branches of geometry. The science of continuous magnitude is alone a vast region, containing many beautiful and extensive mathematical theories. Among the more important of these may be mentioned the theories of double and of multiple periodicity, the treatment of functions of complex variables, the transformation of algebraical expressions (modern algebra), and the higher treatment of algebraical and differential equations as distinguished from their mere solution. It is this kind of scientific exploration which fascinates and rewards the pure mathematician, and upon which his best work is most profitably spent. I do not wish to under-estimate the importance of such a subject as finite differences, in which a number of distinct problems are treated with more or less success by interesting methods specially adapted to their solution. Nor would I willingly undervalue the interest of those branches of mathematics which we owe to the mathematical necessities of physical inquiry. But it always appears to me that there is a certain perfection, and also a certain luxuriance and exuberance in the pure sciences which have resulted from the unaided, and I might almost say inspired genius of the greatest mathematicians which is conspicuously absent from most of the investigations which have had their origin in the attempt to forge the weapons required for research in the less abstract sciences. To illustrate my meaning, I may take as an example of a subject of the latter class the theory of Bessel's functions. The object of mathematicians in this case has been to investigate the properties of functions which have already presented themselves in astronomy and physics. Formulæ for their calculation by means of series, continued fractions, definite integrals, &c., have been obtained in profusion, numerous theorems of various kinds and applicable to different purposes have been discovered, extensions and developments have been made in all directions, and, finally, the large body of interesting analysis thus accumulated has been classified and systematized. But, valuable and suggestive as are many of the results and processes, such a collection of facts and investigations is necessarily fragmentary. We do not find the easy flow or homogeneity of form which is characteristic of a mathematical theory properly so called. In such a theory as, for example, the theory of double periodicity (elliptic functions), the subject develops itself naturally as it proceeds; one group of results leads spontaneously to another; new and unexpected prospects open of themselves; ideas the most novel and striking, which penetrate the mind with a charm of their own, spring directly out of the subject itself. We are surprised by the wonderful connections with other subjects which unexpectedly start into existence, and by the widely different methods of arriving at the same truths; in fact, as our knowledge progresses, we continually find that results which seemed to lie far away in the interior of the subject—so remote and concealed that, at first sight, we might think that no other path except the one actually pursued could have reached them—are actually close to its edge when approached from another side, or viewed from another stand-point. We notice, too, that any great theory gives rise to its own special analysis or algebra, frequently connecting together into one whole what were hitherto merely isolated and apparently independent analytical results, and affording a reason for their existence, and also—what is often even more interesting—a reason for the non-occurrence of others, which analogy might have led us to expect. I do not

pretend that there are not many branches of mathematics which partake of both these characters, nor do I suppose that the description I have given of a mathematical theory is at all peculiar to pure mathematics. Much of it is common to all scientific research in a fruitful field, though, possibly, we may not find elsewhere such profusion of ideas or perfection of form.

I have been tempted to speak at such length on the objects and aims of the mathematician by the feeling that they are not infrequently misunderstood by the workers in the less abstract sciences. I do not think that mathematical formulæ or processes, merely as such, are much more interesting to the pure than to the applied mathematician. The one studies number, quantity, and position, the other deals with matter and motion; and in both cases the investigations are carried on by means of the same symbolic language.

The order in which the subjects which form an ordinary mathematical course are presented to the student is regulated by the fact that portions of the elements of the pure sciences are required for the explanation and development of any exact science; for example, a knowledge of the elements of trigonometry, analytical geometry, and differential and integral calculus, must necessarily precede any adequate treatment of mechanics, light, or electricity. The majority of students, after mastering a sufficient amount of pure mathematics to enable them to pass on to the physical subjects, continue to devote their attention to the latter, and never know more of the nature of the pure sciences than they can derive from the processes and methods which they learned at the very outset of their mathematical studies. This is necessarily the case with many of the wranglers, as the first part of the Mathematical Tripos includes no true mathematical theory. Most of the mathematical text-books in use at Cambridge are so admirably adapted to the purposes for which they are intended that it seems ungracious to make an adverse criticism of a general kind. But I cannot help feeling regret that their writers have had so much in view the immediate application of the principles of the pure subjects to the treatment of physical problems. In the case of the differential and integral calculus, for example, there seems an increasing tendency to introduce into the book-work and examples propositions which really belong to the physical subjects. This is an important tribute to the growth and influence of physical mathematics in this country, and a zealous physicist might even consider it satisfactory that the student should not be required to encumber himself with knowledge which was not directly applicable to the theory of matter. But from the mathematician's point of view it is unfortunate, for, while shortening by very little the path of the student, it cannot fail to give an incomplete, if not erroneous, idea of the relations of the pure to the applied sciences. How can he help feeling that the former are merely ancillary to the latter when he finds that the mathematical problems which arise naturally in physical investigations have been already dealt with out of their place in the treatises which should have been devoted solely to the sciences of quantity and position?

Perhaps few persons who have not had the matter forced upon their attention fully realize how fragmentary and unsatisfactory is the treatment of even those fundamental subjects in pure mathematics which form the groundwork of any course of mathematical study. Algebra is necessarily the first subject set before the student; it has therefore to be adapted to the beginner, who at that time is only learning the first elements of the language of analysis. It is customary to regard trigonometry as primarily concerned with the solution of triangles; the geometrical definitions of the sine and cosine are therefore adopted, and after the application of the formulæ to practical measurement and calculation a new departure is made with De Moivre's theorem. The elementary portions of the theory of equations, and the differential and integral calculus and differential equations, are valuable collections of miscellaneous principles, processes, and theorems, useful either as results or as instruments of research, but possessing no great interest of their own. Analytical geometry fares the best, for it includes one small subject—curves of the second order—which is treated scientifically and with thoroughness. It is true, however, that the course of reading just mentioned includes one theory which, though itself an imperfect one, receives a tolerably complete development—I mean the theory of singly periodic functions; but it is dispersed in such small fragments among the various subjects that it does not naturally present itself to the mind as a whole. If we could commence this theory by considering analytically the forms and necessary properties of functions of one period (thus obtaining their definitions as series and products), and could then proceed to a detailed discussion of the functions so defined—including their derivatives, the integrals involving them, the representation of functions by their means in series (Fourier's theorem), &c.—we should obtain a connected system of results relating to a definite branch of knowledge which would give a good idea of the orderly development of a mathematical theory; but the fact that the student at the time of his introduction to sines and cosines is supposed to be ignorant of all but the most elementary algebra, places great difficulties in the way of any such systematic treatment of the subject.

Passing now to the consideration of pure mathematics itself, that is to say, of the abstract sciences, which can only be conquered and explored by mathematical methods, it is difficult not to feel somewhat appalled by the enormous development they have received in the last fifty years. The mass of investigation, as measured by pages in Transactions and Journals, which is annually added to the literature of the subject, is so great that it is fast becoming bewildering from its mere magnitude, and the extraordinary extent to which many special lines of study have been carried. To those who believe, if any such there are, that mathematics exists for the sake of its applications to the concrete sciences, it must indeed seem that it has long since run wild, and expanded it elf into a thousand useless extravagances. Even the mathematician must sometimes ask himself the question—not infrequently put to him by his friends —"To what is it all tending? What will be the result of it all? Will there be any end?" The last question is readily answered. There certainly can be no end; so wide and so various are the subjects of investigation, so interesting and fascinating the results, so wonderful the fields of research laid open at each succeeding advance—no matter in what direction—that we may be sure that, while the love of learning and knowledge continue to exist in the human mind, there can be no relaxation of our efforts to penetrate still further into the mysterious worlds of abstract truth which lie so temptingly spread before us. The more that is accomplished, the more we see remaining to be done. Every real advance, every great discovery, suggests new fields of inquiry, displays new paths and highways, gives us new glimpses of distant scenery. This wonderful suggestiveness is itself one of the marks of a true theory, one of the signs by which we know that we are investigating the actual, existing truths of Nature, and that our symbols and formulæ are expressing facts quite independent of themselves, though decipherable only by their means. As for the other questions, it is very difficult to render intelligible, even to a mathematician, the kind of knowledge acquired by mathematical research in a new field until he has made himself acquainted with its processes and notation, and we cannot hope to find in the remote regions of an abstract science many results so simple and striking as to appeal forcibly to the imagination of those who are unfamiliar with its conceptions and ideas. It would seem, therefore, that the question, "To what is it all tending?" could never be answered in general terms. I do not think any mathematician could see his way to a reply, or even give definite meaning to the question. He might feel daring enough to predict the probable drift of his own subject, but he could scarcely get a broad-enough view to enable him to indulge his fancy with respect to more than a very minute portion of the field already open to mathematical investigation. To the outsider I am afraid that the subject will continue to present much the same appearance as it does now; it will always seem to be stretching out into limitless symbolic wastes, without producing any results at all commensurate with its expansion.

Instead of attempting to consider the general question of what may be expected to result from the progress of mathematical science, we may restrict ourselves to asking whether the great extension of the bounds of the subject which is taking place in our time will materially add to its powers as a weapon of research in the concrete subjects. This is a question of the highest interest, and one that cannot fail to have occupied the thoughts of every mathematician at some time or another in the course of his work. For my own part, I do not think that the bearing of the modern developments of mathematics upon the physical sciences is likely to be very direct or immediate. It would indeed be rash to assert that there is any branch of mathematics so abstract or so recondite that it might not at any moment find an application in some concrete subject; still it

seems to me that if the extension of the pure sciences could only be justified by the value of their applications, it is very doubtful whether a satisfactory plea for any further developments could be sustained. As a rule each subject involves its own ideas and its own special analysis, and it can only occasionally happen that analytical methods devised for the expression and development of one subject will be found to be appropriate for another. It is obvious also that the chance of such applications becomes less and less as we travel farther and farther from the elementary processes and methods which are common to all the exact sciences. There is a general resemblance of style running through much of the analysis required in the physical sciences, but there is no such resemblance in the case of the pure sciences, or between the pure and the physical sciences. It appears likely therefore that, in the future, the mathematical obstacles which present themselves in physical research will have to be overcome, as heretofore, by means of investigations undertaken for the purpose, and that analysis will continue to be enriched by conceptions and results, and even by whole subjects (such as spherical harmonies), which will be entirely due to the concrete sciences. Of course, it will sometimes happen that a differential equation or an integral has already been considered in connection with some other theory, or a whole body of analysis or geometry will suddenly be found to admit of a physical interpretation; but, after all, even the pure sciences themselves exert but an indirect effect upon the perfection of mathematical formulæ and processes, and we must be prepared to find that in general the requirements of physics have to be met by special analytical researches. Having now endeavoured to consider the proposed question impartially, and from a cold and rational standpoint, I cannot refrain from adding that, in spite of all I have said, I believe that every mathematician must cherish in his heart the conviction that at any moment some special analysis, devised in connection with a branch of pure mathematics, may bear wonderful fruit in one of the applied sciences, giving short and complete solutions of problems which could hitherto be treated only by prolix and cumbrous methods. For example, it is difficult to believe that the present unwieldy and imperfect treatment of the lunar theory is the most satisfactory that can be devised. We cannot but hope that some happy discovery in pure mathematics may replace the clumsy and tedious series of our day by simple and direct analytical methods exactly suited to the problem in question. In the different branches of pure mathematics, we find not infrequently that researches connected with one subject incidentally throw a flood of light upon another, and that we are thus led to solutions of problems and explanations of mysteries which would never have yielded to direct attack in the complete absence of any guide to the proper path to be pursued. So, too, in the lunar theory, if the direct attack should fail to supply any better treatment of the subject, we cannot but hope that some day the development of a new branch of mathematics, entirely unconnected with dynamics, may supply the key to the required method. It should be remembered also that dynamics, which differs from the pure sciences only by the inclusion of the laws of motion, is but little removed from them in the character of its more general problems.

It would seem at first sight as if the rapid expansion of the region of mathematics must be a source of danger to its future progress. Not only does the area widen, but the subjects of study increase rapidly in number, and the work of the mathematician tends to become more and more specialized. It is of course merely a brilliant exaggeration to say that no mathematician is able to understand the work of any other mathematician, but it is certainly true that it is daily becoming more and more difficult for a mathematician to keep himself acquainted, even in a general way, with the progress of any of the branches of mathematics except those which form the field of his own labours. I believe, however, that the increasing extent of the territory of mathematics will always be counteracted by increased facilities in the means of communication. Additional knowledge opens to us new principles and methods which may conduct us with the greatest ease to results which previously were most difficult of access; and improvements in notation may exercise the most powerful effects both in the simplification and accessibility of a subject. It rests with the worker in mathematics not only to explore new truths, but to devise the language by which they may be discovered and expressed; and the genius of a great mathematician displays itself no less in the notation he invents for deciphering his subject than in the results attained. There are some theories in which the notation seems to arise so simply and naturally out of the subject itself, that it is difficult to realize that it could have required any creative power to produce it; but it may well have happened that in these very cases it was the discovery of the appropriate notation which gave the subject its first real start, and rendered it amenable to effective treatment. When the principles that underlie a theory have been well grasped, the proper notation almost necessarily suggests itself, if it has not been already discovered; but some sort of provisional notation is required in the early stages of a theory in order to make any progress at all, and the mathematician who first gains a real insight into the nature of a subject is almost sure to be the first to seize upon the right notation. I have great faith in the power of well-chosen notation to simplify complicated theories and to bring remote ones near; and I think it is safe to predict that the increased knowledge of principles and the resulting improvements in the symbolic language of mathematics will always enable us to grapple satisfactorily with the difficulties arising from the mere extent of the subject.

Quite distinct from the theoretical question of the manner in which mathematics will rescue itself from the perils to which it is exposed by its own prolific nature is the practical problem of finding means of rendering available for the student the results which have been already accumulated, and making it possible for a learner to obtain some idea of the present state of the various departments of mathematics. This is a problem which is common to all rapidly moving branches of science, although the difficulties are increased in the case of mathematics by its wide extent and the comparative smallness of the audience addressed. The great mass of mathematical literature will be always contained in Journals and Transactions, but there is no reason why it should not be rendered far more useful and accessible than at present by means of treatises or higher text-books. The whole science suffers from want of avenues of approach, and many beautiful branches of mathematics are regarded as difficult and technical merely because they are not easily accessible. Ten years ago I should have said that even a bad treatise was better than none at all. I do not say that now, but I feel very strongly that any introduction to a new subject written by a competent person confers a real benefit on the whole science. The number of excellent text-books of an elementary kind that are published in this country makes it all the more to be regretted that we have so few that are intended for the more advanced student. As an example of the higher kind of text-book, the want of which is so badly felt in many subjects, I may mention the second part of Prof. Chrystal's "Algebra" published last year, which in a small compass gives a great mass of valuable and fundamental knowledge that has hitherto been beyond the reach of an ordinary student, though in reality lying so close at hand. I may add that in any treatise or higher text-book it is always desirable that references to the original memoirs should be given, and, if possible, short historical notices also. I am sure that no subject loses more than mathematics by any attempt to dissociate it from its history.

There is no more striking feature in the mathematical literature of our day than the numerous republications in a collected form of the writings of the greatest mathematicians. These collected editions not only set before us as a whole the complete works of the masters of our science, but they make it possible for others besides those who reside in the vicinity of large libraries to become acquainted with the principal contributions with which it has been enriched in our century; and, besides being of immense advantage to the science at large, they even go some way towards supplying the want of systematic introductions to the advanced subjects. Among these republications the collected edition of Cayley's works, now in course of publication by the University of Cambridge, is deserving of especial notice. By undertaking this great work, not only in the lifetime of its author, but while in the full vigour of his powers, the University has secured the inestimable advantage of his own editorship, and thus, under the very best auspices, the world is now being placed in full possession of this grand series of memoirs, which already cover a period of nearly fifty years.

Although it may not be possible to contemplate the actual position of pure mathematics in this country with any great amount of enthusiasm, we may yet feel some satisfaction in reflecting that there is more cause for congratulation at present than there has been at any time in the last hundred and fifty years, and that we are far removed from the state of affairs which existed before the days of Cayley and Sylvester. Unfortunately,

we cannot point with pride to any distinct school of the pure sciences corresponding to the Cambridge school of mathematical physics, and I am afraid that the old saying that we have generals without armies is as true as ever. For this there is no immediate remedy; a school must grow up gradually of itself, as the study of mathematical physics has grown up at Cambridge. I certainly should not wish, even if it were possible, to obtain more recruits for the pure sciences at the expense of the applied, nor do I desire to see the system of instruction which has found favour in this country so modified that pure mathematics could be carried on by narrow specialists. I should be sorry, for example, that a student, after learning algebra and differential calculus, should pass directly to the theory of curves, and devote himself to research in this field without ever having acquired a general knowledge of other branches of mathematics or of any of its applications. Every person who proposes to engage in mathematical research should be equipped at starting upon his career with some knowledge of at least all the subjects included in the first part of the Mathematical Tripos. From what I have said in an earlier portion of this address it may be inferred that, from the point of view of the pure mathematician, I think that the course of study, and some of the text-books, are capable of improvement, but I am satisfied that a general mathematical training such as the Tripos requires is of the greatest possible value to every student, and that without it he cannot even make a good decision as to the class of subjects to which he is likely to devote his labour with the best effect. If the student were brought by the shortest possible route to the frontier of one of the subjects, where a fruitful field of research was pointed out to him, there is no doubt that the amount of mathematical literature produced might be greatly increased, but I am sure that the advantage to science would not be proportional to this increased amount. I am convinced that no one should devote himself to the abstract sciences unless he feels strongly drawn to them by his tastes. These subjects are treated by means of a powerful symbolic language, and it is the business of the investigator to discriminate between equations and formulæ which represent valuable facts in Nature and those which are merely symbolic relations, deducible from others that are more fundamental, and having no special significance in the subject itself. The mathematician requires tact and good taste at every step of his work, and he has to learn to trust to his own instinct to distinguish between what is really worthy of his efforts and what is not; he must take care not to be the slave of his symbols, but always to have before his mind the realities which they merely serve to express. For these and other reasons it seems to me of the highest importance that a mathematician should be trained in no narrow school; a wide course of reading in the first few years of his mathematical study cannot fail to influence for good the character of the whole of his subsequent work.

Before leaving this part of my subject I should like to say a few words upon the subject of accuracy of form in the presentation of mathematical results. In other branches of science, where quick publication seems to be so much desired, there may possibly be some excuse for giving to the world slovenly or ill-digested work, but there is no such excuse in mathematics. The form ought to be as perfect as the substance, and the demonstrations as rigorous as those of Euclid. The mathematician has to deal with the most exact facts of Nature, and he should spare no effort to render his interpretation worthy of his subject, and to give to his work its highest degree of perfection. "*Pauca sed matura*" was Gauss's motto.

The Universities are the natural home of mathematics, and to them we chiefly owe its cultivation and encouragement. There is, however, one other much younger body whose services to our science should not be passed over in any survey of its present state—I mean the London Mathematical Society. Twenty-five years ago, upon its foundation, I think the most sanguine mathematician would scarcely have ventured to predict that it would so soon take the position that it has among the scientific institutions of the world. The continuous interest taken by its members in its meetings, and the number and value of the papers published by it, show how steadily the flame of mathematical inquiry is burning among us. I do not presume to assert that the interest taken in the pure sciences can be regarded as an index of the energy and power of a nation, but it is certain that mathematical research flourishes only in a vigorous community. The search after abstract truth for its own sake, without the smallest thought of practical application or return in any form, and the yearning desire to explore the unknown, are signs of the vitality of a people, which are among the first to disappear when decay begins.

In conclusion, I will refer in some detail to one special subject—the Theory of Numbers. It is much to be regretted that this great theory, perhaps the greatest and most perfect of all the mathematical theories, should have been so little cultivated in this country, and that no portion of it should ever have been included in an ordinary course of mathematical study. It may be said to date from the year 1801, when Gauss published his "Disquisitiones Arithmeticæ," so that it is nearly thirty years older than the Theory of Elliptic Functions, to which we may assign the date 1829, the year in which Jacobi's "Fundamenta Nova' appeared. But the latter theory has already found a congenial home among us, while the former is nowhere systematically studied, and is still without a text-book. The chapters in books upon Algebra which bear the title "Theory of Numbers" give a misleading idea of the nature of the subject, the results there given being mainly introductory lemmas of the simplest kind. The theory has nothing to do with arithmetic in the ordinary sense of the word, or numerical tables, or the representation of numbers by figures in the decimal system or otherwise. All its results are actual truths of the most fundamental kind, which must exist *in rerum naturâ*. Its principal branches are the theory of forms and the so-called complex theories. Such a proposition as that every prime number, which when divided by 4 leaves remainder 1, can always be expressed as the sum of two squares, and that this can be done in one way only, affords a good example of a very simple result in the theory of forms. It is entirely independent of any method of representing numbers, and merely asserts that if we have 5, 13, 17, 29, &c., things—let us say marbles, to fix the ideas—we can always succeed in so arranging them as to form them into two squares, and that for each number we can do this in but one way. Simple as such a theorem is to enunciate and comprehend, the demonstration is far from easy. This is characteristic of the whole subject; simple propositions, which we can easily discover by trial, and of the universal truth of which we can feel but little doubt, require for their demonstration a refined and intricate analysis, founded upon the most difficult and imaginative conceptions which mathematics has as yet attained to in its struggles to grapple with the actual problems of the worlds of thought and matter.

The theory of quantity consists of two distinct branches—one relating to discrete quantity, and the other to continuous quantity. To the latter branch belong algebra and all the ordinary subjects of pure mathematics; the former bears the name of the theory of numbers. Its truths are of the most absolute kind, involving only the notions of number and arrangement; in fact, if we imagine all the exact sciences ranged in order, it naturally takes its place at one end of the series. Different sciences appeal to different intellects with very different force, but there are some minds over which the absolute character of the fundamental truths that belong to this theory and the absolute precision of its methods exercise the strongest fascination, and excite an interest which neither the truths of geometry nor the most important discoveries depending upon the constitution of matter are capable of producing.

Many of the greatest masters of the mathematical sciences were first attracted to mathematical inquiry by problems relating to numbers, and no one can glance at the periodicals of the present day which contain questions for solution without noticing how singular a charm such problems still continue to exert. This interest in numbers seems implanted in the human mind, and it is a pity that it should not have freer scope in this country. The methods of the theory of numbers are peculiar to itself, and are not readily acquired by a student whose mind has for years been familiarized with the very different treatment which is appropriate to the theory of continuous magnitude; it is therefore extremely desirable that some portion of the theory should be included in the ordinary course of mathematical instruction at our Universities. From the moment that Gauss, in his wonderful treatise of 1801, laid down the true lines of the theory, it entered upon a new day, and no one is likely to be able to do useful work in any part of the subject who is unacquainted with the principles and conceptions with which he endowed it.

Undoubted the subject is a difficult and intricate one even in its elementary parts, but there can be but little doubt that when the processes which are now only read by specialists on their way to the border become more generally known and studied, they will be found to admit of great simplification. It is in fact a territory where there is quite as much scope for the mathe-

matician in simplifying what has been already won as in securing new conquests. I hope that the apathy of so many years may lead to a splendid awakening in this country, and that our past neglect of this most beautiful theory may be atoned for in the future by special devotion and appreciation.

SECTION D.

BIOLOGY.

OPENING ADDRESS BY PROF. A. MILNES MARSHALL, M.A., M.D., D.SC., F.R.S., PRESIDENT OF THE SECTION.

As my theme for this morning's address I have selected the development of animals. I have made this choice from no desire to extol one particular branch of biological study at the expense of others, nor through failure to appreciate or at least admire the work done and the results achieved in recent years by those who are attacking the great problems of life from other sides and with other weapons.

My choice is determined by the necessity that is laid upon me, through the wide range of sciences whose encouragement and advancement are the peculiar privilege of this Section, to keep within reasonable limits the direction and scope of my remarks; and is confirmed by the thought that, in addressing those specially interested in and conversant with biological study, your President acts wisely in selecting as the subject-matter of his discourse some branch with which his own studies and inclinations have brought him into close relation.

Embryology, referred to by the greatest of naturalists as "one of the most important subjects in the whole round of natural history," is still in its youth, but has of late years thriven so mightily that fear has been expressed lest it should absorb unduly the attention of zoologists, or even check the progress of science by diverting interest from other and equally important branches.

Nor is the reason of this phenomenal success hard to find. The actual study of the processes of development; the gradual building up of the embryo, and then of the young animal, within the egg; the fashioning of its various parts and organs; the devices for supplying it with food, and for ensuring that the respiratory and other interchanges are duly performed at all stages—all these are matters of absorbing interest. Add to these the extraordinary changes which may take place after leaving the egg, the conversion, for instance, of the aquatic gill-breathing tadpole—a true fish as regards all essential points of its anatomy—into a four-legged frog, devoid of tail, and breathing by lungs; or the history of the metamorphosis by which the sea-urchin is gradually built up within the body of its pelagic larva, or the butterfly derived from its grub. Add to these again the far wider interest aroused by comparing the life-histories of allied animals, or by tracing the mode of development of a complicated organ, *e.g.* the eye or the brain, in the various animal groups, from its simplest commencement, through gradually increasing grades of efficiency, up to its most perfect form as seen in the highest animals. Consider this, and it becomes easy to understand the fascination which embryology exercises over those who study it.

But all this is of trifling moment compared with the great generalization which tells us that the development of animals has a far higher meaning; that the several embryological stages and the order of their occurrence are no mere accidents, but are forced on an animal in accordance with a law, the determination of which ranks as one of the greatest achievements of biological science.

The doctrine of descent, or of evolution, teaches us that as individual animals arise, not spontaneously, but by direct descent from pre-existing animals, so also is it with species, with families, and with larger groups of animals, and so also has it been for all time; that as the animals of succeeding generations are related together, so also are those of successive geologic periods; that all animals, living or that have lived, are united together by blood relationship of varying nearness or remoteness; and that every animal now in existence has a pedigree stretching back, not merely for ten or a hundred generations, but through all geologic time since the dawn of life on this globe.

The study of development, in its turn, has revealed to us that each animal bears the mark of its ancestry, and is compelled to discover its parentage in its own development: that the phases through which an animal passes in its progress from the egg to the adult are no accidental freaks, no mere matters of developmental convenience, but represent more or less closely, in more or less modified manner, the successive ancestral stages through which the present condition has been acquired.

Evolution tells us that each animal has had a pedigree in the past. Embryology reveals to us this ancestry, because every animal in its own development repeats this history, climbs up its own genealogical tree.

Such is the recapitulation theory, hinted at by Agassiz, and suggested more directly in the writings of von Baer, but first clearly enunciated by Fritz Müller, and since elaborated by many, notably by Balfour and by Ernst Haeckel.

It is concerning this theory, which forms the basis of the science of embryology, and which alone justifies the extraordinary attention this science has received, that I venture to address you this morning.

A few illustrations from different groups of animals will best explain the practical bearings of the theory, and the aid which it affords to the zoologist of to-day; while these will also serve to illustrate certain of the difficulties which have arisen in the attempt to interpret individual development by the light of past history—difficulties which I propose to consider at greater length.

A very simple example of recapitulation is afforded by the eyes of the sole, plaice, turbot, and their allies. These "flat fish" have their bodies greatly compressed laterally; and the two surfaces, really the right and left sides of the animal, unlike, one being white, or nearly so, and the other coloured. The flat fish has two eyes, but these, in place of being situated, as in other fish, one on each side of the head, are both on the coloured side. The advantage to the fish is clear, for the natural position of rest of a flat fish is lying on the sea bottom, with the white surface downwards and the coloured one upwards. In such a position an eye situated on the white surface could be of no use to the fish, and might even become a source of danger, owing to its liability to injury from stones or other hard bodies on the sea bottom.

No one would maintain that flat fish were specially created as such. The totality of their organization shows clearly enough that they are true fish, akin to others in which the eyes are symmetrically placed one on each side of the head, in the position they normally hold among vertebrates. We must therefore suppose that flat fish are descended from other fish in which the eyes are normally situated.

The recapitulation theory supplies a ready test. On employing it, *i.e.* on studying the development of the flat fish, we obtain a conclusive answer. The young sole on leaving the egg is shaped just as an ordinary fish, and has the two eyes placed symmetrically on the two sides of the head. It is only after the young fish has reached some size, and has begun to approach the adult in shape, and to adopt its habit of resting on one side on the sea bottom, that the eye of the side on which it rests becomes shifted forwards, then rotated on to the top of the head, and finally twisted completely over to the opposite side.

The brain of a bird differs from that of other vertebrates in the position of the optic lobes, these being situated at the sides instead of on the dorsal surface. Development shows that this lateral position is a secondarily acquired one, for throughout all the earlier stages the optic lobes are, as in other vertebrates, on the dorsal surface, and only shift down to the sides shortly before the time of hatching.

Crabs differ markedly from their allies, the lobsters, in the small size and rudimentary condition of their abdomen or "tail." Development, however, affords abundant evidence of the descent of crabs from macrurous ancestors, for a young crab at what is termed the Megalopa stage has the abdomen as large as a lobster or prawn at the same stage.

Molluscs afford excellent illustrations of recapitulation. The typical gastropod has a large spirally-coiled shell; the limpet, however, has a large conical shell, which in the adult gives no sign of spiral twisting, although the structure of the animal shows clearly its affinity to forms wtth spiral shells. Development solves the riddle at once, telling us that in its early stages the limpet embryo has a spiral shell, which is lost on the formation, subsequently, of the conical shell of the adult.

Recapitulation is not confined to the higher groups of animals, and the Protozoa themselves yield most instructive examples. A very striking case is that of Orbitolites, one of the most complex of the porcellanous Foraminifera, in which each individual during its own growth and development passes through

the series of stages by which the cyclical or discoidal type of shell was derived from the simpler spiral form.

In *Orbitolites tenuissima*, as Dr. Carpenter has shown,[1] "the whole transition is actually presented during the successive stages of its growth. For it begins life as a Cornuspira, ... its shell forming a continuous spiral tube, with slight interruptions at the points at which its successive extensions commence; while its sarcodic body consists of a continuous coil with slight constrictions at intervals. The second stage consists in the opening out of its spire, and the division of its cavity at regular intervals by transverse septa, traversed by separate pores, exactly as in Peneroplis. The third stage is marked by the subdivision of the 'peneropline' chambers into chamberlets, as in the early forms of Orbiculina. And the fourth consists in the exchange of the spiral for the cyclical plan of growth, which is characteristic of Orbitolites; a circular disk of progressively increasing diameter being formed by the addition of successive annular zones around the entire periphery."

The shells both of Foraminifera and of Mollusca afford peculiarly instructive examples for the study of recapitulation. As growth of the shell is effected by the addition of new shelly matter to the part already existing, the older parts of the shell are retained, often unaltered, in the adult; and in favourable cases, as in *Orbitolites tenuissima*, all the stages of development can be determined by simple inspection of the adult shell.

It is important to remember that the recapitulation theory, if valid, must apply not merely in a general way to the development of the animal body, but must hold good with regard to the formation of each organ or system, and with regard to the later equally with the earlier phases of development.

Of individual organs, the brain of birds has been already cited. The formation of the vertebrate liver as a diverticulum from the alimentary canal, which is at first simple, but by the folding of its walls becomes greatly complicated, is another good example; as is also the development of the vomer in amphibians as a series of toothed plates, equivalent morphologically to the placoid scales of fishes, which are at first separate, but later on fuse together and lose the greater number of their teeth.

Concerning recapitulation in the later phases of development and in the adult animal, the mode of renewal of the nails or of the epidermis generally is a good example, each cell commencing its existence in an indifferent form in the deeper layers of the epidermis, and gradually acquiring the adult peculiarities as it approaches the surface, through removal of the cells lying above it.

The above examples, selected almost haphazard, will suffice to illustrate the theory of recapitulation.

The proof of the theory depends chiefly on its universal applicability to all animals, whether high or low in the zoological scale, and to all their parts and organs. It derives, also, strong support from the ready explanation which it gives of many otherwise unintelligible points.

Of these latter a familiar and most instructive instance is afforded by rudimentary organs, *i.e.* structures which, like the outer digits of the horse's leg, or the intrinsic muscles of the ear of a man, are present in the adult in an incompletely developed form, and in a condition in which they can be of no use to their possessors; or else structures which are present in the embryo, but disappear completely before the adult condition is attained—for example, the teeth of whalebone whales, or the branchial clefts of all higher vertebrates.

Natural selection explains the preservation of useful variations, but will not account for the formation and perpetuation of useless organs; and rudiments such as those mentioned above would be unintelligible but for recapitulation, which solves the problem at once, showing that these organs, though now useless, must have been of functional value to the ancestors of their present possessors, and that their appearance in the ontogeny of existing forms is due to repetition of ancestral characters. Such rudimentary organs are, as Darwin pointed out, of larger relative or even absolute size in the embryo than in the adult, because the embryo represents the stage in the pedigree in which they were functionally active.

Rudimentary organs are extremely common, especially among the higher groups of animals, and their presence and significance are now well understood. Man himself affords numerous and excellent examples, not merely in his bodily structure, but by his speech, dress, and customs. For the silent letter *b* in the word doubt, or the *w* of answer, or the buttons on his elastic side boots are as true examples of rudiments, unintelligible but for their past history, as are the ear muscles he possesses but cannot use, or the gill-clefts, which are functional in fishes and tadpoles, and are present, though useless, in the embryos of all higher vertebrates, which in their early stages the hare and the tortoise alike possess, and which are shared with them by cats and by kings.

Another consideration of the greatest importance arises from the study of the fossil remains of the animals that formerly inhabited the earth. It was the elder Agassiz who first directed attention to the remarkable agreement between the embryonic growth of animals and their palæontological history. He pointed out the resemblance between certain stages in the growth of young fish and their fossil representatives, and attempted to establish, with regard to fish, a correspondence between their palæontological sequence and the successive stages of embryonic development. He then extended his observations to other groups, and stated his conclusions in these words:—[1] "It may therefore be considered as a general fact, very likely to be more fully illustrated as investigations cover a wider ground, that the phases of development of all living animals correspond to the order of succession of their extinct representatives in past geological times."

This point of view is of the utmost importance. If the development of an animal is really a repetition of its ancestral history, then it is clear that the agreement or parallelism which Agassiz insists on between the embryological and palæontological records must hold good. Owing to the attitude which Agassiz subsequently adopted with regard to the theory of natural selection, there is some fear of his services in this respect failing to receive full recognition, and it must not be forgotten that the sentence I have quoted was written prior to the clear enunciation of the recapitulation theory by Fritz Müller.

The imperfection of the geological record has been often referred to and lamented. It is very true that our museums afford us but fragmentary pictures of life in past ages; that the earliest volumes of the history are lost, and that of others but a few torn pages remain to us; but the later records are in far more satisfactory condition. The actual number of specimens accumulated from the more recent formations is prodigious; facilities for consulting them are far greater than they were; the international brotherhood of science is now fully established, and the fault will be ours if the material and opportunities now forthcoming are not rightly and fully utilized.

By judicious selection of groups in which long series of specimens can be obtained, and in which the hard skeletal parts, which alone can be suitably preserved as fossils, afford reliable indications of zoological affinity, it is possible to test directly this correspondence between palæontological and embryological histories, while in some instances a single lucky specimen will afford us, on a particular point, all the evidence we require.

Great progress has already been made in this direction, and the results obtained are of the most encouraging description.

By Alexander Agassiz a detailed comparison was made between the fossil series and the developmental stages of recent forms in the case of the Echinoids, a group peculiarly well adapted for such an investigation. The two records agree remarkably in many respects, more especially in the independent evidence they give as to the origin of the asymmetrical forms from more regular ancestors. The gradually increasing complication in some of the historic series is found to be repeated very closely in the development of their existing representatives; and with regard to the whole group, Agassiz concludes that,[2] "comparing the embryonic development with the palæontological one, we find a remarkable similarity in both, and in a general way there seems to be a parallelism in the appearance of the fossil genera and the successive stages of the development of the Echini."

Neumayr has followed similar lines, and by him, as by other authorities on the group, there seems to be general agreement as to the parallelism between the embryological and palæontological records, not merely for Echini, but for other groups of Echinodermata as well.

The Tetrabranchiate Cephalopoda are an excellent group in which to study the problem, for though no opportunity has yet occurred for studying the embryology of the only surviving mem-

[1] "On an Abyssal Type of the Genus Orbitolites," Phil. Trans., 1883, Part ii. p. 553.

[1] "Essay on Classification," 1859, p. 115.

[2] "Palæontological and Embryological Development." An Address before the American Association for the Advancement of Science, 1880.

ber of the group, the pearly nautilus, yet owing to the fact that growth of the shell is effected by addition of shelly matter to the part already present, and to the additions being made in such manner that the older part of the shell persists unaltered, it is possible, from examination of a single shell—and in the case of fossils the shells are the only part of which we have exact knowledge—to determine all the phases of its growth; just as in the shell of Orbitolites all the stages of development are manifest on inspection of an adult specimen.

In such a shell as nautilus or ammonites the central chamber is the oldest or first formed one, to which the remaining chambers are added in succession. If, therefore, the development of the shell is a repetition of ancestral history, the central chamber should represent the palæontologically oldest form, and the remaining chambers in succession forms of more and more recent origin. Ammonite shells present, more especially in their sutures, and in the markings and sculpturing of their surface, characters that are easily recognized, and readily preserved in fossils; and the group, consequently, is a very suitable one for investigation from this standpoint.

Würtenberger's admirable and well-known researches[1] have shown that in the Ammonites such a correspondence between historic and embryonic development does really exist; that, for example, in Aspidoceras the shape and markings of the shells in young specimens differ greatly from those of adults, and that the characters of the young shells are those of palæontologically older forms.

Another striking illustration of the correspondence between the palæontological and developmental records is afforded by the antlers of deer, in which the gradually increasing complication of the antler in successive years agrees singularly closely with the progressive increase in size and complexity shown by the fossil series from the Miocene age to recent times.

Of cases where a single specimen has sufficed to prove the palæontological significance of a developmental character, Archæopteryx affords a typical example. In recent birds the metacarpals are firmly fused with one another, and with the distal series of carpals; but in development the metacarpals are at first, and for some time, distinct. In Archæopteryx this distinctness is retained in the adult, showing that what is now an embryonic character in recent birds was formerly an adult one.

Other examples might easily be quoted, but these will suffice to show that the relation between palæontology and embryology, first enunciated by Agassiz, and required by the recapitulation theory, does in reality exist. There is much yet to be done in this direction. A commencement, a most promising commencement, has been made, but as yet only a few groups have been seriously studied from this standpoint.

It is a great misfortune that palæontology is not more generally and more seriously studied by men versed in embryology, and that those who have so greatly advanced our knowledge of the early development of animals should so seldom have tested their conclusions as to the affinities of the groups they are concerned with by direct reference to the ancestors themselves, as known to us through their fossil remains.

I cannot but feel that, for instance, the determination of the affinities of fossil Mammalia, of which such an extraordinary number and variety of forms are now known to us, would be greatly facilitated by a thorough and exact knowledge of the development, and especially the later development, of the skeleton in their existing descendants, and I regard it as a reproach that such exact descriptions of the later stages of development should not exist, even in the case of our commonest domestic animals.

The pedigree of the horse has attracted great attention, and has been worked at most assiduously, and we are now, largely owing to the labours of American palæontologists, able to refer to a series of fossil forms commencing in the lowest Eocene beds, and extending upwards to the most recent deposits, which show a complete gradation from a more generalized mammalian type to the highly specialized condition characteristic of the horse and its allies, and which may reasonably be regarded as indicating the actual line of descent of the horse. In this particular case, more frequently cited than any other, the evidence is entirely palæontological. The actual development of the horse has yet to be studied, and it is greatly to be desired that it should be undertaken speedily. Klever's[2] recent work on the development of the teeth in the horse may be referred to as showing that important and unexpected evidence is to be obtained in this way.

A brilliant exception to the statement just made as to the want of exact knowledge of the later development of the more highly organized animals is afforded by the splendid labours of Prof. Kitchen Parker, whose recent death has deprived zoology of one of her most earnest and single-minded students, and zoologists, young and old alike, of a true and sincere friend. Prof. Parker's extraordinarily minute and painstaking investigations into the development of the vertebrate skull rank among the most remarkable of zootomical achievements, and afford a rich mine of carefully recorded facts, the full value and bearing of which we are hardly yet able to appreciate.

If further evidence as to the value and importance of the recapitulation theory were needed, it would suffice to refer to the influence which it has had on the classification of the animal kingdom. Ascidians and Cirripedes may be quoted as important groups, the true affinities of which were first revealed by embryology; and in the case of parasitic animals the structural modifications of the adult are often so great that but for the evidence yielded by development their zoological position could not be determined. It is now indeed generally recognized that in doubtful cases embryology affords the safest of all clues, and that the zoological position of such forms can hardly be regarded as definitely established unless their development, as well as their adult anatomy, is ascertained.

It is owing to this recapitulation theory that embryology has exercised so marked an influence on zoological speculation. Thus the formation in most, if not in all, animals of the nervous system and of the sense organs from the epidermal layer of the skin, acquired a new significance when it was recognized that this mode of development was to be regarded as a repetition of the primitive mode of formation of such organs; while the vertebral theory of the skull affords a good example of a view, once stoutly maintained, which received its death-blow through the failure of embryology to supply the evidence requisite in its behalf. The necessary limits of time and space forbid that I should attempt to refer to even the more important of the numerous recent discoveries in embryology, but mention may be very properly made here of Sedgwick's determination of the mode of development of the body cavity in Peripatus, a discovery which has thrown most welcome light on what was previously a great morphological puzzle.

We must now turn to another side of the question. Although it is undoubtedly true that development is to be regarded as a recapitulation of ancestral phases, and that the embryonic history of an animal presents to us a record of the race history, yet it is also an undoubted fact, recognized by all writers on embryology, that the record so obtained is neither a complete nor a straightforward one.

It is indeed a history, but a history of which entire chapters are lost, while in those that remain many pages are misplaced, and others are so blurred as to be illegible; words, sentences, or entire paragraphs are omitted, and worse still, alterations or spurious additions have been freely introduced by later hands, and at times so cunningly as to defy detection.

Very slight consideration will show that development cannot in all cases be strictly a recapitulation of ancestral stages. It is well known that closely allied animals may differ markedly in their mode of development. The common frog is at first a tadpole, breathing by gills, a stage which is entirely omitted by the West Indian Hylodes. A crayfish, a lobster, and a prawn are allied animals, yet they leave the egg in totally different forms. Some developmental stages, as the pupa condition of insects, or the stage in the development of a dogfish in which the œsophagus is imperforate, cannot possibly be ancestral stages. Or again, a chick embryo of say the fourth day is clearly not an animal capable of independent existence, and therefore cannot correctly represent any ancestral condition, an objection which applies to the developmental history of many, perhaps of most animals.

Haeckel long ago urged the necessity of distinguishing in actual development between those characters which are really historical and inherited, and those which are acquired or spurious additions to the record. The former he termed palingenetic or ancestral characters, the latter cenogenetic or acquired. The distinction is undoubtedly a true one, but an exceedingly difficult one to draw in practice. The causes which

[1] "Studien über die Stammesgeschichte der Ammoniten. Ein geologischer Beweis für die Darwin'sche Theorie" (Leipzig, 1880.)

[2] "Zur Kenntniss der Morphogenese des Equidengebisses," *Morphologisches Jahrbuch*, xv., 1889, p. 308.

prevent development from being a strict recapitulation of ancestral characters, the mode in which these came about, and the influence which they respectively exert, are matters which are greatly exercising embryologists, and the attempt to determine which has as yet met with only partial success.

The most potent and the most widely spread of these disturbing causes arise from the necessity of supplying the embryo with nutriment. This acts in two ways. If the amount of nutritive matter within the egg is small, then the young animal must hatch early, and in a condition in which it is able to obtain food for itself. In such cases there is of necessity a long period of larval life, during which natural selection may act so as to introduce modifications of the ancestral history, spurious additions to the text.

If, on the other hand, the egg contain within itself a considerable quantity of nutrient matter, then the period of hatching can be postponed until this nutrient matter has been used up. The consequence is that the embryo hatches at a much later stage of its development, and if the amount of food-material is sufficient, may even leave the egg in the form of the parent. In such cases the earlier developmental phases are often greatly condensed and abbreviated; and as the embryo does not lead a free existence, and has no need to exert itself to obtain food, it commonly happens that these stages are passed through in a very modified form, the embryo being, as in a four-day chick, in a condition in which it is clearly incapable of independent existence.

The nutrition of the embryo prior to hatching is most usually effected by granules of nutrient matter, known as food yolk, and embedded in the protoplasm of the egg itself; and it is on the relative abundance of these granules that the size of the egg chiefly depends.

Large size of eggs implies diminution of number of the eggs, and hence of the offspring; aud it can be well understood that while some species derive advantage in the struggle for existence by producing the maximum number of young, to others it is of greater importance that the young on hatching should be of considerable size and strength, and able to begin the world on their own account. In other words, some animals may gain by producing a large number of small eggs, others by producing a smaller number of eggs of larger size—*i.e.* provided with more food yolk.

The immediate effect of a large amount of food yolk is to mechanically retard the processes of development; the ultimate result is to greatly shorten the time occupied by development. This apparent paradox is readily explained. A small egg, such as that of Amphioxus, starts its development rapidly, and in about eighteen hours gives rise to a free-swimming larva, capable of independent existence, with a digestive cavity and nervous system already formed; while a large egg, like that of the hen, hampered by the great mass of food yolk by which it is distended, has, in the same time, made but very slight progress.

From this time, however, other considerations begin to tell. Amphioxus has been able to make this rapid start owing to its relative freedom from food yolk. This freedom now becomes a retarding influence, for the larva, containing within itself but a very scanty supply of nutriment, must devote much of its energies to hunting for, and to digesting its food, and hence its further development will proceed more slowly.

The chick embryo, on the other hand, has an abundant supply of food in the egg itself; it has no occasion to spend time searching for food, but can devote its whole energies to the further stages of its development. Hence, except in the earliest stages, the chick develops more rapidly than Amphioxus, and attains its adult form in a much shorter time.

The tendency of abundant food yolk to lead to shortening or abbreviation of the ancestral history, and even to the entire omission of important stages, is well known. The embryo of forms well provided with yolk takes short cuts in its development, jumps from branch to branch of its genealogical tree, instead of climbing steadily upwards.

Thus the little West Indian frog, Hylodes, produces eggs which contain a larger amount of food yolk than those of the common English frog. The young Hylodes is consequently enabled to pass through the tadpole stage before hatching—to attain the form of a frog before leaving the egg; and the tadpole stage is only imperfectly recapitulated, the formation of gills, for instance, being entirely omitted.

The influence of food yolk on the development of animals is closely analogous to that of capital in human undertakings. A new industry, for example that of pen-making, has often been started by a man working by hand and alone, making and selling his own wares; if he succeed in the struggle for existence, it soon becomes necessary for him to call in others to assist him, and to subdivide the work; hand labour is soon superseded by machines, involving further differentiation of labour; the earlier machines are replaced by more perfect and more costly ones; factories are built, agents engaged, and, in the end, a whole army of work-people employed. In later times a man commencing business with very limited means will start at the same level as the original founder, and will have to work his way upwards through much the same stages, *i.e.* will repeat the pedigree of the industry. The capitalist, on the other hand, is enabled, like Hylodes, to omit these earlier stages, and, after a brief period of incubation, to start business with large factories equipped with the most recent appliances, and with a complete staff of work-people, *i.e.* to spring into existence fully fledged.

There is no doubt that abundance of food-yolk is a direct and very frequent cause of the omission of ancestral stages from individual development; but it must not be viewed as a sole cause. It is quite impossible that any animal, except perhaps in the lowest zoological groups, should repeat all the ancestral stages in the history of the race; the limits of time available for individual development will not permit this. There is a tendency in all animals towards condensation of the ancestral history—towards striking a direct path from the egg to the adult.

This tendency is best marked in the higher, the more complicated members of a group; *i.e.* in those which have a longer and more tortuous pedigree; and though greatly strengthened by the presence of food yolk in the egg, is apparently not due to this in the first instance.

Thus the simpler forms of Orbitolites, as *O. tenuissima*, repeat in their development all the stages leading from a spiral to a cyclical shell; but in the more complicated species, as Dr. Carpenter has pointed out, there is a tendency towards precocious development of the adult characters, the earlier stages being hurried over in a modified form; while in the most complex examples, as in *O. complanata*, the earlier spiral stages may be entirely omitted, the shell acquiring almost from its earliest commencement the cyclical mode of growth. There is no question here of relative abundance of food yolk, but merely of early or precocious appearance of adult characters.

The question of the relations and influence of food yolk, involving as it does the larger or smaller size of the egg, is, however, merely a special side of the much wider question of the nutrition of the embryo, one of the most potent of the disturbing elements affecting development.

Speaking generally, we may say that large eggs are more often met with in the higher than the lower groups of animals. Birds and reptiles are cases in point, and, if mammals do not now produce large eggs, it is because a more direct and more efficient mode of nourishing the young by the placenta has been acquired by the higher forms, and has replaced the food yolk that was formerly present, and is now retained in quantity by Monotremes alone. Molluscs afford another good example, the eggs of Cephalopoda being of larger size than those of the less highly organized groups.

The large size of the eggs of Elasmobranchs, and perhaps that of Cephalopods also, may possibly be associated with the carnivorous habits of the animals; for it is of importance that forms which prey on other animals should hatch of considerable size and strength.

The influence of habitat must also be considered. It has long been noticed as a general rule that marine animals lay small eggs, while their fresh-water allies have eggs of much larger size. The eggs of the salmon or trout are much larger than those of the cod or herring; and the crayfish, though only a quarter the length of a lobster, lay eggs of actually larger size.

This larger size of the eggs of fresh-water forms appears to be dependent on the nature of the environment to which they are exposed. Considering the geological instability of the land as compared with the ocean, there can be no doubt that the fresh-water fauna is, speaking generally, derived from the marine fauna; and the great problem with regard to fresh-water life is to explain why it is that so many groups of animals which flourish abundantly in the sea should have failed to establish themselves in fresh water. Sponges and Cœlenterates abound in the sea, but their fresh-water representatives are extremely few in number; Echinoderms are exclusively marine: there are

no fresh-water Cephalopods, and no Ascidians; and of the smaller groups of Worms, Molluscs, and Crustaceans, there are many that do not occur in fresh-water.

Direct experiment has shown that in many cases this distribution is not due to inability of the adult animals to live in fresh water; and the real explanation appears to be that the early larval stages are unable to establish themselves under such conditions. This interesting suggestion, which has been worked out in detail by Prof. Sollas,[1] undoubtedly affords an important clue. To establish itself permanently in fresh water an animal must either be fixed, or else be strong enough to withstand and make headway against the currents of the streams or rivers it inhabits, for otherwise it will in the long run be swept out to sea, and this consideration applies to larval forms equally with adults.

The majority of marine invertebrates leave the egg as minute ciliated larvæ; and such larvæ are quite incapable of holding their own in currents of any strength. Hence, it is only forms which have got rid of the free-swimming ciliated larval stage, and which leave the egg of considerable size and strength, that can establish themselves as fresh-water animals. This is effected most readily by the acquisition of food yolk—hence the large size of the eggs of fresh-water animals—and is often supplemented, as Sollas has shown, by special protective devices of a most interesting nature. For this reason fresh-water forms are not so well adapted as their marine allies for the study of ancestral history as revealed in larval or embryonic development.

Before leaving the question of food yolk, reference must be made to the proposal of the brothers Sarasin, to regard the yolk cells as forming a distinct embryonic layer, the lecithoblast,[2] distinct from the blastoderm. I do not desire to speak dogmatically on a point the full bearings of which are not yet apparent, but I venture to think that this suggestion will not commend itself to embryologists. The distinction between the yolk granules and the cells in which they are embedded is a real and fundamental one; but I see no reason for regarding the yolk cells as other than originally functional endoderm cells in which yolk granules have accumulated to such an extent that they have in extreme cases become devoted solely to the storing of food for the embryo.[3]

Of all the causes tending to modify development, tending to obscure or falsify the ancestral record, food yolk is the most frequent and the most important; its position in the egg determines the mode of segmentation; and its relative abundance affects profoundly the entire embryonic history, and decides at what particular stage, and of what size and form, the embryo shall hatch.

The loss of food yolk is another disturbing element, the full influence of which is as yet imperfectly understood, but the possibility of which must be always kept in mind. It is best known in the case of mammals, where it has led to apparent, though very deceptive, simplification of development; and it will probably not be until the embryology of the large-yolked Monotremes is at length described, that we shall fully understand the formation of the germinal layers in the higher placental mammals.

Amongst invertebrates we know but little as yet concerning the effects of loss of food yolk. It has been suggested that the extraordinary nature of the segmentation of the egg of *Peripatus capensis*, made known to us through Mr. Sedgwick's admirable researches, may be due to loss of food yolk; a suggestion which receives support from the long duration of uterine development in this case.

Our knowledge is very imperfect as to the ease with which food yolk may be acquired or lost; but until our information is more precise on this point, it seems unwise to lay much stress on suggested pedigrees which involve great and frequent alternations in the amount of food yolk present.

Of causes other than food yolk, or only indirectly connected with it, which tend to falsify the ancestral history, many are now known, but time will only permit me to notice the more important. These are distortion, whether in time or space; sudden or violent metamorphosis; a series of modifications, due chiefly to mechanical causes, and which may be spoken of as developmental conveniences; the important question of variability in development; and finally the great problem of degeneration.

Concerning distortions in time, all embryologists have noticed the tendency to anticipation or precocious development of characters which really belong to a later stage in the pedigree. The early attainment of the cyclical form in the shell of *Orbitolites complanata* is a case in point; and Würtemberger has specially noticed this tendency in Ammonites. Many early larvæ show it markedly, the explanation in this case being that it is essential for them to hatch in a condition capable of independent existence, *i.e.* capable, at any rate, of obtaining and digesting their own food.

Anachronisms, or actual reversal of the historical order of development of organs or parts, occur frequently. Thus the joint surfaces of bones acquire their characteristic curvatures before movement of one part on another is effected, and before even the joint cavities are formed.

Another good example is afforded by the development of the mesenterial filaments in Alcyonarians. Wilson has shown in the case of Renilla that in the development of an embryo from the egg the six endodermal filaments appear first, and the two long ectodermal filaments at a later period; but that in the formation of a bud this order of development is reversed, the ectodermal filaments being the first formed. He suggests, in explanation, that, as the endodermal filaments are the digestive organs, it is of primary importance to the free embryo that they should be formed quickly. The long ectodermal filaments are chiefly concerned with maintaining currents of water through the colony; in bud-development they appear before the endodermal filaments, because they enable the bud during its early stages to draw nutrient matter from the body fluid of the parent; while the endodermal filaments cannot come into use until the bud has acquired both mouth and tentacles.

The completion of the ventricular septum in the heart of higher vertebrates before the auricular septum is a well-known anachronism, and every embryologist could readily furnish many other cases.

A curious instance is afforded by the development of the teeth in mammals, if recent suggestions as to the origin of the milk dentition are confirmed, and the milk dentition prove to be a more recent acquisition than the permanent one.[1]

But the most important cases in reference to distortion in time concern the reproductive organs. If development were a strict and correct recapitulation of ancestral history, then each stage would possess reproductive organs in a mature condition. This is not the case, and it is clearly of the greatest importance that it should not be. It is true that the first commencement of the reproductive organs may occur at a very early larval stage, or even that the very first step in development may be a division of the egg into somatic and reproductive cells; and it is possible that, as maintained by Weismann, this latter condition is a primitive one. Still, even in these cases the reproductive organs merely commence their development at these early stages, and do not become functional until the animal is adult.

Exceptionally in certain animals, and as a normal occurrence in others, precocious maturation of the reproductive organs takes place, and a larval form becomes capable of sexual reproduction. This may lead to arrest of development, either at a late larval period, as in the Axolotl, or at successively earlier and earlier stages, as in the gonophores of the Hydromedusæ, until finally the extreme condition seen in Hydra is produced.

We do not know the causes that determine the period, whether late or early, at which the reproductive organs ripen, but the question is one of great interest and importance and deserves careful attention. The suggestion has been made that entire groups of animals, such as the Mesozoa, are merely larvæ, arrested through such precocious acquiring of reproductive power, and it is conceivable that this may be the case. Mesozoa are a puzzling group in which the life-history, though known with tolerable completeness, has as yet given us no reliable clue concerning their affinities to other animals—a tantalizing distinction that is shared with them by Rotifers and Polyzoa.

Distortion of a curious kind is seen in cases of abrupt metamorphosis, where, as in the case of many Echinoderms, of Phoronis, and of the metabolic insects, the larva and the adult differ greatly in form, habits, mode of life, and very usually in

[1] "On the Origin of Freshwater Faunas," Scientific Transactions of the Royal Dublin Society, vol. iii. Ser. 11, 1886.

[2] "Ergebnisse naturwissenschaftlicher Forschungen auf Ceylon," Bd. ii. Heft. iii., 1889.

[3] *Cf.* E. B. Wilson, "The Development of Renilla," Phil. Trans., 1883, p. 755.

[1] *Cf.* Thomas Oldfield, "On the Homologies and Succession of the Teeth in the Dasyuridæ, with an attempt to trace the history of the evolution of the Mammalian teeth in general," Phil. Trans., 1887.

the nature of their food and the mode of obtaining it; and the transition from one stage to the other is not a gradual but an abrupt one, at any rate so far as external characters are concerned.

Sudden changes of this kind, as from the free-swimming Pluteus to the creeping Echinus, or from the sluggish leaf-eating caterpillar to the dainty butterfly, cannot possibly be recapitulatory, for even if small jumps are permissible in Nature, there is no room for bounds forward of this magnitude. Cases of abrupt metamorphosis may always be viewed as due to secondary modifications, and rarely, if ever, have any significance beyond the particular group of animals concerned. For example, a Pluteus larva may be recognized as belonging to the group of Echinoidea before the adult urchin has commenced to be formed within it, and the Lepidopteran caterpillar is already an unmistakable insect. Hence, for the explanation of the metamorphoses in these cases it is useless to look outside the groups of Echinoidea and Insecta respectively.

Abrupt metamorphosis is always associated with great change in external form and appearance, and in mode of life, and very usually in mode of nutrition. A gradual transition in such cases is inadmissible, because in the intermediate stages the animal would be adapted to neither the larval nor the adult condition; a gradual conversion of the biting mouth parts of the caterpillar to the sucking proboscis of a moth would inevitably lead to starvation. The difficulty is evaded by retaining the external form and habits of one particular stage for an unduly long period, so that the relations of the animal to the surrounding environment remain unchanged, while internally preparations for the later stages are in progress. Cinderella and the princess are equally possible entities, each being well adapted to her environment. The exigencies of the situation do not permit, however, of a gradual change from one to the other: the transformation, at least as regards external appearance, must be abrupt.

Kleinenberg has recently directed attention to cases in which the larval and adult organs develop independently; the larval nervous system, for instance, aborting completely and forming no part of that of the adult. I am not sure that I fully understand Kleinenberg's argument, but it seems very possible that such cases, which are probably far more numerous than is yet admitted, may be due to what may be termed the telescoping of ancestral stages one within another, which takes place in actual development, and may accordingly be grouped under the head of developmental convenience. Undue prolongation of an early ancestral stage, as in cases of abrupt metamorphosis, must involve modification, especially in the muscular and nervous systems; in such cases a telescoping of ancestral stages takes place, as we have seen, the adult being developed within the larva. Such telescoping must distort the recapitulatory history, and as the shape of the larva and adult may differ widely, an independent origin of organs, especially the muscular and nervous systems, may be acquired secondarily.

The stage in the development of Squilla, in which the three posterior maxillipedes disappear completely, to reappear at a later stage in a totally different form, is not to be interpreted as meaning that the adult maxillipedes are entirely new structures unconnected historically with those of the larva. Neither is the annual shedding of the antlers of deer to be regarded as the repetition of an ancestral hornless condition intercalated historically between successive stages provided with antlers. In both cases the explanation is afforded by convenience, whether of the embryo or adult.

Many embryological modifications or distortions may be attributed to mechanical causes, and may fairly be considered under the head of developmental conveniences.

The amnion of higher vertebrates is a case in point, and is probably rightly explained as due in the first instance to sinking or depression of the embryo into the yolk, in order to avoid distortion through pressure against a hard unyielding eggshell. A similar device is employed, presumably for the same reason, in the early development of many insect embryos; and the depression of the Tænia head within the cyst is a phenomenon of very similar nature.

Restriction of the space within which development occurs often causes displacement or distortion of organs, whose growth, restricted in its normal direction, takes place along the lines of least resistance. The telescoping of the limbs and other organs within the body of an insect larva is a simple case of such distortion; and a more complicated example, closely comparable in many ways to the invagination of the Tænia head, is afforded by the remarkable inversion of the germinal layers in rodents, first described by Bischoff in the guinea-pig, and long believed to be peculiar to that animal, but subsequently and simultaneously discovered by three independent observers—Kupffer, Selenka, and Fraser—to occur in varying degrees in rats, mice, and in other rodents.

One of the most recent attempts to explain developmental peculiarities as due to mechanical causes is Mr. Dendy's suggestion with regard to the pseudogastrula stage in the development of the calcareous sponges. It is well known that, while the larva is in the amphiblastula stage, and still embedded in the tissues of the parent, the granular cells become invaginated within the ciliated cells, giving rise to the pseudogastrula stage. At a slightly later stage, when the larva becomes free, the invaginated granular cells become again everted, and the larva spherical in shape; while still later invagination occurs once more, the ciliated cells being this time invaginated within the granular cells. The significance of the pseudogastrula stage has hitherto been undetermined, but Mr. Dendy points out that the larva always occupies a definite position with reference to the parental tissues; that the ciliated half of the larva is covered by a soft and yielding wall, while the opposite half, composed of the granular cells, is covered by a layer stiffened with rigid spicules; and his observations on the growth of the larva lead him to think that the pseudogastrula stage is brought about mechanically by flattening of the granular cells through pressure against this rigid wall of spicules.

Embryology supplies us with many unsolved problems, and it is not to be wondered at that this should be the case. Some of these may fairly be spoken of as mere curiosities of development, while others are clearly of greater moment. I do not propose to catalogue these, but will merely mention two or three which I happen to have recently run my head against, and remember vividly.

The solid condition of the œsophagus, in Elasmobranch embryos, first noticed by Balfour, is a very curious point. The œsophagus has at first a well-developed lumen, like the rest of the alimentary canal; but at an early period, stage K of Balfour's nomenclature, the part of the œsophagus overlying the heart, and immediately behind the branchial region, becomes solid, and remains solid for a long time, the exact date of reappearance of the lumen not being yet ascertained.

Mr. Bles and myself have recently noticed that a similar solidification of the œsophagus occurs in tadpoles of the common frog. In young free-swimming tadpoles the œsophagus is perforate, but in tadpoles of about 7½ mm. length it becomes solid and remains so until a length of about 10½ mm. has been attained. The solidification occurs at a stage closely corresponding with that in which it first appears in the dogfish, and a curious point about it is that in the frog the œsophagus becomes solid just before the mouth-opening is formed, and remains solid for some little time after this important event.

This closing of the œsophagus clearly cannot be recapitulation, but the fact that it occurs at corresponding periods in the frog and dogfish suggests that it may possibly, as Balfour hinted, "turn out to have some unsuspected morphological bearing."

Another developmental curiosity is the duplication of the gill-slits by growth downwards of tongues from their dorsal margins; a duplication which is described as occurring in Amphioxus and in Balanoglossus, but in no other animal; and the occurrence of which, in apparently closely similar fashion, is one of the strongest arguments in favour of a real affinity between these two forms. It is hardly possible that such a modification should have been acquired independently twice over.

A much more litigious question is the significance of the neurenteric canal of vertebrates, that curious tubular communication between the central canal of the nervous system and the hinder end of the alimentary canal that is conspicuously present in the embryos of lower vertebrates, and retained in a more or less disguised condition in the higher groups as well.

The neurenteric canal was discovered by that famous embryologist Kowalevsky in Ascidians and in Amphioxus. He drew special attention to the occurrence of a stage in both Ascidians and in Amphioxus in which the larva is free-swimming and in which the sole communication between the alimentary cavity and the exterior is through the neurenteric canal and the central canal of the nervous system; and suggested [1] that animals may

[1] "Weitere Studien über die Entwickelungs-Geschichte des *Amphioxus lanceolatus*" (*Archiv für mikroskopische Anatomie*, Bd. xiii., 1877, p. 201).

have existed or may still exist in which the nerve tube fulfilled a non-nervous function, and possibly acted as part of the alimentary canal, a suggestion that has recently been revived in a somewhat extravagant form.

A passage of food particles into the alimentary cavity through the neural tube has not yet been seen, and probably does not occur, as the larva still possesses sufficient food yolk to carry it on in its development. It is therefore permissible to hold that the neurenteric canal may be a mere embryological device, and devoid of any deep morphological significance.

The question of variation in development is one of very great importance, and has perhaps not yet received the attention it deserves. We are in some danger of assuming tacitly that the mode of development of allied animals will necessarily agree in all-important respects or even in details, and that if the development of one member of a group be known, that of the others may be assumed to be similar. The more recent progress of embryology is showing us that such inferences are not safe, and that in allied genera or species, or even in different individuals of the same species, variations of development may occur affecting important organs and at almost any stage in their formation.

Great individual variations in the earliest processes of development, *i.e.* the segmentation of the egg, have been described by different writers.

In Renilla, Wilson found an extraordinary range of variation in the segmentation of eggs from which apparently identical embryos were produced. In some cases the egg divided into two in the normal manner; in other cases it divided at once into eight, sixteen, or thirty-two segments, which in different specimens were approximately equal or markedly unequal in size. Sometimes a preliminary change of form occurred without any further result, the egg returning to its spherical shape, and pausing for a time before recommencing the attempt to segment. Segmentation sometimes commenced at one pole, as in telolecithal eggs, with the formation of four or five small segments, the rest of the egg breaking up later, either simultaneously or progressively, into segments about equal in size to those first formed; while lastly, in some instances segmentation was very irregular, following no apparent law.

It is noteworthy that the variability in the case of Renilla is apparently confined to the earliest stages, for whatever the mode of segmentation, the embryos in their later stages were indistinguishable from one another.

Similar modifications in the segmentation of the egg have been described in the oyster by Brooks, in Anodon and other Mollusca, in Hydra, and in Lumbricus, in which last Wilson has recently shown that marked differences occur in the eggs even of the same individual animal. In the different species of Peripatus there appear also to be considerable variations in the details of segmentation.

In the early embryonic stages after the completion of segmentation very considerable variation may occur in allied species or genera. Among Cœlenterates, for instance, the mode of formation of the hypoblast presents most perplexing modifications: it may arise as a true gastrula invagination; as cells budded off from one pole of the blastula into its cavity; as cells budded off from various parts of the wall of the blastula; by delamination or actual division of each cell of the blastula wall; or it may be present from the start as a solid mass of cells inclosed by the epiblast cells. It is in connection with these variations that controversy has arisen as to the primitive mode of development of the gastrula, a point to which I shall return later on.

Among the higher Metazoa or Cœlomata the extraordinary modifications in the position and in every conceivable detail of formation of the mesoblast in different and often in closely allied forms have given rise to ardent discussion, and have led to the proposal of theory after theory, each rejected in turn as only affording a partial explanation, and now culminating in Kleinenberg's protest against the use of the term mesoblast at all, at any rate in a sense implying any possibility of comparison with the primary layers, epiblast and hypoblast, of Cœlenterata.

This is not the place to attempt to decide so difficult and technical a point, even were I capable of so doing, but we may well take warning from this extraordinary diversity of development, the full extent of which I believe we as yet realize most imperfectly, that in our attempts to reconstruct ancestral history from ontogenetic development we have taken in hand no light task. To reconstruct Latin from modern European languages would in comparison be but child's play.

Of the readiness with which special developmental characters are acquired by allied animals the brothers Sarasin [1] have given us evidence in the extraordinary modifications presented by the embryonic and larval respiratory organs of Amphibians.

Confining ourselves to those forms which do not lay their eggs in water, and in which consequently development takes place within the egg, we find that Ichthyophis and Salamandra have three pairs of specially modified external gills. Nototrema has two pairs; Alytes and Typhlonectes have only a single pair, which in the latter genus take the form of enormous leaf-like outgrowths from the sides of the neck. In Hylodes and Pipa there are no gills, the tail acting as the larval respiratory organ; and in *Rana opisthodon*, according to Boulenger, larval respiration is effected by nine pairs of folds of the skin of the ventral surface of the body.

Most of these extraordinarily diversified organs are clearly secondarily acquired structures; it is possible that they all are, and that external gills, as was suggested by Balfour for Elasmobranchs, are to be regarded as embryonal respiratory organs acquired by the larvæ, and of no ancestral value. The point, however, cannot be considered settled, for on this view the external gills of Elasmobranchs and Amphibians would be independently acquired and not homologous structures, a view contradicted by the close agreement in their relations in the two groups, as well as by the absence of any real break between external and internal gills in Amphibians.

It is well known that the frog and the newt differ greatly in important points of their development. The two-layered condition of the epiblast in the frog is a marked point of difference, which involves further changes in the mode of formation of the nervous system and sense organs. The kidneys and their ducts differ considerably in their development in the two forms, as do also the blood-vessels.

Concerning the early development of the blood-vessels, there are considerable differences even between allied species of frogs. In *Rana esculenta* Maurer finds that there is at first in each branchial arch a single vessel or aortic arch, running directly from the heart to the aorta: from the cardiac end of this aortic arch a vessel grows out into the gill as the afferent branchial vessel, the original aortic arch losing its connection with the heart, and becoming the efferent branchial vessel. Afferent and efferent branchial vessels become connected by capillaries in the gill, and the course of the circulation, so long as gill-breathing is maintained, is from the heart through the truncus arteriosus to the afferent branchial vessel, then through the gill capillaries to the efferent branchial vessel, and then on to the aorta. When the pulmonary circulation is thoroughly established, the branchial circulation is cut off by the efferent vessel reacquiring its connection with the heart, when the blood naturally takes the direct passage along it to the aorta, and so escapes the gill capillaries.

In *Rana temporaria* the mode of development is very different: the afferent and efferent vessels arise in each arch independently and almost simultaneously: the afferent vessel soon acquires connection with the heart; but, unlike *R. esculenta*, the efferent vessel has no connection with the heart until the gills are about to atrophy.

In other words, the continuous aortic arch, from heart to aorta, is present in *R. esculenta* prior to the development of the gills: it becomes interrupted while the gills are in functional use, but is re-established when these begin to atrophy. In *R. temporaria*, on the other hand, there is no continuous aortic arch until the gills begin to atrophy.

The difference is an important one, for it is a matter of considerable morphological interest to determine whether the continuous aortic arch is primitive for vertebrates: *i.e.* whether it existed prior to the development of gills. This point could be practically settled if we could decide which of the two frogs, *R. esculenta* and *R. temporaria*, has most correctly preserved its ancestral history in this respect.

About this there can be little doubt. The development of the vessels in the newts, a less modified group than the frogs, agrees with that of *R. esculenta*, and interesting confirmation is afforded by a single aberrant specimen of *R. temporaria*, in which Mr. Bles and myself found the vessels developing after the type of *R.*

[1] "Ergebnisse naturwissenschaftlicher Forschungen auf Ceylon," vol. ii. chap. i. pp. 24–38.

esculenta, *i.e.* in which a complete aortic arch was present before the gills were formed.

We are therefore justified in concluding that as regards the development of the branchial blood-vessels, *R. esculenta* has retained a primitive ancestral character which is lost in *R. temporaria*, and it is interesting to note that were our knowledge of the development of amphibians confined to the common frog, the most likely form to be studied, we should, in all probability, have been led to wrong conclusions concerning the ancestral condition of the blood-vessels in a point of considerable importance.

A matter which at present is attracting much attention is the question of degeneration.

Natural selection, though consistent with and capable of leading to steady upward progress and improvement, by no means involves such progress as a necessary consequence. All it says is that those animals will, in each generation, have the best chance of survival which are most in harmony with their environment, and such animals will not necessarily be those which are ideally the best or most perfect.

If you go into a shop to purchase an umbrella, the one you select is by no means necessarily that which most nearly approaches ideal perfection, but the one which best hits off the mean between your idea of what an umbrella should be and the amount of money you are prepared to give for it: the one, in fact, that is on the whole best suited to the circumstances of the case, or the environment for the time being. It might well happen that you had a violent antipathy to a crooked handle, or else were determined to have a catch of a particular kind to secure the ribs, and this might lead to the selection, *i.e.* the survival, of an article that in other and even in more important respects was manifestly inferior to the average.

So it is also with animals: the survival of a form that is ideally inferior is very possible. To animals living in profound darkness the possession of eyes is of no advantage, and forms devoid of eyes would not merely lose nothing thereby, but would actually gain, inasmuch as they would escape the dangers that might arise from injury to a delicate and complicated organ. In extreme cases, as in animals leading a parasitic existence, the conditions of life may be such as to render locomotor, digestive, sensory, and other organs entirely useless; and in such cases those forms will be best in harmony with their surroundings which avoid the waste of energy resulting from the formation and maintenance of these organs.

Animals which have in this way fallen from the high estate of their forefathers, which have lost organs or systems which their progenitors possessed, are commonly called degenerate. The principle of degeneration, recognized by Darwin as a possible, and, under certain conditions, a necessary consequence of his theory of natural selection, has been since advocated strongly by Dohrn, and later by Lankester in an evening discourse delivered before the Association at the Sheffield meeting in 1879. Both Dohrn and Lankester suggested that degeneration occurred much more widely than was generally recognized.

In animals which are parasitic when adult, but free-swimming in their early stages, as in the case of the Rhizocephala, whose life-history was so admirably worked out by Fritz Müller, degeneration is clear enough: so also is it in the case of the solitary Ascidians, in which the larva is a free-swimming animal with a notochord, an elongated tubular nervous system, and sense organs, while the adult is fixed, devoid of the swimming tail, with no notochord, and with a greatly reduced nervous system and aborted sense organs.

In such cases the animal, when adult, is, as regards the totality of its organization, at a distinctly lower morphological level, is less highly differentiated than it is when young, and during individual development there is actual retrograde development of important systems and organs.

About such cases there is no doubt; but we are asked to extend the idea of degeneration much more widely. It is urged that we ought not to demand direct embryological evidence before accepting a group as degenerate. We are reminded of the tendency to abbreviation or to complete omission of ancestral stages of which we have quoted examples above; and it is suggested that if such larval stages were omitted in all the members of a group we should have no direct evidence of degeneration in a group that might really be in an extremely degenerate condition.

Supposing, for instance, the free larval stages of the solitary Ascidians were suppressed, say through the acquisition of food yolk, then it is urged that the degenerate condition of the group might easily escape detection. The supposition is by no means extravagant; food yolk varies greatly in amount in allied animals, and cases like Hylodes, or amongst Ascidians Pyrosoma, show how readily a mere increase in the amount of food yolk in the egg may lead to the omission of important ancestral stages.

The question then arises whether it is not possible, or even probable, that animals which now show no indication of degeneration in their development are in reality highly degenerate, and whether it is not legitimate to suppose such degeneration to have occurred in the case of animals whose affinities are obscure or difficult to determine.

It is more especially with regard to the lower vertebrates that this argument has been employed; and at the present day, zoologists of authority, relying on it, do not hesitate to speak of such forms as Amphioxus and the Cyclostomes as degenerate animals, as wolves in sheep's clothing, animals whose simplicity is acquired and deceptive rather than real and ancestral.

I cannot but think that cases such as these should be regarded with some jealousy; there is at present a tendency to invoke degeneration rather freely as a talisman to extricate us from morphological difficulties; and an inclination to accept such suggestions, at any rate provisionally, without requiring satisfactory evidence in their support.

Degeneration of which there is direct embryological evidence stands on a very different footing from suspected degeneration, for which no direct evidence is forthcoming; and in the latter case the burden of proof undoubtedly rests with those who assume its existence.

The alleged instances among the lower vertebrates must be regarded particularly closely, because in their case the suggestion of degeneration is admittedly put forward as a means of escape from difficulties arising through theoretical views concerning the relation between vertebrates and invertebrates.

Amphioxus itself, so far as I can see, shows in its development no sign of degeneration, except possibly with regard to the anterior gut diverticula, whose ultimate fate is not altogether clear. With regard to the earlier stages of development, concerning which, thanks to the patient investigations of Kowalevsky and Hatschek, our knowledge is precise, there is no animal known to us in which the sequence of events is simpler or more straightforward. Its various organs and systems are formed in what is recognized as a primitive manner; and the development of each is a steady upward progress towards the adult condition. Food yolk, the great cause of distortion in development, is almost absent, and there is not the slightest indication of the former possession of a larger quantity. Concerning the later stages our knowledge is incomplete, but so much as has been ascertained gives no support to the suggestion of general degeneration.

Our knowledge of the conditions leading to degeneration is undoubtedly incomplete, but it must be noticed that the conditions usually associated with degeneration do not occur. Amphioxus is not parasitic, is not attached when adult, and shows no evidence of having formerly possessed food yolk in quantity sufficient to have led to the omission of important ancestral stages. Its small size as compared with other vertebrates is one of the very few points that can be referred to as possibly indicating degeneration, and will be considered more fully at a later point in my address.

A consideration of much less importance, but deserving of mention, is that in its mode of life Amphioxus not merely differs as already noticed from those groups of animals which we know to be degenerate, but agrees with some, at any rate, of those which there is reason to regard as primitive or persistent types. Amphioxus, like Balanoglossus, Lingula, Dentalium, and Limulus, is marine, and occurs in shallow water, usually with a sandy bottom, and, like the three smaller of these genera, it lives habitually buried almost completely in the sand, into which it burrows with great rapidity.

I do not wish to speak dogmatically. I merely wish to protest against a too ready assumption of degeneration; and to repeat that, so far as I can see, Amphioxus has not yet, either in its development, in its structure, or in its habits, been shown to present characters that suggest, still less that prove, the occurrence in it of general or extensive degeneration.

In a sense, all the higher animals are degenerate; that is,

they can be shown to possess certain organs in a less highly developed condition than their ancestors, or even in a rudimentary state.

Thus a crab as compared with a lobster is degenerate in the matter of its tail, a horse as compared with Hipparion in regard to its outer toes; but it is neither customary nor advisable to speak of a crab as a degenerate animal compared to a lobster; to do so would be misleading. An animal should only be spoken of as degenerate when the retrograde development is well marked, and has affected not one or two organs only, but the totality of its organization.

It is impossible to draw a sharp line in such cases, and to limit precisely the use of the term degeneration. It must be borne in mind that no animal is at the top of the tree in all respects. Man himself is primitive as regards the number of his toes, and degenerate in respect to his ear muscles; and between two animals even of the same group it may be impossible to decide which of the two is to be called the higher and which the lower form.

Thus, to compare an oyster with a mussel. The oyster is more primitive than the mussel as regards the position of the ventricle of the heart and its relations to the alimentary canal; but is more modified in having but a single adductor muscle; and almost certainly degenerate in being devoid of a foot.

Care must also be taken to avoid speaking of an animal as degenerate in regard to a particular organ merely because that organ is less fully developed than in allied animals. An organ is not degenerate unless its present possessor has it in a less perfect condition than its ancestors had.

A man is not degenerate in the matter of the length of his neck as compared with a giraffe, nor as compared with an elephant in respect of the size of his front teeth, for neither elephant nor giraffe enters into the pedigree of man. A man is, however, degenerate, whoever his ancestors may have been, in regard to his ear muscles; for he possesses these in a rudimentary and functionless condition, which can only be explained by descent from some better equipped progenitor.

Closely connected with the question of degeneration is that of the size of animals, and its bearing on their structure and development; a problem noticed by many writers, but which has perhaps not yet received the attention it merits.

If we are right in interpreting the eggs of Metazoa as representing the unicellular or protozoan stage in their ancestry, then the small size of the egg may be viewed as recapitulatory.

But the gradual increase in size of the embryo, and its growth up to the adult condition, can only be regarded as representing in a most general way, if at all, the actual or even the relative sizes of the intermediate ancestral stages of the pedigree.

It is quite true that animals belonging to the lower groups are, as a general rule, of smaller size than those of higher grade; and also that the giants are met with among the highest members of each division. Cephalopoda are the highest molluscs, and the largest cephalopods greatly exceed in size any other members of the group; decapods are at once the highest and the largest crustaceans; and whales, the hugest animals that exist, or, so far as we know, that ever have existed, belong to the highest group of all, the mammalia. It would be easy to quote exceptions, but the general rule obtains admittedly.

However, although there may be, and probably is, a general parallelism between the increase in size from the egg to the adult, and the historical increase in size during the passage from lower to higher forms; yet no one could maintain that the sizes of embryos represent at all correctly those of the ancestors; that, for instance, the earliest birds were animals the size of a chick embryo at a time when avian characters first declared themselves, or that the ancestral series in all cases presented a steady progression in respect of actual magnitude.

In the lower animals, *e.g.*, in Orbitolites, the actual size of the several ancestral stages is probably correctly recapitulated during the growth of the adult; and it is very possible that it is so also in such forms as the solitary sponges. In higher animals, except in the early stages of those forms which are practically devoid of food yolk, and which hatch as pelagic larvæ, this certainly does not obtain.

This is clear enough, but is worth pointing out, for if, as most certainly is the case, the embryos of animals are actually smaller than the ancestral forms they represent, it is possible that the smallness of the embryo may have had some influence on its organization, and be responsible for some of the modifications in the ancestral history; and more especially for the disappearance of ancestral organs in free-swimming larvæ.

In adult animals the relation between size and structure has been very clearly pointed out by Herbert Spencer. Increased size involves by itself increased complexity of structure; the determining consideration being that while the surface area of the body increases as the squares of the linear dimensions, the mass of the body increases as their cubes.

If, for example, we imagine two animals of similar shape and proportions, but of different size; for the sake of simplicity, we may suppose them to be spherical, and that the diameter of one is twice that of the other; then the larger one will have four times the extent of surface of the smaller, but eight times its mass or bulk: and it is quite possible that while the extent of surface, or skin, in the smaller animal might suffice for the necessary respiratory and excretory interchanges, it would be altogether insufficient in the larger animal, in which increased extent of surface must be provided by foldings of the skin, as in the form of gills.

To take an actual instance; Limapontia is a minute nudibranchiate, or sea-slug, about the sixth of an inch in length; it has a smooth body, totally devoid of respiratory processes, while forms allied to it, but of larger size, have their extent of surface increased by branching processes, which often take the form of specialized gills.

This is a peculiarly instructive case, because Limapontia in its early developmental stages possesses a large spirally-coiled shell, and shows other evidence of descent from forms with specialized breathing organs. We are certainly right in associating the absence of respiratory organs in the adult with the small size of the animal; and comparison with allied forms suggests very strongly that there has been in its pedigree an actual reduction of size, which has led to the degeneration of the respiratory organs.

This is an important conclusion: it is a well-known fact that the smaller members of a group are, as a rule, more simply organized than the larger members, especially with regard to their respiratory and circulatory systems; but if we are right in concluding that reduction in size may be an actual cause of simplification or degeneration in structure, then we must be on our guard against assuming hastily that these smaller and simpler animals are necessarily primitive in regard to the groups to which they belong. It is possible, for instance, that the simplification or even absence of respiratory organs seen in Pauropus, in the Thysanura, and in other small Tracheata, may be a secondary character, acquired through reduction of size.

An interesting illustration of the law discussed above is afforded by the brains of mammals; it has been noticed by many anatomists that the extent of convolution, or folding of the surface of the cerebral hemispheres in mammals, is related not to the degree of intelligence of the animal, but to its actual size, a beaver having an almost smooth brain and a cow a highly complicated one. Jelgersma, and, independently of him, Prof. Fitzgerald,[1] have explained this as due to the necessity of preserving the due proportion between the outer layer of grey matter or cortex, which is approximately uniform in thickness, and the central mass of white matter. But for the foldings of the surface the proportion of white matter to grey matter would be far higher in a large than in a small brain.

It must not be forgotten, on the other hand, that many zoologists hold the view, in favour of which the evidence is steadily increasing, that the primitive or ancestral members of each group were of small size. Thus Fürbringer remarks, with regard to birds, that on the whole small birds show more primitive and simpler conditions of structure than the larger members of the same group. He expresses the opinion that the first birds were probably smaller than Archæopteryx, and notes that reptiles and mammals also show in their earlier and smaller types more primitive features than do their larger descendants. Finally, Fürbringer concludes that "it is therefore the study of the smaller members within given groups of animals which promises the best results as to their phylogeny."

Again, one of the most striking points with regard to the pedigree of the horse, as agreed on by palæontologists, is the progressive reduction in size which we meet with as we pass backwards in time from stage to stage. The Pliocene Hipparion was smaller than the existing horse, in fact about the size of a

[1] Cf. Nature, June 5, 1890, p. 125.

donkey; the Miocene Mesohippus about equalled a sheep; while Eohippus, from the Lower Eocene deposits, was no larger than a fox. Not only is there good reason for holding that, as a rule, larger animals are descended from ancestors of smaller size, but there is also much evidence to show that increase in size beyond certain limits is disadvantageous, and may lead to destruction rather than to survival. It has happened more than once in the history of the world, and in more than one group of animals, that gigantic stature has been attained immediately before extinction of the group—a final and tremendous effort to secure survival, but a despairing and unsuccessful one. The Ichthyosauri, Plesiosauri, and other extinct reptilian groups, the Moas, and the huge extinct Edentates, are well-known examples, to which before long will be added the elephants and the whales, and, it may be, ironclads as well.

The whole question of the influence of size is of the greatest possible interest and importance, and it is greatly to be hoped that it will not be permitted to remain in its present uncertain and unsatisfactory condition.

It may be suggested that Amphioxus is an animal which has undergone reduction in size, and that its structural simplicity may, like that of Limapontia, be due, in part at least, to this reduction. Such evidence as we have tells against this suggestion; the first system to undergo degeneration in consequence of a reduction in size is the respiratory, and the respiratory organs of Amphioxus, though very simple, are also, for a vertebrate, unusually extensive.

We have now considered the more important of the influences which are recognized as affecting developmental history in such a way as to render the recapitulation of ancestral stages less complete than it might otherwise be, which tend to prevent ontogeny from correctly repeating the phylogenetic history. It may at this point reasonably be asked whether there is any way of distinguishing the palingenetic history from the later cenogenetic modifications grafted on to it—any test by which we can determine whether a given larval character is or is not ancestral.

Most assuredly there is no one rule, no single test, that will apply in all cases; but there are certain considerations which will help us, and which should be kept in view.

A character that is of general occurrence among the members of a group, both high and low, may reasonably be regarded as having strong claims to ancestral rank; claims that are greatly strengthened if it occurs at corresponding developmental periods in all cases; and still more if it occurs equally in forms that hatch early as free larvæ, and in forms with large eggs, which develop directly into the adult. As examples of such characters may be cited the mode of formation and relations of the notochord, and of the gill clefts of vertebrates, which satisfy all the conditions mentioned.

Characters that are transitory in certain groups, but retained throughout life in allied groups, may, with tolerable certainty, be regarded as ancestral for the former: for instance, the symmetrical position of the eyes in young flat fish, the spiral shell of the young limpet, the superficial positions of the madreporite in Elasipodous Holothurians, or the suckerless condition of the ambulacral feet in many Echinoderms.

A more important consideration is that if the developmental changes are to be interpreted as a correct record of ancestral history, then the several stages must be possible ones, the history must be one that could actually have occurred, *i.e.* the several steps of the history as reconstructed must form a series, all the stages of which are practicable ones.

Natural selection explains the actual structure of a complex organ as having been acquired by the preservation of a series of stages, each a distinct, if slight, advance on the stage immediately preceding it—an advance so distinct as to confer on its possessor an appreciable advantage in the struggle for existence. It is not enough that the ultimate stage should be more advantageous than the initial or earlier condition, but each intermediate stage must also be a distinct advance. If, then, the development of an organ is strictly recapitulatory, it should present to us a series of stages, each of which is not merely functional, but a distinct advance on the stage immediately preceding it. Intermediate stages, *e.g.* the œsophagus of the tadpole, which are not and could not be functional, can form no part of an ancestral series—a consideration well expressed by Sedgwick[1] thus: "Any phylogenetic hypothesis which presents difficulties from a physiological standpoint must be regarded as very provisional indeed."

A good example of an embryological series fulfilling these conditions is afforded by the development of the eye in the higher Cephalopoda. The earliest stage consists in the depression of a slightly modified patch of skin; round the edge of the patch the epidermis becomes raised up as a rim; this gradually grows inwards from all sides, so that the depressed patch now forms a pit, communicating with the exterior through a small hole or mouth. By further growth the mouth of the pit becomes still more narrowed, and ultimately completely closed, so that the pit becomes converted into a closed sac or vesicle; at the point at which final closure occurs, formation of cuticle takes place, which projects as a small transparent drop into the cavity of the sac; by formation of concentric layers of cuticle, this drop becomes enlarged into the spherical transparent lens of the eye, and the development is completed by histological changes in the inner wall of the vesicle, which convert it into the retina, and by the formation of folds of skin around the eye, which become the iris and the eyelids respectively.

Each stage in this developmental history is a distinct advance, physiologically, on the preceding stage, and, furthermore, each stage is retained at the present day as the permanent condition of the eye in some member of the group Mollusca.

The earliest stage, in which the eye is merely a slightly depressed and slightly modified patch of skin, represents the simplest condition of the Molluscan eye, and is retained throughout life in Solen. The stage in which the eye is a pit, with widely open mouth, is retained in the limpet; it is a distinct advance on the former, as through the greater depression the sensory cells are less exposed to accidental injury.

The narrowing of the mouth of the pit in the next stage is a simple change, but a very important step forwards. Up to this point the eye has served to distinguish light from darkness, but the formation of an image has been impossible. Now, owing to the smallness of the aperture, and the pigmentation of the walls of the pit which accompanies the change, light from any one part of an object can only fall on one particular part of the inner wall of the pit or retina, and so an image, though a dim one, is formed. This type of eye is permanently retained in the Nautilus.

The closing of the mouth of the pit by a transparent membrane will not affect the optical properties of the eye, and will be a gain, as it will prevent the entrance of foreign bodies into the cavity of the eye.

The formation of the lens by deposit of cuticle is the next step. The gain here is increased distinctness and increased brightness of the image, for the lens will focus the rays of light more sharply on the retina, and will allow a greater quantity of light, a larger pencil of rays from each part of the object, to reach the corresponding part of the retina. The eye is now in the condition in which it remains throughout life in the snail and other gastropods. Finally the formation of the folds of skin known as iris and eyelids provides for the better protection of the eye, and is a clear advance on the somewhat clumsy method of withdrawal seen in the snail.

The development of the vertebrate liver is another good but simpler example. The most primitive form of the liver is that of Amphioxus, in which it is present as a simple saccular diverticulum of the intestinal canal, with its wall consisting of a single layer of cells, and with blood-vessels on its outer surface. The earliest stage in the formation of the liver in higher vertebrates—the frog, for instance—is practically identical with this. In the frog the next stage consists in folding of the wall of the sac, which increases the efficiency of the organ by increasing the extent of surface in contact with the blood-vessels. The adult condition is attained simply by a continuance of this process; the foldings of the wall becoming more and more complicated, but the essential structure remaining the same—a single layer of epithelial cells in contact on one side with blood-vessels, and bounding on the other directly or indirectly the cavity of the alimentary canal.

It is not always possible to point out the particular advantage gained at each step even when a complete developmental series is known to us, but in such cases, as, for instance, in Orbitolites, our difficulties arise chiefly from ignorance of the particular conditions that confer advantage in the struggle for existence in the case of the forms we are dealing with.

[1] "On the Early Development of the Anterior Part of the Wolffian Duct and Body in the Chick" (*Quarterly Journal of Microscopical Science*, vol. xxi., 1881, p. 456).

The early larval stages in the development of animals, and more especially those that are marine and pelagic in habit, have naturally attracted much attention, since in the absence, probably inevitable, of satisfactory palæontological evidence, they afford us the sole available clue to the determination of the mutual relations of the large groups of animals, or of the points at which these diverged from one another.

In attempting to interpret these early ontogenetic stages as actual ancestral forms, beyond which development at one time did not proceed, we must keep clearly in view the various disturbing causes which tend to falsify the ancestral record, such as the influence of food yolk, or of habitat, and the tendency of diminution in size to give rise to simplification of structure, a point of importance if it be granted that these free larvæ are of smaller size than the ancestral forms to which they correspond.

If, on the other hand, in spite of these powerful modifying causes, we do find a particular larval form occurring widely and in groups not very closely akin, then we certainty are justified in attaching great importance to it, and in regarding it as having strong claims to be accepted as ancestral for these groups.

Concerning these larval forms, and their possible ancestral significance, our knowledge has made no great advance since the publication of Balfour's memorable chapter on this subject; and I propose merely to allude briefly to a few of the more striking instances.

The earliest, the most widely spread, and the most famous of larval forms is the gastrula, which occurs in a simple or in a modified form in some members of each of the large animal groups. It is generally admitted that its significance is the same in all cases, and the evidence is very strong in favour of regarding it as a stage ancestral for all Metazoa. The difficulty arising from its varying mode of development in different forms is, however, still unsolved, and embryologists are not yet agreed whether the invaginate or delaminate form is the more primitive. In favour of the former is its much wider occurrence; in favour of the latter the fact that it is easy to picture a series of stages leading gradually from a unicellular protozoon to a blastula, a diblastula, and ultimately a gastrula, each stage being a distinct advance, both morphological and physiological, on the preceding stage; while in the case of the invaginate gastrula it is not easy to imagine any advantage resulting from a flattening or slight pitting in of one part of the surface, sufficient to lead to its preservation and further development.

Of larval forms later than the gastrula, the most important by far is the Pilidium larva, from which it is possible, as Balfour has shown, that the slightly later Echinoderm larva, as well as the widely spread Trochosphere larva, may both be derived. Balfour concludes that the larval forms of all Cœlomata, excluding the Crustacea and vertebrates, may be derived from one common type, which is most nearly represented now by the Pilidium larva, and which "was an organism something like a Medusa, with a radial symmetry." The tendency of recent phylogenetic speculations is to accept this in full, and to regard as the ancestor of Turbellarians and of all higher forms, a jelly-fish or ctenophoran, which, in place of swimming freely, has taken to crawling on the sea bottom.

Of the two groups excluded above, the Crustacea and the Vertebrata, the interest of the former centres in the much discussed problem of the significance of the Nauplius larva. There is now a fairly general agreement that the primitive Crustacea were types akin to the phyllopods, *i.e.* forms with elongated and many-segmented bodies, and a large number of pairs of similar appendages. If this is correct, then the explanation of the Nauplius stage must be afforded by the phyllopods themselves, and it is no use looking beyond this group for it. A Nauplius larva occurs in other Crustacea merely because they have inherited from their phyllopod ancestors the tendency to develop such a stage, and it is quite legitimate to hold that higher crustaceans are descended from phyllopods, and that the Nauplius represents in more or less modified form an earlier ancestor of the phyllopods themselves.

As to the Nauplius itself, the first thing to note is that, though an early larval form, it cannot be a very primitive form, for it is already an unmistakable crustacean; the absence of cilia, the formation of a cuticular investment, the presence of jointed schizopodous limbs, together with other anatomical characters, proving this point conclusively. It follows, therefore, either that the earlier and more primitive stages are entirely omitted in the development of Crustacea, or else that the Nauplius represents such an early ancestral stage, with crustacean characters, which properly belong to a later stage, thrown back upon it and precociously developed.

The latter explanation is the one usually adopted; but before the question can be finally decided, more accurate observations than we at present possess are needed concerning the stages intermediate between the egg and the Nauplius.

The absence of a heart in the Nauplius may reasonably be associated with the small size of the larva.

Concerning the larval forms of vertebrates, it is only in Amphioxus and the Ascidians that the earliest larval stages are free-living, independent animals. In both groups the most characteristic larval stage is that in which a notochord is present, and a neural tube, open in front, and communicating behind through a neurenteric canal with the digestive cavity, which has no other opening to the exterior. This is a very early stage, both in Amphioxus and Ascidians; but, so far as we know, it cannot be compared with any invertebrate larva. It is customary, in discussions on the affinities of vertebrates, to absolutely ignore the vertebrate larval forms, and to assume that their peculiarities are due to precocious development of vertebrate characteristics. It may turn out that this view of the matter is correct; but it has certainly not yet been proved to be so, and the development of both Amphioxus and Ascidians is so direct and straightforward that evidence of some kind may reasonably be required before accepting the doctrine that this development is entirely deceptive with regard to the ancestry of vertebrates.

Zoologists have not quite made up their minds what to do with Amphioxus: apparently the most guileless of creatures, many view it with the utmost suspicion, and not merely refuse to accept its mute protestations of innocence, but regard and speak of it as the most artful of deceivers. Few questions at the present day are in greater need of authoritative settlement.

That ontogeny really is a repetition of phylogeny must, I think, be admitted, in spite of the numerous and various ways in which the ancestral history may be distorted during actual development.

Before leaving the subject, it is worth while inquiring whether any explanation can be found of recapitulation. A complete answer can certainly not be given at present, but a partial one may, perhaps, be obtained.

Darwin himself suggested that the clue might be found in the consideration that at whatever age a variation first appears in the parent, it tends to reappear at a corresponding age in the offspring; but this must be regarded rather as a statement of the fundamental fact of embryology than as an explanation of it.

It is probably safe to assume that animals would not recapitulate unless they were compelled to do so: that there must be some constraining influence at work, forcing them to repeat more or less closely the ancestral stages. It is impossible, for instance, to conceive what advantage it can be to a reptilian or mammalian embryo to develop gill-clefts which are never used, and which disappear at a slightly later stage, or how it can benefit a whale, that in its embryonic condition it should possess teeth which never cut the gum, and which are lost before birth.

Moreover, the history of development in different animals or groups of animals, offers to us, as we have seen, a series of ingenious, determined, varied, but more or less unsuccessful efforts to escape from the necessity of recapitulating, and to substitute for the ancestral process a more direct method.

A further consideration of importance is that recapitulation is not seen in all forms of development, but only in sexual development, or, at least, only in development from the egg. In the several forms of asexual development, of which budding is the most frequent and most familiar, there is no repetition of ancestral phases; neither is there in cases of regeneration of lost parts, such as the tentacle of a snail, the arm of a starfish, or the tail of a lizard; in such regeneration it is not a larval tentacle, or arm, or tail, that is produced, but an adult one.

The most striking point about the development of the higher animals is that they all alike commence as eggs. Looking more closely at the egg and the conditions of its development, two facts impress us as of special importance: first, the egg is a single cell, and therefore represents morphologically the Protozoan, or earliest ancestral phase; secondly, the egg, before it can develop, must be fertilized by a spermatozoon, just as the stimulus of fertilization by the pollen-grain is necessary before the ovum of a plant will commence to develop into the plant-embryo.

The advantage of cross-fertilization in increasing the vigour of the offspring is well known, and in plants devices of the most varied and even extraordinary kind are adopted to ensure that such cross-fertilization occurs. The essence of the act of cross-fertilization, which is already established among Protozoa, consists in combination of the nuclei of two cells, male and female, derived from different individuals. The nature of the process is of such a kind that two individual cells are alone concerned in it; and it may, I think, be reasonably argued that the reason why animals commence their existence as eggs, *i.e.* as single cells, is because it is in this way only that the advantage of cross-fertilization can be secured, an advantage admittedly of the greatest importance, and to secure which natural selection would operate powerfully.

The occurrence of parthenogenesis, either occasionally or normally, in certain groups is not, I think, a serious objection to this view. There are very strong reasons for holding that parthenogenetic development is a modified form, derived from the sexual method. Moreover, the view advanced above does not require that cross-fertilization should be essential to individual development, but merely that it should be in the highest degree advantageous to the species, and hence leaves room for the occurrence, exceptionally, of parthenogenetic development.

If it be objected that this is laying too much stress on sexual reproduction, and on the advantage of cross-fertilization, then it may be pointed out in reply that sexual reproduction is the characteristic and essential mode of multiplication among Metazoa: that it occurs in all Metazoa, and that when asexual reproduction, as by budding, &c., occurs, this merely alternates with the sexual process which, sooner or later, becomes essential.

If the fundamental importance of sexual reproduction to the welfare of the species be granted, and if it be further admitted that Metazoa are descended from Protozoa, then we see that there is really a constraining force of a most powerful nature compelling every animal to commence its life-history in the unicellular condition, the only condition in which the advantage of cross-fertilization can be obtained; *i.e.* constraining every animal to begin its development at its earliest ancestral stage, at the very bottom of its genealogical tree.

On this view the actual development of any animal is strictly limited at both ends: it must commence as an egg, and it must end in the likeness of the parent. The problem of recapitulation becomes thereby greatly narrowed; all that remains being to explain why the intermediate stages in the actual development should repeat the intermediate stages of the ancestral history.

Although narrowed in this way, the problem still remains one of extreme difficulty.

It is a consequence of the theory of natural selection that identity of structure involves community of descent: a given result can only be arrived at through a given sequence of events: the same morphological goal cannot be reached by two independent paths. A negro and a white man have had common ancestors in the past; and it is through the long-continued action of selection and environment that the two types have been gradually evolved. You cannot turn a white man into a negro merely by sending him to live in Africa: to create a negro the whole ancestral history would have to be repeated; and it may be that it is for the same reason that the embryo must repeat or recapitulate its ancestral history in order to reach the adult goal.

I am not sure that we can at present get much further; but the above considerations give opportunity for brief notice of what is perhaps the most noteworthy of recent embryological papers, Kleinenberg's remarkable monograph on Lopadorhynchus.

Kleinenberg directs special attention to what is known to evolutionists as the difficulty with regard to the origin of new organs, which is to the effect that although natural selection is competent to account for any amount of modification in an organ after it has attained a certain size, and become of functional importance, yet that it cannot account for the earlier stages in the formation of an organ before it has become large enough or sufficiently developed to be of real use. The difficulty is a serious one: it is carefully considered by Mr. Darwin, and met completely in certain cases; but, as Kleinenberg correctly states, no general explanation has been offered with regard to such instances.

As such general explanation Kleinenberg proposes his theory of the development of organs by substitution. He points out that any modification of an organ or tissue must involve modification, at least in functional activity, of other organs. He then continues by urging that one organ may replace or be substituted for another, the replacing organ being in no way derived morphologically from the replaced or preceding organ, but having a genetic relation to it of this kind:—that it can only arise in an organism so constituted, and is dependent on the prior existence of the replaced organ, which supplies the necessary stimulus for its formation.

As an example he takes the axial skeleton of vertebrates. The notochord, formed by change of function from the wall of the digestive canal, is the sole skeleton of the lowest vertebrates, and the earliest developmental phase in all the higher forms. The notochord gives rise directly to no other organ, but is gradually replaced by other and unlike structures by substitution. The notochord is an intermediate organ, and the cartilaginous skeleton which replaces it is only intelligible through the previous existence of the notochord; while, in its turn, the cartilaginous skeleton gives way, being replaced, through substitution, by the bony skeleton.

The successive phases in the evolution of weapons might be quoted as an illustration of Kleinenberg's theory. The bow and arrow is a better weapon than a stick or stone; it is used for the same purpose, and the importance or need for a better weapon led to the replacement of the sling by the bow; the bow does not arise by further development or increasing perfection of the sling; it is an entirely new weapon, towards the formation of which the older and more primitive weapons have acted as a stimulus, and which has replaced these latter by substitution, while the substitution at a later date of firearms for the bow and arrow is merely a further instance of the same principle.

It is too early yet to realize the full significance of Kleinenberg's most suggestive theory; but if it be really true that each historic stage in the evolution of an organ is necessary as a stimulus to the development of the next succeeding stage, then it becomes clear why animals are constrained to recapitulate. Kleinenberg suggests further that the extraordinary persistence in embryonic life of organs which are rudimentary and functionless in the adult may also be explained by his theory, the presence of such organs in the embryo being indispensable as a stimulus to the development of the permanent structures of the adult.

It would be easy to point out difficulties in the way of the theory. The omission of historic stages in the actual ontogenetic development, of which almost all groups of animals supply striking examples, is one of the most serious; for if these stages are necessary as stimuli for the succeeding stages, then their omission requires explanation; while, if such stimuli are not necessary, the theory would appear to need revision.

Such objections may, however, prove to be less serious than they appear at first sight; and in any case Kleinenberg's theory may be welcomed as an important and original contribution, which deserves—indeed demands—the fullest and most careful consideration from all morphologists, and which acquires special interest from the explanation which it offers of recapitulation as a mechanical process, through which alone is it possible for an embryo to attain the adult structure.

That recapitulation does actually occur, that the several stages in the development of an animal are inseparably linked with and determined by its ancestral history, must be accepted. "To take any other view is to admit that the structure of animals and the history of their development form a mere snare to entrap our judgment."

Embryology, however, is not to be regarded as a master-key that is to open the gates of knowledge and remove all obstacles from our path without further trouble on our part; it is rather to be viewed and treated as a delicate and complicated instrument, the proper handling of which requires the utmost nicety of balance and adjustment, and which, unless employed with the greatest skill and judgment, may yield false instead of true results.

Embryology is indeed a most powerful and efficient aid, but it will not, and cannot, provide us with an immediate or complete answer to the great riddle of life. Complications, distortions, innumerable and bewildering, confront us at every step, and the progress of knowledge has so far served rather to increase the number and magnitude of these pitfalls than to teach us how to avoid them.

Still, there is no cause for despair—far from it; if our difficulties are increasing, so also are our means of grappling with them; if the goal appears harder to reach than we thought for, on the other hand its position is far better defined, and the means of approach, the lines of attack, are more clearly recognized.

One thing above all is apparent, that embryologists must not work single-handed, and must not be satisfied with an acquaintance, however exact, with animals from the side of development only; for embryos have this in common with maps, that too close and too exclusive a study of them is apt to disturb a man's reasoning power.

Embryology is a means, not an end. Our ambition is to explain in what manner and by what stages the present structure of animals has been attained. Towards this embryology affords most potent aid; but the eloquent protest of the great anatomist of Heidelberg must be laid to heart, and it must not be forgotten that it is through comparative anatomy that its power to help is derived.

What would it profit us, as Gegenbaur justly asks, to know that the higher vertebrates when embryos have slits in their throats, unless through comparative anatomy we were acquainted with forms now existing in which these slits are structures essential to existence? Anatomy defines the goal, tells us of the things that have to be explained; embryology offers a means, otherwise denied to us, of attaining it.

Comparative anatomy and palæontology must be studied most earnestly by those who would turn the lessons of embryology to best account, and it must never be forgotten that it is to men like Johannes Müller, Stannius, Cuvier, and John Hunter, the men to whom our exact knowledge of comparative anatomy is due, that we owe also the possibility of a science of embryology.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY LIEUTENANT-COLONEL SIR R. LAMBERT PLAYFAIR, K.C.M.G., H.M. CONSUL-GENERAL IN ALGERIA, PRESIDENT OF THE SECTION.

The Mediterranean, Physical and Historical.

WHEN the unexpected honour was proposed to me of presiding over your deliberations, I felt some embarrassment as to the subject of my address. Geography as a science and the necessity of encouraging a more systematic study of it, had been treated in an exhaustive manner during previous meetings. The splendid discoveries of Stanley and the prolonged experiences of Emin have been amply illustrated by the personal narrative of the former. The progress of geography during the past year has been fully detailed in the annual address of the President of the Royal Geographical Society in June last; so that it would be a vain and presumptuous endeavour for me to compress these subjects into the limits of an opening address. Closely connected with them are the magnificent experiments for opening out Africa which are being made by our merchant princes, amongst whom the name of Sir William Mackinnon stands pre-eminent, and by our missionary societies of various churches, all acting cordially in unison, and sinking, in the dark continent, the differences and heartburnings which divide Christianity at home; I have thought it better, however, not to discuss matters so closely connected with political questions which have not yet passed into the realm of history.

In my perplexity I applied for the advice of one of the most experienced geographers of our Society, whose reply brought comfort to my mind. He reminded me that it was generally the custom for Presidents of Sections to select subjects with which they were best acquainted, and added: "What more instructive and captivating subject could be wished than THE MEDITERRANEAN, PHYSICAL AND HISTORICAL?"

For nearly a quarter of a century I have held an official position in Algeria, and it has been my constant delight to make myself acquainted with the islands and shores of the Mediterranean, in the hope of being able to facilitate the travels of my countrymen in that beautiful part of the world.

I cannot pretend to throw much new light on the subject, and I have written so often about it already that what I have to say may strike you as a twice-told tale: nevertheless, if you will permit me to descend from the elevated platform occupied by more learned predecessors, I should like to speak to you in a familiar manner of this "great sea," as it is called in sacred Scripture, the *Mare Internum* of the ancients, "our sea," *Mare nostrum* of Pomponius Mela.

Its shores include about three million square miles of the richest country on the earth's surface, enjoying a climate where the extremes of temperature are unknown, and with every variety of scenery, but chiefly consisting of mountains and elevated plateaux. It is a well-defined region of many parts, all intimately connected with each other by their geographical character, their geological formation, their flora, fauna, and the physiognomy of the people who inhabit them. To this general statement there are two exceptions—namely, Palestine, which belongs rather to the tropical countries lying to the east of it, and so may be dismissed from our subject; and the Sahara, which stretches to the south of the Atlantic region—or region of the Atlas—but approaches the sea at the Syrtis, and again to the eastward of the Cyrenaica, and in which Egypt is merely a long oasis on either side of the Nile.

The Mediterranean region is the emblem of fertility and the cradle of civilization, while the Sahara—Egypt, of course, excepted—is the traditional panther's skin of sand, dotted here and there with oases, but always representing sterility and barbarism. The sea is in no sense, save a political one, the limit between them; it is a mere gulf, which, now bridged by steam, rather unites than separates the two shores. Civilization never could have existed if this inland sea had not formed the junction between the three surrounding continents, rendering the coasts of each easily accessible, whilst modifying the climate of its shores.

The Atlas range is a mere continuation of the south of Europe. It is a long strip of mountain land, about 200 miles broad, covered with splendid forests, fertile valleys, and in some places arid steppes, stretching eastward from the ocean to which it has given its name. The highest point is in Morocco, forming a pendant to the Sierra Nevada of Spain; thence it runs, gradually decreasing in height, through Algeria and Tunisia, it becomes interrupted in Tripoli, and it ends in the beautiful green hills of the Cyrenaica, which must not be confounded with the oases of the Sahara, but is an island detached from the eastern spurs of the Atlas, in the ocean of the desert.

In the eastern part the flora and fauna do not essentially differ from those of Italy; in the west they resemble those of Spain; one of the noblest of the Atlantic conifers, the *Abies pinsapo*, is found also in the Iberian peninsula and nowhere else in the world, and the valuable alfa grass or esparto (*Stipa tenacissima*), from which a great part of our paper is now made, forms one of the principal articles of export from Spain, Portugal, Morocco, Algeria, Tunisia, and Tripoli. On both sides of the sea the former plant is found on the highest and most inaccessible mountains, amongst snows which last during the greater part of the year, and the latter from the sea-level to an altitude of 5000 feet, but in places where the heat and drought would kill any other plant, and in undulating land where water cannot lodge.

Of the 3000 plants found in Algeria by far the greater number are natives of Southern Europe, and less than 100 are peculiar to the Sahara. The *macchie* or maquis of Algeria in no way differs from that of Corsica, Sardinia, and other places; it consists of lentisk, arbutus, myrtle, cistus, tree-heath, and other Mediterranean shrubs. If we take the commonest plant found on the southern shores of the Mediterranean, the dwarf palm (*Chamærops humilis*), we see at once how intimately connected is the whole Mediterranean region, with the exception of the localities I have before indicated. This palm still grows spontaneously in the south of Spain, and in some parts of Provence, in Corsica, Sardinia, and the Tuscan Archipelago, in Calabria and the Ionian Islands, on the continent of Greece, and in several of the islands in the Levant, and it has only disappeared from other countries as the land has been brought under regular cultivation. On the other hand, it occurs neither in Palestine, Egypt, nor in the Sahara.

The presence of European birds may not prove much, but there are mammalia, fish, reptiles, and insects common to both sides of the Mediterranean. Some of the larger animals, such as the lion, panther, jackal, &c., have disappeared before the march of civilization in the one continent, but have lingered, owing to Mohammedan barbarism, in the other. There is abundant evidence of the former existence of these, and of the other large mammals which now characterize tropical Africa, in France, Germany, and Greece; it is probable that they only

migrated to their present habitat after the upheaval of the great sea which in Eocene times stretched from the Atlantic to the Indian Ocean, making Southern Africa an island continent like Australia. The original fauna of Africa, of which the lemur is the distinctive type, is still preserved in Madagascar, which then formed part of it.

The fish fauna is naturally the most conclusive evidence as to the true line of separation between Europe and Africa. We find the trout in the Atlantic region, and in all the snow-fed rivers falling into the Mediterranean; in Spain, Italy, Dalmatia; it occurs in Mount Olympus, in rivers of Asia Minor, and even in the Lebanon, but nowhere in Palestine south of that range, in Egypt, or in the Sahara. This fresh-water salmonoid is not exactly the same in all these localities, but is subject to considerable variation, sometimes amounting to specific distinction. Nevertheless, it is a European type found in the Atlas, and it is not till we advance into the Sahara, at Tuggurt, that we come to a purely African form in the Chromidæ, which have a wide geographical distribution, being found everywhere between that place, the Nile and Mozambique.

The presence of newts, tailed batrachians, in every country round the Mediterranean, except again in Palestine, Egypt, and the Sahara, is another example of the continuity of the Mediterranean fauna, even though the species are not the same throughout.

The Sahara is an immense zone of desert which commences on the shores of the Atlantic Ocean, between the Canaries and Cape de Verde, and traverses the whole of North Africa, Arabia, and Persia, as far as Central Asia. The Mediterranean portion of it may be said roughly to extend between the 15th and 30th degrees of north latitude.

This was popularly supposed to have been a vast inland sea in very recent times, but the theory was supported by geological facts wrongly interpreted. It has been abundantly proved by the researches of travellers and geologists that such a sea was neither the cause nor the origin of the Libyan Desert.

Rainless and sterile regions of this nature are not peculiar to North Africa, but occur in two belts which go round the world in either hemisphere, at about similar distances north and south of the equator. These correspond in locality to the great inland drainage areas from which no water can be discharged into the ocean, and which occupy about one-fifth of the total land surface of the globe.

The African Sahara is by no means a uniform plain, but forms several distinct basins containing a considerable extent of what may almost be called mountain land. The Hoggar Mountains in the centre of the Sahara are 7000 feet high, and are covered during three months with snow. The general average may be taken at 1500. The physical character of the region is very varied; in some places, such as at Tiout, Moghrar, Touat, and other oases in or bordering on Morocco, there are well-watered valleys, with fine scenery and almost European vegetation, where the fruits of the north flourish side by side with the palm tree. In others there are rivers like the Oued Guir, an affluent of the Niger, which the French soldiers, who saw it in 1870, compared to the Loire. Again, as in the bed of the Oued Rir, there is a subterranean river, which gives a sufficient supply of water to make a chain of rich and well-peopled oases equal in fertility to some of the finest portions of Algeria. The greater part of the Sahara, however, is hard and undulating, cut up by dry water-courses, such as the Igharghar, which descends to the Chott Melghigh, and almost entirely without animal or vegetable life.

About one-sixth of its extent consists of dunes of moving sand, a vast accumulation of detritus washed down from more northern and southern regions—perhaps during the glacial epoch—but with no indication of marine formation. These are difficult and even dangerous to traverse; but they are not entirely destitute of vegetation. Water is found at rare but well-known intervals, and there is an abundance of salsolaceous plants which serve as food for the camel. This sand is largely produced by wind action on the underlying rocks, and is not sterile in itself, it is only the want of water which makes it so. Wherever water does exist, or artesian wells are sunk, oases of great fertility never fail to follow.

Some parts of the Sahara are below the level of the sea, and here are formed what are called *chotts* or *sebkhas*, open depressions without any outlets, inundated by torrents from the southern slopes of the Atlas in winter and covered with a saline efflorescence in summer. This salt by no means proves the former existence of an inland sea; it is produced by the concentration of the natural salts, which exist in every variety of soil, washed down by winter rains, with which the unevaporated residue of water becomes saturated.

Sometimes the drainage, instead of flooding open spaces and forming chotts, finds its way through the permeable sand till it meets impermeable strata below it, thus forming vast subterranean reservoirs where the artesian sound daily works as great miracles as did Moses' rod of yore at Meribah. I have seen a column of water thrown up into the air equal to 1300 cubic metres per diem; a quantity sufficient to redeem 1800 acres of land from sterility and to irrigate 60,000 palm trees. This seems to be the true solution of the problem of an inland sea; a sea of verdure and fertility caused by the multiplication of artesian wells, which never fail to bring riches and prosperity in their train.

The climate of the Sahara is quite different from that of what I have called the Mediterranean region, where periodical rains divide the year into two seasons. Here, in many places, years elapse without a single shower; there is no refreshing dew at night, and the winds are robbed of their moisture by the immense continental extents over which they blow. There can be no doubt that it is to these meteorological, and not to geological, causes that the Sahara owes its existence.

Reclus divides the Mediterranean into two basins, which, in memory of their history, he calls the Phœnician and the Carthaginian, or the Greek and Roman seas, more generally known to us as the Eastern and Western Basins, separated by the island of Sicily.

If we examine the submarine map of the Mediterranean, we see that it must at one time have consisted of two enclosed or inland basins, like the Dead Sea. The western one is separated from the Atlantic by the Straits of Gibraltar, a shallow ridge, the deepest part of which is at its eastern extremity, averaging about 300 fathoms; while on the west, bounded by a line from Cape Spartel to Trafalgar, it varies from 50 to 200 fathoms. Fifty miles to the west of the Straits the bottom suddenly sinks down to the depths of the Atlantic, while to the east it descends to the general level of the Mediterranean, from 1000 to 2000 fathoms.

The Western is separated from the Eastern Basin by the isthmus which extends between Cape Bon in Tunisia and Sicily, known as the "Adventure Bank," on which there is not more than from 30 to 250 fathoms. The depth between Italy and Sicily is insignificant, and Malta is a continuation of the latter, being only separated from it by a shallow patch of from 50 to 100 fathoms; while to the east and west of this bank the depth of the sea is very great. These shallows cut off the two basins from all but superficial communication.

The configuration of the bottom shows that the whole of this strait was at one time continuous land, affording free communication for land animals between Africa and Europe. The palæontological evidence of this is quite conclusive. In the caves and fissures of Malta, amongst river detritus, are found three species of fossil elephants, a hippopotamus, a gigantic dormouse, and other animals which could never have lived in so small an island. In Sicily, remains of the existing elephant have been found, as well as the *Elephas antiquus*, and two species of hippopotamus, while nearly all these and many other animals of African type have been found in the Pliocene deposits and caverns of the Atlantic region.

The rapidity with which such a transformation might have occurred can be judged by the well-known instance of Graham's Shoal, between Sicily and the island of Pantellaria; this, owing to volcanic agency, actually rose above the water in 1832, and for a few weeks had an area of 3240 feet in circumference and a height of 107 feet.

The submersion of this isthmus no doubt occurred when the waters of the Atlantic were introduced through the Straits of Gibraltar. The rainfall over the entire area of the Mediterranean is certainly not more than 30 inches, while the evaporation is at least twice as great; therefore, were the Straits to be once more closed, and were there no other agency for making good this deficiency, the level of the Mediterranean would sink again till its basin became restricted to an area no larger than might be necessary to equalize the amount of evaporation and precipitation. Thus not only would the strait between Sicily and Africa be again laid dry, but the Adriatic and Ægean Seas also, and a great part of the Western Basin.

The entire area of the Mediterranean and Black Seas has been

estimated at upwards of a million square miles, and the volume of the rivers which are discharged into them at 226 cubic miles. All this and much more is evaporated annually. There are two constant currents passing through the Straits of Gibraltar, superimposed on each other; the upper and most copious one flows in from the Atlantic at a rate of nearly three miles an hour, or 140,000 cubic metres per second, and supplies the difference between the rainfall and evaporation, while the under-current of warmer water, which has undergone concentration by evaporation, is continually flowing out at about half the above rate of movement, getting rid of the excess of salinity; even thus, however, leaving the Mediterranean salter than any other part of the ocean except the Red Sea.

A similar phenomenon occurs at the eastern end, where the fresher water of the Black Sea flows as a surface current through the Dardanelles, and the salter water of the Mediterranean pours in below it.

The general temperature of the Mediterranean from a depth of fifty fathoms down to the bottom is almost constantly 56°, whatever may be its surface elevation. This is a great contrast to that of the Atlantic, which at a similar depth is at least 3° colder, and which at 1000 fathoms sinks to 40°.

This fact was of the greatest utility to Dr. Carpenter in connection with his investigations regarding currents through the Straits, enabling him to distinguish with precision between Atlantic and Mediterranean water.

For all practical purposes the Mediterranean may be accepted as being, what it is popularly supposed to be, a tideless sea, but it is not so in reality. In many places there is a distinct rise and fall, though this is more frequently due to winds and currents than to lunar attraction. At Venice there is a rise of from one to two feet in spring tides, according to the prevalence of winds up or down the Adriatic, but in that sea itself the tides are so weak that they can hardly be recognized, except during the prevalence of the Bora, our old friend *Boreas*, which generally raises a surcharge along the coast of Italy. In many straits and narrow arms of the sea there is a periodical flux and reflux, but the only place where tidal influence, properly so called, is unmistakably observed is in the Lesser Syrtis, or Gulf of Gabes; there the tide runs at the rate of two or three knots an hour, and the rise and fall varies from three to eight feet. It is most marked and regular at Djerba, the Homeric island of the Lotophagi; one must be careful in landing there in a boat, so as not to be left high and dry a mile or two from the shore. Perhaps the companions of Ulysses were caught by the receding tide, and it was not only a banquet of dates, the "honey-sweet fruit of the Lotus," or the potent wine which is made from it, which made them "forgetful of their homeward way."

The Gulf of Gabes naturally calls to mind the proposals which were made a few years ago for inundating the Sahara, and so restoring to the Atlantic region the insular condition which it is alleged to have had in prehistoric times. I will not allude to the English project for introducing the waters of the Atlantic from the west coast of Africa; that does not belong to my subject. The French scheme advocated by Commandant Roudaire, and supported by M. de Lesseps, was quite as visionary and impracticable.

To the south of Algeria and Tunis there exists a great depression stretching westward from the Gulf of Gabes to a distance of about 235 miles, in which are several *chotts* or salt lakes, sometimes only marshes, and in many places covered with a saline crust strong enough to bear the passage of camels. Commandant Roudaire proposed to cut through the isthmuses which separated the various chotts, and so prepare their basins to receive the waters of the Mediterranean. This done, he intended to introduce the sea by a canal, which should have a depth of one metre below low-water level.

This scheme was based on the assumption that the basin of the chotts had been an inland sea within historic times; that, little by little, owing to the difference between the quantity of water which entered and the amount of evaporation and absorption, this interior sea had disappeared, leaving the chotts as an evidence of the former condition of things; that, in fact, this was none other than the celebrated Lake Triton, the position of which has always been a puzzle to geographers.

This theory, however, is untenable; the isthmus of Gabes is not a mere sandbank; there is a band of rock between the sea and the basin of the chotts, through which the former never could have penetrated in modern times. It is much more probable that Lake Triton was the large bight between the island of Djerba and the mainland, on the shores of which are the ruins of the ancient city of Meninx, which, to judge by the abundance of Greek marble found there, must have carried on an important commerce with the Levant.

The scheme has now been entirely abandoned; nothing but the mania for cutting through isthmuses all over the world which followed the brilliant success achieved at Suez can explain its having been started at all. Of course, no mere mechanical operation is impossible in these days, but the mind refuses to realize the possibility of vessels circulating in a region which produces nothing, or that so small a sheet of water in the immensity of the Sahara could have any appreciable effect in modifying the climate of its shores.

The Eastern Basin is much more indented and cut up into separate seas than the Western one; it was therefore better adapted for the commencement of commerce and navigation; its high mountains were landmarks for the unpractised sailor, and its numerous islands and harbours afforded shelter for his frail barque, and so facilitated communication between one point and another.

The advance of civilization naturally took place along the axis of this sea, Phœnicia, Greece, and Italy being successively the great nurseries of human knowledge and progress. Phœnicia had the glory of opening out the path of ancient commerce, for its position in the Levant gave it a natural command of the Mediterranean, and its people sought the profits of trade from every nation which had a seaboard on the three continents washed by this sea. Phœnicia was already a nation before the Jews entered the Promised Land, and when they did so they carried on inland traffic as middlemen to the Phœnicians. Many of the commercial centres on the shores of the Mediterranean were founded before Greece and Rome acquired importance in history. Homer refers to them as daring traders nearly a thousand years before the Christian era.

For many centuries the commerce of the world was limited to the Mediterranean, and when it extended in the direction of the East it was the merchants of the Adriatic, of Genoa, and of Pisa who brought the merchandise of India, at an enormous cost, to the Mediterranean by land, and who monopolized the carrying trade by sea. It was thus that the elephant trade of India, the caravan traffic through Babylon and Palmyra, as well as the Arab *kafilehs*, became united with the Occidental commerce of the Mediterranean.

As civilization and commerce extended westwards, mariners began to overcome their dread of the vast solitudes of the ocean beyond the Pillars of Hercules, and the discovery of America by Columbus, and the circumnavigation of Africa by the Portuguese, changed entirely the current of trade as well as increased its magnitude, and so relegated the Mediterranean, which had hitherto been the central sea of human intercourse, to a position of secondary importance.

Time will not permit me to enter into further details regarding the physical geography of this region, and its history is a subject so vast that a few episodes of it are all that I can possibly attempt. It is intimately connected with that of every other country in the world, and here were successively evolved all the great dramas of the past and some of the most important events of less distant date.

As I have already said, long before the rise of Greece and Rome its shores and islands were the seat of an advanced civilization. Phœnicia had sent out her pacific colonies to the remotest parts, and not insignificant vestiges of their handicraft still exist to excite our wonder and admiration. We have the megalithic temples of Malta sacred to the worship of Baal, the generative god, and Ashtoreth, the conceptive goddess, of the universe. The three thousand *nurhagi* of Sardinia, round towers of admirable masonry, intended probably for defence in case of sudden attack, and the so-called giant graves, were as great a mystery to classical authors as they are to us at the present day. Menorca has its *talayots*, tumuli somewhat analogous to, but of ruder construction than, the *nurhagi*, more than 200 groups of which exist in various parts of the island; with these are associated subordinate constructions intended for worship; altars composed of two immense monoliths, erected in the form of a T; sacred enclosures and megalithic habitations. One type of *talayot* is especially remarkable, of better masonry than the others, and exactly resembling inverted boats. One is tempted to believe that the Phœnicians had in view the grass habitations

or *mapalia* of the Numidians described by Sallust, and had endeavoured to reproduce them in stone: *Oblonga, incurvis lateribus tecta, quasi navium carinæ sunt.*

For a long time the Phœnicians had no rivals in navigation, but subsequently the Greeks—especially the Phocians—established colonies in the Western Mediterranean, in Spain, Corsica, Sardinia, Malta, and the south of France, through the means of which they propagated not only their commerce but their arts, literature, and ideas. They introduced many valuable plants, such as the olive, thereby modifying profoundly the agriculture of the countries in which they settled. They have even left traces of their blood, and it is no doubt to this that the women of Provence owe the classical beauty of their features.

But they were eclipsed by their successors; the empire of Alexander opened out a road to India, in which, indeed, the Phœnicians had preceded him, and introduced the produce of the East into the Mediterranean, while the Tyrian colony of Carthage became the capital of another vast empire, which, from its situation, midway between the Levant and the Atlantic Ocean, enabled it to command the Mediterranean traffic.

The Carthaginians at one time ruled over territory extending along the coast from Cyrene to Numidia, besides having a considerable influence over the interior of the continent, so that the name of Africa, given to their own dominions, was gradually applied to a whole quarter of the globe. The ruling passion with the Carthaginians was love of gain, not patriotism, and their wars were largely fought with mercenaries. It was the excellence of her civil constitution which, according to Aristotle, kept in cohesion for centuries her straggling possessions. A country feebly patriotic, which entrusts her defence to foreigners, has the seeds of inevitable decay, which ripened in her struggle with Rome, despite the warlike genius of Hamilcar and the devotion of the magnanimous Hannibal. The gloomy and cruel religion of Carthage, with its human sacrifices to Moloch and its worship of Baal under the name of Melkarth, led to a criminal code of Draconic severity and alienated it from surrounding nations. When the struggle with Rome began, Carthage had no friends. The first Punic War was a contest for the possession of Sicily, whose prosperity is even now attested by the splendour of its Hellenic monuments. When Sicily was lost by the Carthaginians, so also was the dominion of the sea, which hitherto had been uncontested. The second Punic War resulted in the utter prostration of Carthage and the loss of all her possessions out of Africa; and in 201 B.C., when this war was ended, 552 years after the foundation of the city, Rome was mistress of the world.

The destruction of Carthage after the third Punic War was a heavy blow to Mediterranean commerce. It was easy for Cato to utter his stern *Delenda est Carthago;* destruction is easy, but construction is vastly more difficult. Although Augustus in his might built a new Carthage near the site of the old city, he could never attract again the trade of the Mediterranean which had been diverted into other channels. Roman supremacy was unfavourable to the growth of commerce, because, though she allowed unrestricted trade throughout her vast empire, and greatly improved internal communications in the subjugated countries, Rome itself absorbed the greater part of the wealth, and did not produce any commodities in return for its immense consumption, therefore Mediterranean commerce did not thrive under the Roman rule. The conquest of Carthage, Greece, Egypt, and the East poured in riches to Rome, and dispensed for a time with the needs of productive industry, but formed no enduring basis of prosperity.

It is only in relation to the Mediterranean that I can refer to Roman history, but I must allude to the interesting episode in the life of Diocletian, who, after an anxious reign of twenty-one years in the eastern division of the empire, abdicated at Nicomedia and retired to his native province of Illyria. He spent the rest of his life in rural pleasures and horticulture at Salona, near which he built that splendid palace within the walls of which subsequently arose the modern city of Spalato. Nothing more interesting exists on the shores of the Mediterranean than this extraordinary edifice, perhaps the largest that ever arose at the bidding of a single man; not only vast and beautiful, but marking one of the most important epochs in the history of architecture.

Though now obstructed with a mass of narrow, tortuous streets, its salient features are distinctly visible. The great temple, probably the mausoleum of the founder, has become the cathedral, and after the Pantheon at Rome there is no finer specimen of a heathen temple turned into a Christian church. Strange it is that the tomb of him whose reign was marked by such unrelenting persecution of the Christians should have been accepted as the model of those baptisteries so commonly constructed in the following centuries.

Of Diocletian's Salona, one of the chief cities of the Roman world, but little now remains save traces of the long irregular wall; recent excavations have brought to light much that is interesting, but all of the Christian epoch, such as a large basilica which had been used as a necropolis, and a baptistery, one of those copied from the temple of Spalato, on the Mosaic pavement of which can still be read the text, *Sicut cervus desiderat fontem aquarum ita anima mea ad te Deus.*

The final partition of the Roman Empire took place in 365; forty years later the barbarians of the North began to invade Italy and the south of Europe; and in 429, Genseric, at the head of his Vandal hordes, crossed over into Africa from Andalusia, a province which still bears their name, devastating the country as far as the Cyrenaica. He subsequently annexed the Balearic Islands, Corsica and Sardinia, he ravaged the coasts of Italy and Sicily, and even of Greece and Illyria, but the most memorable of his exploits was the unresisted sack of Rome, whence he returned to Africa laden with treasure and bearing the Empress Eudoxia a captive in his train.

The degenerate emperors of the West were powerless to avenge this insult, but Byzantium, though at this time sinking to decay, did make a futile attempt to attack the Vandal monarch in his African stronghold. It was not, however, till 533, in the reign of Justinian, when the successors of Genseric had fallen into luxurious habits and had lost the rough valour of their ancestors, that Belisarius was able to break their power and take their last king a prisoner to Constantinople. The Vanda domination in Africa was destroyed, but that of the Byzantines was never thoroughly consolidated; it rested not on its own strength, but on the weakness of its enemies, and it was quite unable to cope with the next great wave of invasion which swept over the land, perhaps the most extraordinary event in the world's history, save only the introduction of Christianity.

In 647, twenty-seven years after the Hedjira of Mohammed, Abdulla ibn Saad started from Egypt for the conquest of Africa with an army of 40,000 men.

The expedition had two determining causes—the hope of plunder and the desire to promulgate the religion of El Islam. The sands and scorching heat of the desert, which had nearly proved fatal to the army of Cato, were no bar to the hardy Arabians and their enduring camels. The march to Tripoli was a fatiguing one, but it was successfully accomplished; the invaders did not exhaust their force in a vain effort to reduce its fortifications, but swept on over the Syrtic desert, and north to the province of Africa, where, near the splendid city of Suffetula, a great battle was fought between them and the army of the Exarch Gregorius, in which the Christians were signally defeated, their leader killed, and his daughter allotted to Ibn-ez-Zobair, who had slain her father.

Not only did the victorious Moslems overrun North Africa, but soon they had powerful fleets at sea which dominated the entire Mediterranean, and the emperors of the East had enough to do to protect their own capital.

Egypt, Syria, Spain, Provence, and the islands of the Mediterranean successively fell to their arms, and until they were checked at the Pyrenees by Charles Martel it seemed at one time as if the whole of Southern Europe would have been compelled to submit to the disciples of the new religion. Violent, implacable, and irresistible at the moment of conquest, the Arabs were not unjust or hard masters in countries which submitted to their conditions. Every endeavour was, of course, made to proselytize, but Christians were allowed to preserve their religion on payment of a tax, and even Popes were in the habit of entering into friendly relations with the invaders. The Church of St. Cyprian and St. Augustine, with its 500 Sees, was indeed expunged, but five centuries after the passage of the Mohammedan army from Egypt to the Atlantic a remnant of it still existed. It was not till the twelfth century that the religion and language of Rome became utterly extinguished.

The Arabs introduced a high state of civilization into the countries where they settled; their architecture is the wonder and admiration of the world at the present day; their irrigational

works in Spain have never been improved upon; they fostered literature and the arts of peace, and introduced a system of agriculture far superior to what existed before their arrival.

Commerce, discouraged by the Romans, was highly honoured by the Arabs, and during their rule the Mediterranean recovered the trade which it possessed in the time of the Phœnicians and Carthaginians; it penetrated into the Indian Archipelago and China; it travelled westward to the Niger, and to the east as far as Madagascar, and the great trade route of the Mediterranean was once more developed.

The power and prosperity of the Arabs culminated in the ninth century, when Sicily fell to their arms; it was not, however, very long before their empire began to be undermined by dissensions; the temporal and spiritual authority of the Ommiade Khalifs, which extended from Sind to Spain, and from the Oxus to Yemen, was overthrown by the Abbasides in the year 132 of the Hedjira, A.D. 750. Seven years later Spain detached itself from the Abbasside empire; a new Caliphate was established at Cordova, and hereditary monarchies began to spring up in other Mohammedan countries.

The Carlovingian empire gave an impulse to the maritime power of the south of Europe, and in the Adriatic the fleets of Venice and Ragusa monopolized the traffic of the Levant. The merchants of the latter noble little republic penetrated even to our own shores, and Shakespeare has made the Argosy or Ragusie a household word in our language.

During the eleventh century the Christian Powers were no longer content to resist the Mohammedans: they began to turn their arms against them. If the latter ravaged some of the fairest parts of Europe, the Christians began to take brilliant revenge.

The Mohammedans were driven out of Corsica, Sardinia, Sicily, and the Balearic Islands, but it was not till 1492 that they had finally to abandon Europe, after the conquest of Granada by Ferdinand and Isabella.

About the middle of the eleventh century an event took place which profoundly modified the condition of the Mohammedan world. The Caliph Mostansir let loose a horde of nomad Arabs, who, starting from Egypt, spread over the whole of North Africa, carrying destruction and blood wherever they passed, thus laying the foundation for the subsequent state of anarchy which rendered possible the interference of the Turks.

English commercial intercourse with the Mediterranean was not unknown even from the time of the Crusades, but it does not appear to have been carried on by means of our own vessels till the beginning of the sixteenth century. In 1522 it was so great that Henry VIII. appointed a Cretan merchant, Censio de Balthazari, to be "Master, governor, protector, and consul of all and singular the merchants and others his lieges and subjects within the port, island, and country of Crete or Candia." This is the very first English consul known to history, but the first of English birth was my own predecessor in office, Master John Tipton, who, after having acted at Algiers during several years in an unofficial character, probably elected by the merchants themselves to protect their interests, was duly appointed consul by Sir William Harebone, ambassador at Constantinople in 1585, and received just such an exequatur from the Porte as has been issued to every consul since by the Government of the country in which he resides.

Piracy has always been the scourge of the Mediterranean, but we are too apt to associate its horrors entirely with the Moors and Turks. The evil had existed from the earliest ages; even before the Roman conquest of Dalmatia the Illyrians were the general enemies of the Adriatic; Africa under the Vandal reign was a nest of the fiercest pirates; the Venetian chronicles are full of complaints of the ravages of the Corsairs of Ancona, and there is no other name but piracy for such acts of the Genoese as the unprovoked pillage of Tripoli by Andrea Doria in 1535. To form a just idea of the Corsairs of the past it is well to remember that commerce and piracy were often synonymous terms, even among the English, up to the reign of Elizabeth. Listen to the description given by the pious Cavendish of his commercial circumnavigation of the globe:—"It hath pleased Almighty God to suffer me to circumpass the whole globe of the world. . . . I navigated along the coast of Chili, Peru, and New Spain, where I made great spoils. All the villages and towns that ever I landed at I burned and spoiled, and had I not been discovered upon the coast I had taken a great quantity of treasure," and so he concludes, "The Lord be praised for all his mercies!"

Sir William Monson, when called upon by James I. to propose a scheme for an attack on Algiers, recommended that all the maritime Powers of Europe should contribute towards the expense, and participate in the gains by the sale of Moors and Turks as slaves.

After the discovery of America and the expulsion of the Moors from Spain, piracy developed to an extraordinary extent. The audacity of the Barbary corsairs seems incredible at the present day; they landed on the shores and islands of the Mediterranean, and even extended their ravages to Great Britain, carrying off all the inhabitants whom they could seize into the most wretched slavery. The most formidable of these piratical States was Algiers, a military oligarchy, consisting of a body of janissaries, recruited by adventurers from the Levant, the outcasts of the Mohammedan world, criminals and renegades from every nation in Europe. They elected their own ruler or Dey, who exercised despotic sway, tempered by frequent assassination; they oppressed without mercy the natives of the country, accumulated vast riches, had immense numbers of Christian slaves, and kept all Europe in a state bordering on subjection by the terror which they inspired. Nothing is sadder or more inexplicable than the shameful manner in which this state of things was accepted by civilized nations. Many futile attempts were made during successive centuries to humble their arrogance, but it only increased by every manifestation of the powerlessness of Europe to restrain it. It was reserved for our own countryman, Lord Exmouth, by his brilliant victory in 1816, for ever to put an end to piracy and Christian slavery in the Mediterranean. His work, however, was left incomplete, for though he destroyed the navy of the Algerines, and so rendered them powerless for evil on the seas, they were far from being humbled; they continued to slight their treaties and to subject even the agents of powerful nations to contumely and injustice. The French took the only means possible to destroy this nest of ruffians, by the almost unresisted occupation of Algiers and the deportation of its Turkish aristocracy.

They found the whole country in the possession of a hostile people, some of whom had never been subdued since the fall of the Roman Empire, and the world owes France no small debt of gratitude for having transformed what was a savage and almost uncultivated country into one of the richest as well as the most beautiful in the basin of the Mediterranean.

What has been accomplished in Algeria is being effected in Tunisia. The treaty of the Kasr-es-Saeed, which established a French Protectorate there, and the military occupation of the Regency, were about as high-handed and unjustifiable acts as are recorded in history; but there can be no possible doubt regarding the important work of civilization and improvement that has resulted from them. European courts of justice have been established all over the country; the exports and imports have increased from twenty-three to fifty-one millions of francs, the revenue from six to nineteen millions, without the imposition of a single new tax, and nearly half a million per annum is being spent on education.

Sooner or later the same thing must happen in the rest of North Africa, though at present international jealousies retard this desirable consummation. It seems hard to condemn such fair countries to continued barbarism, in the interest of tyrants who misgovern and oppress their people. The day cannot be far off when the whole southern shores of the Mediterranean will enjoy the same prosperity and civilization as the northern coast, and when the deserts, which are the result of misgovernment and neglect, will assume the fertility arising from security and industry, and will again blossom as the rose.

It cannot be said that any part of the Mediterranean basin is still unknown, if we except the empire of Morocco. But even that country has been traversed in almost every direction during the past twenty years, and its geography and natural history have been illustrated by men of the greatest eminence; such as Gerhard Rohlfs, Monsieur Tissot, Sir Joseph Hooker, the Vicomte de Foucauld, Joseph Thomson, and numerous other travellers. The least known portion, at least on the Mediterranean coast, is the Riff country, the inhospitality of whose inhabitants has given the word "ruffian" to the English language. Even that has been penetrated by De Foucauld disguised as a Jew, and the record of his exploration is one of the most brilliant contributions to the geography of the country which has hitherto been made.

Although, therefore, but little remains to be done in the way of actual exploration, there are many by-ways of travel com-

paratively little known to that class of the community with which I have so much sympathy, the ordinary British tourist. These flock every year in hundreds to Algeria and Tunis, but few of them visit the splendid Roman remains in the interior of those countries. The Cyrenaica is not so easily accessible, and I doubt whether any Englishmen have travelled in it since the exploration of Smith and Porcher in 1861.

Cyrene almost rivalled Carthage in commercial importance. The Hellenic ruins still existing bear witness to the splendour of its five great cities. It was the birthplace of many distinguished people, and amongst its hills and fountains were located some of the most interesting scenes in mythology, such as the Gardens of the Hesperides, and the "silent, dull, forgetful waters of Lethe."

This peninsula is only separated by a narrow strait from Greece, whence it was originally colonized. There, and indeed all over the eastern basin of the Mediterranean, are many little-trodden routes; but the subject is too extensive; I am reluctantly compelled to restrict my remarks to the western half.

The south of Italy is more frequently traversed and less travelled in than any part of that country. Of the thousands who yearly embark or disembark at Brindisi, few ever visit the Land of Manfred. Otranto is only known to them from the fanciful descriptions in Horace Walpole's romance. The general public in this country is quite ignorant of what is going on at Taranto, and of the great arsenal and dockyard which Italy is constructing in the Mare Piccolo, an inland sea containing more than 1000 acres of anchorage for the largest ironclads afloat, yet with an entrance so narrow that it is spanned by a revolving bridge. Even the Adriatic, though traversed daily by steamers of the Austrian Lloyd's Company, is not a highway of travel; yet where is it possible to find so many places of interest within the short space of a week's voyage, between Corfu and Trieste, as along the Dalmatian and Istrian shores, and among the islands that fringe the former, where it is difficult to realize that one is at sea at all, and not on some great inland lake?

There is the Bocche di Cattaro, a vast rent made by the Adriatic among the mountains, where the sea flows round their spurs in a series of canals, bays, and lakes of surpassing beauty. The city of Cattaro itself, the gateway of Montenegro, with its picturesque Venetian fortress, nestling at the foot of the black mountain, Ragusa, the Roman successor of the Hellenic Epidaurus, Queen of the Southern Adriatic, battling with the waves on her rock-bound peninsula, the one spot in all that sea which never submitted either to Venice or the Turk, and for centuries resisting the barbarians on every side, absolutely unique as a mediæval fortified town, and worthy to have given her name to the argosies she sent forth; Spalato, the grandest of Roman monuments; Lissa, colonized by Dionysius of Syracuse, and memorable to us as having been a British naval station from 1812 to 1814, while the French held Dalmatia; Zara, the capital, famous for its siege by the Crusaders, interesting from an ecclesiological point of view, and venerated as the last resting-place of St. Simeon, the prophet of the *Nunc dimittis*; Parenza, with its great basilica; Pola, with its noble harbour, whence Belisarius sailed forth, now the chief naval port of the Austrian Empire, with its Roman amphitheatre and graceful triumphal arches; besides many other places of almost equal interest. Still further west are Corsica, Sardinia, and the Balearic Islands, all easily accessible from the coasts of France, Italy, and Spain. Their ports are constantly visited by mail-steamers and private yachts, yet they are but little explored in the interior.

A physical and historical description of Corsica was then given. The address concluded as follows:—

I have endeavoured to sketch, necessarily in a very imperfect manner, the physical character and history of the Mediterranean, to show how the commerce of the world originated in a small maritime State at its eastern extremity; how it gradually advanced westward till it burst through the Straits of Gibraltar, and extended over seas and continents until then undreamt of, an event which deprived the Mediterranean of that commercial prosperity and greatness which for centuries had been limited to its narrow basin.

Once more this historic sea has become the highway of nations; the persistent energy and genius of two men have revolutionized navigation, opened out new and boundless fields for commerce; and it is hardly too much to say that if the Mediterranean is to be restored to its old position of importance; if the struggle for Africa is to result in its regeneration, as happened in the New World; if the dark places still remaining in the further East are to be civilized, it will be in a great measure due to Waghorn and Ferdinand de Lesseps, who developed the overland route and created the Suez Canal.

But the Mediterranean can only hope to retain its regenerated position in time of peace. Nothing is more certainly shown by past history than that war and conquest have changed the route of commerce in spite of favoured geographical positions. Babylon was conquered by Assyrians, Persians, Macedonians, and Romans, and though for a time her position on the Euphrates caused her to rise like a Phœnix from her ashes, successive conquests, combined with the luxury and effeminacy of her rulers, caused her to perish. Tyre, conquered by Nebuchadnezzar and Alexander, fell as completely as Babylon had done, and her trade passed to Alexandria. Ruined sites of commercial cities rarely again become emporia of commerce; Alexandria is an exception dependent on very exceptional circumstances.

The old route to the East was principally used by sailing-vessels, and was abandoned for the shorter and more economical one by the Suez Canal, which now enables a round voyage to be made in sixty days, which formerly required from six to eight months. This, however, can only remain open in time of peace. It is quite possible that in the event of war the old route by the Cape may be again used, to the detriment of traffic by the Mediterranean. Modern invention has greatly economized the use of coal; and steamers, by the use of duplex and triplex engines, can run with a comparatively small consumption of fuel, thus leaving a larger space for cargo. England, the great carrying Power of the world, may find it more advantageous to trust to her own strength and the security of the open seas than to run the gauntlet of the numerous strategical positions in the Mediterranean, such as Port Mahon, Bizerta, and Taranto, each of which is capable of affording impregnable shelter to a hostile fleet, and though the ultimate key to the Indian Ocean is in our own hands, our passage to it may be beset with a thousand dangers. There is no act of my career on which I look back with so much satisfaction as on the share I had in the occupation of Perim, one of the most important links in that chain of coaling stations which extends through the Mediterranean to the further East, and which is so necessary for the maintenance of our naval supremacy. It is a mere islet, it is true, a barren rock, but one surrounding a noble harbour, and so eminently in its right place that we cannot contemplate with equanimity the possibility of its being in any other hands than our own.

It is by no means certain whether exaggerated armaments are best suited for preserving peace or hastening a destructive war; the golden age of disarmament and international arbitration may not be near at hand, but it is even now talked of as a possibility.

Should the poet's prophecy or the patriot's dream be realized, and a universal peace indeed bless the world, then this sea of so many victories may long remain the harvest field of a commerce nobler than conquest.

NOTES.

THE Kew Herbarium has just been enriched by a set of the dried plants from the extensive collections made by Regel, Przewalski, Potanin, and other recent Russian travellers in Central and Eastern Asia. This valuable set numbers about 2600 species, including very many novelties, and it was presented to the Royal Gardens, Kew, through the good offices of Dr. A. E. von Regel, Director of the Imperial Botanic Garden at St. Petersburg, and Mr. C. J. Maximowicz, the Curator of the Herbarium in the same establishment.

A LABORATORY for plant-biology has been recently opened at Fontainebleau. It is under the direction of M. Bonnier, Professor of Botany at the Sorbonne in Paris, to whom application should be made by any contemplating research there.

DR. WILLIAM WAAGEN, F.G.S., formerly Palæontologist to the Geological Survey of India, and of late years Professor of Geology at Prague, has been appointed Professor of Palæontology to the University of Vienna, in succession to the late Dr. Neumayr.

IN a "Supplement to the Catalogue of Diurnal Accipitres in the Australian Museum at Sydney, N.S.W.," Dr. E. P. Ramsay

gives descriptions of many plumages of birds of prey not previously recorded. The nestlings described have been preserved for the Museum by Mr. K. H. Bennett, and include those of the rarest of the Australian accipitres. The colours of the soft parts have been most carefully noted, and are deemed in one instance (*Aquila morphnoides*, p. 7) to be worthy of a duplicate reproduction by Dr. Ramsay.

THE Rev. Dr. Norman has just returned from a dredging expedition in the Varanger Fiord and Sydvaranger. He has been absent nine weeks, and has brought home extensive collections in all branches of Marine Invertebrata. The fiords of Sydvaranger were found to possess a rich fauna, with depths descending to 120 fathoms. These fiords had never before been scientifically investigated, though Baron de Guerne took a few hauls of the dredge there in 1881 when on board the French vessel *Coligny* as a member of the Mission Scientifique en Laponie, and published a list of the Mollusca obtained.

DR. RAMSAY has also compiled a catalogue of the Striges in the Australian Museum, which appears to possess a good series of every species known to inhabit Australia and the adjacent islands, with the exception of *Ninox ocellata* and *Ninox rufa*, the latter being a good species in Dr. Ramsay's opinion, though Mr. Sharpe considered it to be the young of *N. connivens*. It is to be hoped that Dr. Ramsay will continue his useful catalogues of the specimens of birds in the Australian Museum.

DR. J. B. STEERE has just published his preliminary descriptions of new species discovered by the members of the expedition to the Philippine Islands. It is to be hoped that a complete memoir on this important exploration will be published later on, as the diagnoses set forth in the little *brochure* just issued are, in many cases, worse than useless.

IN the *Times* for September 9 we read the following note on "How to keep salt dry"—"The Dutch Indian Government offers a prize of 10,000 fl. for the best practical answer to the question in what manner the salt which is sold in Dutch India in small packets should be packed up so as to keep dry."

WE have received the general Guide to the Science and Art Museum, Dublin, under the directorship of V. Ball, LL.D., F.R.S. The Museum is divided into two parts. Part I., which is in the old museum buildings, deals with natural history, while Part II. treats of arts and industry, and is in the new buildings. In this Museum there are short printed labels attached to the specimens, and for the more important objects, descriptive tables containing "greater detail than even an ordinary hand-book could conveniently contain" are added. In some cases small maps are attached, indicating the localities where the objects were found. In this edition, which by the way is the first issued, the several branches of the collections are dealt with generally, and we are told that "hand-books will be prepared later on for some of them," which will add greatly to the interest of the objects concerned.

THE Observatory of Zi-ka-Wei, near Shanghai, has published vol. xv. of its *Bulletin Mensuel*, for the year 1889. This Observatory is equipped with the best self-recording and other instruments, and the volume in question contains, in addition to the usual tables of hourly observations, diagrams of the mean diurnal variations, and of the tracks of typhoons, as well as comparisons of the monthly means of magnetical and meteorological observations for the year 1889, and those of the previous 17 years. An appeal was made to the missionaries of the province of Kiang-nan to record thunderstorm observations, and some interesting results are published for each month. These storms occur most frequently between noon and midnight, and generally proceed from west to east; they mostly occur in July and August; there is also a second maximum in April; they most frequently occur with a falling barometer, and are generally accompanied with rain, but very rarely with hail. The work also contains interesting general remarks upon the depressions and cyclones of the coast of China.

THE Journal of the Asiatic Society of Bengal, three numbers of which we have received, contains some interesting papers on various subjects. In No. 1, Part 2 of vol. lix. there is a paper by John Eliot on the occasional inversion of the temperature relations between the hills and plains of Northern India. Alfred Alcock, Surgeon-Naturalist to the Marine Survey, contributes a paper on observations on the gestation of some sharks and rays, made on board H.M. Indian Marine Survey steamer *Investigator*, Commander Alfred Carpenter, R.N., while from the same ship we have descriptions of seven additional new Indian Amphipods by G. M. Giles, late Surgeon-Naturalist to the Survey. Asutosh Mukhopadhyay contributes three papers, as follows:—Note on Stokes's theorem and hydrokinetic circulation; on Clebsch's transformation of the hydrokinetic equation; and on a curve of aberrancy. The supplement to No. 1 of this part consists of a catalogue of the Insecta of the Oriental region and the order of Coleoptera, family Carabidæ, by E. T. Atkinson. The third pamphlet contains the title-page, index, &c., to vol. lvii. Part 2, 1888.

THE *American Meteorological Journal* for August contains the conclusion of M. Faye's articles on Trombes and Tornadoes. The author considers that the facts adduced show (1) that there are no centripetal movements, either at the foot of trombes or tornadoes, or toward the base of cyclones; (2) that these are descending whirls with vertical axes, originated in the upper currents of the atmosphere, and follow the direction of these currents. The same journal contains the tornado prize essays. The first prize has been awarded to Lieutenant J. P. Finley. The following are some of the general results arrived at:—Tornadoes generally accompany an area of low barometer. Their progressive motion to the north-east arises from the fact that as they always form in the south-east quadrant of an area of low barometer, they must come within the influence of the general drift of the atmosphere on that side of the low barometer which is always to the north-east. A hailstorm is an incipient tornado in the cloud-region of an area of low barometer. As the area of low barometer progresses eastward, the region lying on an average about 350 miles to the south and east of the centre of the general storm, is the region within which tornadoes may be expected. Tornadoes, with hardly an exception, occur in the afternoon, just after the hottest part of the day; the destructive power of the wind increases rapidly from the circumference of the storm to its centre. The months of greatest frequency, as determined from a period of over 200 years, are April to July; the average frequency of the storms does not appear to have changed within that time. The shortest time occupied by the tornado-cloud in passing a given point varies from an instant to about twenty minutes, the average time being 74 seconds. The second prize was awarded to Mr. A. McAdie.

A NEW method of measuring the inductive power and conductivity of dielectrics has been recently described by M. Curie in the *Annales de Chimie et de Physique*; it is based on the use of an apparatus he calls the piezo-electric quartz. He has studied with it those qualities in various crystalline dielectrics; and he enunciates a *law of superposition*, which shows the independence of the effects produced by different variations of electromotive force. Quartz shows a difference of conductivity in the direction of the optic axis (where it is strong), and at right angles (where it is insensible); and this gives rise to striking phenomena. Plates parallel to the axis, and with the extremities of the axis

communicating with the earth, behave, beyond 120°, as dielectrics of zero inductive power. With prolonged heating, the conductivity along the axis quite disappears. Water plays a capital *rôle* in the conductivity of a great many dielectrics (possibly in all). With plates of baked porcelain kept moist the various types of conductivity could be reproduced. The electromotive forces of polarization of moist porous bodies may attain several hundred volts.

PRAIRIE dogs, it appears from a recent letter by Dr. Wilder to *Science*, lack the sense of distance. At Cornell University, several individuals walked off chairs, tables, and window-sills unhesitatingly. This is thought to be due to the nature of their usual habitat, a plain, with no sharper inequalities than burrows and mounds. One adult female seemed to have wonderful immunity from the ill-effects of falls: it once fell from the top of an elevator 21 feet high, and another time from a window-sill, about as high, on a granite pavement, but soon recovered. These animals respond to sudden sound by erecting the body and barking, and the nervous mechanism involved seems to be largely reflex, rapidly exhausted, but nearly or quite uncontrollable; indeed, one of those falls seems to have been due to an unguarded erection of the body on hearing a large clock strike.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. Jesser Coope, F.Z.S.; a Nightingale (*Daulias luscinia*), British, presented by Mr. J. Young, F.Z.S.; two Green Doves (*Chalcophaps indica*) from Ceylon, presented by Mrs. Thompson; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Master E. S. Forwood; two Short-tailed Wallabys (*Halmaturus brachyurus*) from Australia, received in exchange; a Brown Capuchin (*Cebus fatuellus* ♂) from Brazil, a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, a Banksian Cockatoo (*Calyptorhynchus banksii*) from New South Wales, deposited; two Red-vented Bulbuls (*Pycnonotus hæmorrhous*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on September 11 = 21h. 24m. 28s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4600	—	—	20 41 7	+30 19
(2) D.M. + 44° 3877 ...	6·7	Reddish-yellow.	21 31 51	+44 53
(3) 2 Pegasi	4·5	Yellowish-red.	21 24 58	+23 10
(4) δ Equulei...	4·5	Whitish-yellow.	21 9 6	+ 9 34
(5) α Equulei	4·0	White.	21 10 18	+ 4 45
(6) 238 Schj.	7·4	Yellowish-red.	20 10 40	−21 35
(7) S Vulpeculæ	Var.	Yellow-red.	19 43 53	+27 1

Remarks.

(1) In the General Catalogue this nebula is described as "a very remarkable object; pretty bright; considerably large; extremely irregular figure; *k* Cygni involved." The spectrum, as observed by Dr. Huggins, is a continuous one with a suspicion of an unusual brightness in the region beyond F. The excess of blue light is probably due to the radiation of some substance, most likely carbon, added to a dim and short continuous spectrum. The green flutings would in that case be masked by continuous spectrum, and the result would be a spectrum apparently continuous in the green and discontinuous in the blue. This discontinuity should be looked for in the nebula spectrum, and comparisons made with the carbon flutings.

(2) This star has a spectrum of Group II. "of extraordinary beauty," all the bands 1–9 being very wide and very dark (Dunér). It is probably a star of mean condensation, and the bright carbon flutings should therefore be well seen.

(3) The spectrum of this star may advantageously be studied in connection with the observations of stars which have a spectrum hitherto described as of the solar type. It is one of a very late stage of Group II., the distinctive dark bands being very narrow, so that the spectrum approaches one of Group III. The *lines* which are seen at this stage will in all probability be continued to Group III. stars, and will therefore serve as criteria for distinguishing between stars of Group III. and stars of Group V.

(4) Vogel writes the spectrum of this star as II.*a* (I.*a*), which means that the star is either at a late stage of Group III., or an early one of Group V. Its precise position on the "temperature-curve" may be determined by reference to the criteria mentioned in the note on (3).

(5) The spectrum of this star is one of Group IV.

(6) Although this star is far from being a faint one compared with many other stars of Group VI., very few details have been observed in its spectrum, either by Secchi or Dunér. Dunér simply states that the spectrum consists of three zones, the blue being very weak. The intensity of band 6 (λ 564) relatively to band 9 (λ 517), and other details, should be noted.

(7) This is a variable of Group II. of very small range and short period. The mean magnitude at maximum is about 8·85, and that at minimum 9·95. The mean period is about 67·8 days, and the increase to maximum is much more rapid than the decrease to minimum, the former occupying 20·6 days, and the latter 47·2. According to Dunér, the spectrum is only feebly developed, and this is exactly what it should be if Mr. Lockyer's view as to the constitution of this class of bodies be correct. The central swarm being well advanced in condensation, only revolving swarms of short period will be effective in producing changes of light, because long period swarms will pass clear of the central swarm at periastron. It is only to be expected, therefore, that a well advanced, or "feebly-developed" spectrum should be associated with a short period in variables of Group II. Under these circumstances it is not likely that bright hydrogen lines will appear at maximum, but an observation of their absence will be of value, and other variations may occur. There will be a maximum about September 15.

A. FOWLER.

OBSERVATIONS OF THE COMPANIONS TO BROOK'S COMET (V. 1889).—Mr. E. E. Barnard, in *Astronomische Nachrichten*, No. 2988, gives the physical and micrometrical observations of the companions to this comet made with the 12-inch and 36-inch refractors of the Lick Observatory, and those made elsewhere. It will be remembered that Mr. Barnard discovered two companions to Brook's comet on August 2, 1889, and two others on August 5. His remarks on the appearance of the companions, and the physical changes which they underwent from the date of discovery, until they disappeared from sight, are very important. Two of the companions seemed to undergo the same process of disintegration. Beginning with a nucleus and a tail, each became enlarged, diffused, and fainter, until it had dissipated into space. In some concluding remarks Mr. Barnard writes: "I have no doubt but that the great telescope would readily reveal more unknown nebulæ than the entire number now contained in the latest catalogue of Dreyes," and the number of unknown nebulæ incidentally found by him during these observations supports this assertion.

PARALLAX OF β ORIONIS.—In the *Observatory* for September, Dr. Gill has a note on the parallax of β Orionis. The star is situated near one corner of the nebulous area encircled by α Orionis, Lalande 11382, Lalande 11329, ω Orionis, ψ Orionis, and β Eridani, and the observations show that it has a negative parallax of about 0″·17 relative to the near parallax of the stars D.M. − 7° 997 and − 8° 1078, and therefore belongs to a more distant system. It also results from the calculations that the former star and θ Orionis are members of an immensely more remote system than the latter one. The reductions were suggested by an examination of a photograph of the region about the Orion nebula taken under the direction of Prof. E. C. Pickering.

CARL FREDERIK FEARNLEY.—The death of this eminent Norwegian astronomer occurred on the 23rd ult. at Christiania. He was born at Frederikshald on December 19, 1818. In 1844 he became attached to the Observatory of Christiania University as an assistant, and since 1861 has been the Director. He was made a Professor of Astronomy in 1857.

The deceased astronomer published many observations of planets, comets, and the sun, and directed some attention to the determination of the height of the aurora borealis. He also published a memoir on atmospheric refraction, and participated in many geodetical observations.

Fearnley's death is severely felt by those with whom he came in contact.

United States Naval Observatory, Washington.—The report of the superintendent of the U.S. Naval Observatory for the year ending June 30, 1889, has just been issued, and contains an account of the work done in each department.

The large equatorial has been employed in observing double stars and the satellites of Saturn; attention also being paid to the division on the ring and to the shadows.

Seventeen hundred observations have been made with the transit circle since October 9, 1888; of this number 68 were of the sun, 60 of the moon, 93 of the major planets, 18 of the minor planets, and 5 of Comet *e* 1888.

The 9·6-inch equatorial has been used for the identification of stars whenever necessary, and for the observations of small planets, comets, and occultation of stars by the moon. During the past year 3 comets were seen and observed whenever possible. Two nights a week this instrument is set apart for the accommodation of visitors, and permits for 1665 visitors were issued.

Assistant-Astronomer H. M. Paul, who has for the last year and a half been here observing suspected variables, has just discovered a new variable in the constellation Antlia, with a period of less than 12 hours, the shortest period yet known.

The chronometer and time service, under the charge of Lieutenant Taylor, have been doing good work. Fifty-six chronometers received from the makers, cleaned, and repaired, were tested in the temperature-room for a period of about two months. Chronometers were issued to eleven ships and one shore station, and the same number were received back.

No alteration has been made in the routine of sending out time signals, which are telegraphed every day, Sundays and holidays excepted.

In the magnetic department under the charge of Ensign Marsh, observations have been made on Tuesdays each week for the determination of the absolute horizontal intensity, and on each month they were made with the inertia cylinder attached to the magnet. Observations on the magnetic inclination, using three needles in rotation, were made every Monday and Friday. Two seismoscopes and a seismograph have been added to the stock of instruments, and they have been set up and are in good working order.

The library, which has lately been placed under the charge of Assistant-Astronomer Paul, in addition to his other duties, contains, up to June 30, 12,226 volumes and 2696 pamphlets, of which the accessions, since the last Report, have been 308 in number, 235 volumes and 73 pamphlets.

The appendix contains a report of the work done during the past year, by Prof. William Harkness, who was attached for special duty as a member of the Transit of Venus Commission, and had charge of the reductions and computations of the observations of 1874 and 1882. The result of his work is the determination of the sun's distance to be 92,455,000 miles, with a probable error of 123,400 miles.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, September 3.—Mr. Henry T. Stainton, F.R.S., in the chair.—Mr. C. Fenn exhibited and remarked on specimens of *Eupithecia satyrata*, *Eudorea ambigualis*, and *Tortrix viburnana* from Darlington.—Mr. H. Goss exhibited, on behalf of Mr. Martin S. Higgs, a remarkable variety of *Melitæa aurinia* (*artemis*), taken a few years ago, in Gloucestershire, by Mr. Joseph Merrin.—The Rev. Dr. Walker communicated some observations on the entomology of Iceland, and gave an account of his recent travels in that island. He stated that he had taken *Bombus terrestris* this year, for the first time, in the north-west of Iceland, from which quarter of the island it had not been recorded by Dr. Staudinger; he also referred to the enormous numbers of Ichneumonidæ and Diptera which he had noticed in the island. He further stated that in 1889, in the months of June and July, *Noctua conflua* was the most abundant species of Lepidoptera in Iceland; but that this year, in July and August, *Crymodes exulis* was the prevailing species, and that *Charæas graminis* and *Coremia munitata* also occurred in great numbers. In reply to a question by Mr. Stainton, Dr. Walker said that the flowers chiefly frequented by the humble-bees were those of a small species of white Galium (probably *Galium saxatile*) and *Viola tricolor*. Dr. Walker also read notes on *Calathus melanocephalus* collected in Iceland and the Faroe Isles in June and July 1890. Messrs. M'Lachlan, Stainton, Jenner Weir, Stevens, Jacoby, Lewis, and others took part in the discussion which ensued.—Mr. Arthur G. Butler communicated a paper entitled "Further Notes on the Synonymy of the Genera of Noctuites."

Paris.

Academy of Sciences, September 1.—M. Duchartre in the chair.—MM. G. Seguy and Verschaffel gave a description of a photometer founded upon the principle of Crooke's radiometer.—M. Faye announced the publication of the *Connaissance des Temps* for 1892.—Influence of altitude on the development of plants, by M. Gaston Bonnier. The author has observed that the amount of carbon dioxide decomposed by plants increases with the altitude. Plants cultivated in an Alpine climate undergo a modification of their functions such that the chlorophyllian assimilation and transpiration are augmented, whilst respiration and transpiration in the dark appear little modified or slightly diminished.—On the chlorophyllian assimilation of trees with red leaves, by M. Henri Jumelle. The author has investigated the difference of physiological functions in the leaves of the green and red type of such trees as the beech, sycamore, elm, &c. He finds: (1) in trees with red or coppery-coloured leaves the chlorophyllian assimilation is always more feeble than in trees of the same kind having green leaves; (2) the intensity in the copper beech and purple sycamore is only about one-sixth that of the ordinary types of the same trees.—On the oospores formed by the fusion of multi-nuclei sexual elements, by M. P. A. Dangeard. The author has studied the sexual reproduction of plants of a lower order.—First observations on the cyclone of August 19 in Jura, by M. Bourgeat. A circumstantial account of the St. Claud cyclone is given. It is noted that the lower parts of the region visited by the storm suffered the most, that the direction of the gyratory movement was opposite to that of the hands of a watch, that the velocity of translation was about 1 kilometre a minute, and that the zone ravaged had a breadth from 500 to 1000 metres.—On the signification of the word "cyclone," by M. H. Faye. It was remarked by M. Faye that although all the papers and the author of the preceding note had named the St. Claud storm a cyclone, yet really it was a tornado. The difference between the two phenomena was pointed out, it being noted that the base of a cyclone is considerably larger than that of the St. Claud storm, and has a well-defined region of calm at its centre.

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THURSDAY, SEPTEMBER 18, 1890.

THE ABORIGINES OF TASMANIA.

The Aborigines of Tasmania. By H. Ling Roth. (London: Kegan Paul, Trench, Trübner, and Co., 1890.)

MR. H. L. ROTH has written an honest, unpretentious, and therefore most useful book on "The Aborigines of Tasmania." He gives us on pp. 2–8 a very complete bibliography of all works treating of his subject, and he then proceeds to place before us the quintessence distilled from that little library. Why he should have printed two hundred copies only of his work, is difficult to understand, and does not speak well for the study of anthropology. No serious student of human palæontology can be without this book, and we should have supposed that the public at large also would have much preferred a trustworthy description of the life and manners of this now extinct race to the ever-varying theories of what a savage is supposed to have been or not to have been, to have done or not to have done, which abound in some of the most popular works on anthropology and sociology. In the fourteen chapters of his book Mr. Roth treats of the country, the form and size of its inhabitants, the psychology of the natives, their wars, their knowledge of fire, hunting, and fishing, their nomadic life, their personal habits, their scientific and artistic acquirements, their manufactures, their trade, their customs, good and bad, their language, their osteology, and lastly their origin.

It would be impossible to give an idea of the wealth of information on all these subjects which Mr. Roth has rendered accessible in this volume. It is well arranged, and all his statements can readily be verified, for he always give his references, and a complete index renders its use easy at all times. The illustrations also show great care and cleverness.

Perhaps not the least important lesson which anthropologists might learn from this book is the extremely uncertain character of the accounts which visitors of Tasmania, and even persons long settled in the island, have given us of its inhabitants. This is a sore point with the students of sociology, but it is high time that it should be thoroughly probed. We shall confine our remarks to one subject only, the Tasmanian religion, and, with the help of Mr. Roth, we shall undertake to show that there is not one essential point in the religion of the Tasmanians on which different authorities have not made assertions diametrically opposed to each another.

No Religion.—Nothing staggers a savage—perhaps even an educated man—so much as when he is asked what his religion is. No wonder that many of the Tasmanians, when asked that question, answered, with a broad grin, "Don't know." What should we say if we were asked whether we believed in *Raegoo Wrapper* or *Namma?* Widowson, however, assures us that the Tasmanians had really no religion at all. "It is generally supposed," he says, "that they have not the slightest idea of a Supreme Being." Briton adds: "They do not appear to have any rites or ceremonies, religious or otherwise."

Dualism.—That the Tasmanians were Dualists, believing, like the followers of Zoroaster, in a good and an evil spirit, is attested by numerous authorities. Leigh says:—"Their notions of religion are very obscure. However, they believe in two spirits: one, they say, governs the day, whom they call the good spirit; the other governs the night, and him they think evil. To the good spirit they attribute everything good, and to the evil spirit everything hurtful." Jeffreys says:—"They have but a very indistinct notion of their imaginary deity, who, they say, presides over the day, an evil spirit making its appearance in the night. This deity, whosoever it is, they believe to be the giver of everything good." He adds, however, that they appear to acknowledge no more than one God, thus furnishing an exact parallel to the Parsis, who, though they admit two spirits, acknowledge Ormasd only as their true god. Milligan confirms this view. He admits that the Tasmanians believed in many spirits, but he adds that "they considered one or two spirits to be of omnipotent energy, though they do not seem to have invested even these last with attributes of benevolence." Robinson maintains that "they were fatalists (whatever that may mean in their language), and that they believed in the existence both of a good and evil spirit. The latter they called *Raegoo Wrapper*, to whom they attributed all their afflictions, and they used the same word to express thunder and lightning."

Nature-Gods.—That the Tasmanians derived some of their ideas of the godhead from the great phenomena of Nature we have seen already from their identifying day and night with their good and evil spirits. Thunder and Lightning were their names for the evil spirit, or their devil, as some observers call him. Besides day and night, thunder and lightning, the moon also is mentioned as an object of their worship. Thus, Lloyd tells us "that it was customary among the aborigines to meet at some time-honoured trysting-place at every full moon, a period regarded by them with most profound reverence." Indeed, he adds, "judging from their extraordinary gestures in the dance, the upturned eye and outstretched arm, apparently in a supplicating spirit, I have been often disposed to conclude that the poor savages were invoking the mercy and protection of that planet as their guardian deity."

Devil-worship.—We now come to the testimony in support of an exclusive devil-worship. Davies asserts that the aborigines certainly believed in the existence of an evil spirit, called by some tribes Namma, who has power by night. Of him they are much afraid, and never will willingly go out in the dark. But, he adds, "I could never make out that they believed in a good deity, for although they spoke of one, it struck me that it was what they had been told; they may, however, believe in one who has power by day."

Backhouse speaks in the same hesitating tone:—

"These people," he says, "have received a few faint ideas of the existence and superintending providence of God; but they still attribute the strong emotions of their minds to the devil, who, they say, tells them this or that, and to whom they attribute the power of prophetic communication. It is not clear that by the devil they mean anything more than a spirit; but they say he lives in their breasts, on which account they shrink from having the breast touched."

If we could fully trust this statement, and it is confirmed to some extent by Horton, it would be most important as showing the germs of moral ideas among the Tasmanians. To believe in a devil, not simply with horns and hoofs, but living within our own hearts, is an advance which, even in Europe, has as yet been made by a small minority only. The majority of Tasmanians evidently represented their devil in a more material form. Thus Dove says that, "while they had no term in their native language to designate the Creator of all things, they stood in awe of an imaginary spirit who was disposed to annoy and hurt them. The appearance of this malignant demon in some horrible form, was especially dreaded in the season of night."

Monotheism.—But while some authorities seem inclined to reduce the Tasmanian religion to a belief in a devil only, others seem to look upon it as almost monotheism. Thus Jeffreys, though he admits that the Tasmanians (like most Agnostics) have a very indistinct notion of their imaginary deity, relates that they have a kind of song which they chant to him. He knows that they believe in a good and an evil spirit, but he adds, that they believe the good spirit to be the giver of everything good, and that they do not appear to acknowledge any more than one God. That good spirit had, as we saw, no name, and this, which to some may seem to be a serious defect, is again a feature which the Tasmanian religion shares in common with the religion of far more advanced races.

Spirit-worship.—Those who hold that religion began everywhere with a belief in spirits may likewise find some support for their theory in the accounts given of the Tasmanians. Henderson states:—

"A common belief prevails in Tasmania and New South Wales regarding the existence of inferior spirits, who conceal themselves in the deep woody chasms during the day, but who wander forth after dark, with power to injure or even to destroy. Their rude encampments are frequently alarmed by these unearthly visitors, whose fearful moanings are at one time borne on the midnight breeze, and at another are heard mingling with the howling tempest."

This does not prove as yet that these spirits are always believed to be the spirits of the departed. Milligan, however, after telling us that the Tasmanians were polytheists—that is, that they believed in guardian angels or spirits, and in a plurality of powerful but generally evil-disposed beings, inhabiting crevices and caverns of rocks, and making temporary abode in hollow trees and solitary valleys, adds "that the aborigines were extremely superstitious, believing most implicitly in the return of the spirits of their departed friends and relations to bless or injure them, as the case might be. To their guardian spirits, the spirits of their departed friends or relations, they gave the generic name *Warrawah*, an aboriginal term signifying shade, shadow, ghost, or apparition."

Immortality of the Soul.—One point on which nearly all witnesses seem to agree is the belief of the Tasmanians in the immortality of the soul. They evidently had not yet advanced so far as to be able to doubt it. Milligan had ascertained that the aborigines of Tasmania, previous to their intercourse with Europeans, distinctly entertained the idea of immortality, as regarded the soul or spirit of man. Robinson, who was present at the burning of a dead body, received the following explanation from a native:—"Native dead, fire; goes road England, plenty natives England." What he meant to say was that when a black fellow was dead and had been burnt, he went to England, where there are many black fellows. The name of England, *Dreany*, as a distant country, and the home of white people, had become with them the name of a new Elysium. Others expected to reappear on an island in the Straits, and to jump up white men. They anticipated in another life the full enjoyment of what they coveted in this. Backhouse declares that they have some vague ideas of a future existence. Dove remarks that they were persuaded of their being ushered by death into another and happier state, and he considers this as almost the only remnant of a primitive religion which maintained a firm abode in their minds. However, as if to show that no account of their religious persuasions should go uncontradicted, Davies remarks that, "though it is hard to believe that the natives have no idea of a future state, yet from every inquiry, both from themselves and from whites most conversant with them, I have never been able to ascertain that such a belief exists."

Prayers.—Of course those who maintain that the Tasmanians have no religion, maintain at the same time that they have no kind of worship, no sacrifices, no prayers. But Leigh tells us that, "when any of the family are on a journey, they are accustomed to sing to the good spirit for the purpose of securing his protection over their absent friends, and that they may be brought back in health and safety." Jeffreys relates that it frequently happens that the sealers . . . are compelled to leave their native women for several days together. On these occasions these affectionate creatures have a kind of song, which they chant to their imaginary deity.

Charms.—It is known also that the Tasmanians carried charms, mostly a bone or even the skull of their relatives and friends. In some cases they ascribed healing powers to these bones, or at all events they put them by their side or on their head when they felt sick. This after all is no more than our preserving a lock of hair, and looking at it when we are in trouble or grief.

Negative evidence is always less trustworthy than positive. Still it may be taken for what it is worth, that observers seem never to have discovered idols (p. 69), totems (p. 75), or fetishes, among the natives of Tasmania.

Such is the nature of the evidence bearing on the religious ideas of the Tasmanians, which Mr. Roth has collected so carefully and so conscientiously. Nothing can be more full of contradictions, more doubtful, more perplexing. Yet with such materials our best anthropologists and sociologists have built up their systems.

The Tasmanians, being reputed the lowest of savages, were represented as the children of Nature, and whatever the children of Nature were supposed to have been, when emerging from a purely animal into a more or less human state, the Tasmanians and other savages were called up as witnesses to confirm every kind of psychological speculation.

We saw that there is hardly any kind of religion which could not be proved to have been the original religion of the Tasmanians. How then can we wish for more

pliant witnesses in support of any theory as to what the primordial religion of mankind must have been? If it were desired to prove that, prior to the advent of Europeans, they were atheists, without any religious ideas or ceremonial usages, we have several excellent witnesses to prove it. We could prove equally well that they believed in a devil only, that they were Dualists, believing in a good and an evil spirit, that they had deified the powers of Nature, that they had arrived at a belief in one God, that they were polytheists, that they believed in ghosts, in the return of the spirits of their friends, in the immortality of the soul, and in the efficacy of prayers and charms. Nay, if it were desired to produce perfectly unprejudiced evidence in favour of the descent of man from some higher animal, Lord Monboddo might have appealed to the Tasmanians. For, according to Mr. Horton, they believed "that they were formed with tails and without knee-joints, by a benevolent being, and that another descended from heaven, and compassionating the sufferers, cut off their tails, and with grease softened their knees."

Dr. E. B. Tylor, F.R.S., the Reader in Anthropology at Oxford, has written a short preface, in which he expresses his general approval of the work.

F. MAX MÜLLER.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

British Association Procedure.

THERE is one point on which I am unable to agree with Prof. Tilden's letter in your issue of September 4 (p. 456), viz. that concerning the work and constitution of the Sectional Committees. I can only speak in terms of Section A, but I believe that whatever cynical doubt may be expressed as to the utility of the proceedings of the section there is none as to the utility of its proceedings in Committee. Here matters of moment are brought forward, suggestions made, new researches encouraged or the reverse; and here, as Mr. Shenstone implies, younger members become acquainted with those whom they have long revered at a distance. A sectional committee is not, and should not be, a small executive body, but a large, representative, and suggestive body comprising all the real workers in the particular subject present at the year's gathering, and by no means excluding those younger men who, though now retiring and inconspicuous, will have at some future time to take a prominent place.

Prof. Tilden speaks, however, of the *demand* for election upon the sectional committees.

If there is anything of this sort, and I believe that to some extent there is, it is an abuse to be checked with vigour.

I should like to propose a general agreement that any direct demand or solicitation to be placed on any committee should be accepted as at once disqualifying for that year. But all the more would it be incumbent on accustomed members to see that no real original worker was accidentally excluded from the healthy and stimulating conference with his seniors which these meetings may afford.

OLIVER J. LODGE.

The Mode of Observing the Phenomena of Earthquakes.

Forwarded by Dr. John Marshall.

HAVING seen in NATURE, of the 28th ult. (p. 414), your remarks, on the uncertainty of the evidence to be obtained from a narration of the subjective impression of movements of the earth and surrounding objects, in obtaining information with regard to earthquakes, and that you also remark that, "possibly, some evidence on this subject might even now be obtained," I venture to say that I was in a first storey room of Wickham Place, near Witham, Essex, during the earthquake that occurred somewhat severely in part of Essex a few years back; and that I was sitting against a partition wall, facing a window to the east, during the whole time of its duration. A hill about $1\frac{1}{2}$ miles away formed the horizon, the outline of which passed across this window about half way up, from my point of view. I saw this outline apparently rise up to the top of the window, and sink down again, a displacement which, if it had been due to the movement of the hill itself, must have meant a great deal; but although this was really due, no doubt, to the motion of the house itself, yet the appearance was so deceptive that it produced entirely the idea, at the time, on my senses, that it was the hill that moved.

At Guy's Hospital last year, about 14 months ago, while I was in bed, at somewhere about eight o'clock, I fancy, I felt nothing, but saw the other parts of the building, through the windows, sway slowly, and the sight of it gave me a more or less dizzy feeling. There was a friend sitting on the bed at the time, but he felt nothing. Until I drew his attention to the fact that the bed curtains were swaying, he saw nothing of it.

These impressions make me think that such are of no value in a house except to determine very slight shocks.

I made notes at the time of these points, but they are not to hand just now. I scarcely think such evidence can be of any use to you, but on the chance that it may be, I send it.

HAROLD G. DIXON.

Nelson House, Shanklin, Isle of Wight, September 3.

THE BRITISH ASSOCIATION.

SECTION F.

ECONOMIC SCIENCE AND STATISTICS.

OPENING ADDRESS BY PROF. ALFRED MARSHALL, M.A., F.S.S., PRESIDENT OF THE SECTION.

Some Aspects of Competition.

I UNDERSTAND that the function of an opening address to a section of this Association is to give an account of the advances made in some part of the field of study with which that section is specially concerned. The part of our field to which I would direct your attention to-day is the action of competition. We cannot, in the short space of time allotted to us, make an adequate study of the progress that has been made even in this part of our field; but we may be able to go some way towards ascertaining the character of the changes that are going on in our own time in the mode of action of competition, and in the attitude of economists towards it.

I do not now speak of changes in the moral sentiments of economists with regard to competition—though these, also, are significant in their way—but of changes in their mental attitude towards it, and in the way in which they analyse and reason about its methods of action. Of these changes, the most conspicuous and important is the abandonment of general propositions and dogmas in favour of processes of analysis and reasoning carefully worked out, and held ready for application to the special circumstances of particular problems relating to different countries and different ages, to different races and different classes of industry.

This movement may, perhaps, best be regarded as a passing onward from that early stage in the development of scientific method, in which the operations of Nature are represented as conventionally simplified for the purpose of enabling them to be described in short and easy sentences, to that higher stage in which they are studied more carefully, and represented more nearly as they are, even at the expense of some loss of simplicity and definiteness, and even apparent lucidity. To put the same thing in more familiar words, the English economists of fifty years ago were gratified, rather than otherwise, when some faithful henchman, or henchwoman, undertook to set forth their doctrines in the form of a catechism or creed; and the economists of to-day abhor creeds and catechisms. Such things are now left for the Socialists.

It has, indeed, been an unfortunate thing for the reputation of the older economists, that many of the conditions of England

at the beginning of this century were exceptional, some being transitional, and others, even at the time, peculiar to England. Their knowledge of facts was, on the average, probably quite as thorough as that of the leading economists of England or Germany to-day, though their range was narrow. Their thoroughness was their own, the narrowness of their range belonged to their age; and though each of them knew a great deal, their aggregate knowledge was not much greater than that of any one of them, because there were so few of them, and they were so very well agreed. In these matters we economists of to-day have the advantage over them.

Their agreement with one another made them confident; the want of a strong opposition made them dogmatic; the necessity of making themselves intelligible to the multitude made them suppress even such conditioning and qualifying clauses as they had in their own minds: and thus, although their doctrines contained more that was true, and new, and important than those promulgated by almost any other set of men that have ever lived—doctrines for which they will be gratefully remembered as long as the history of our century retains any interest—yet, still, these doctrines were so narrow and inelastic that, when they were applied under conditions of time and place different from those in which they had their origin, their faults became obvious and created a reaction against them.

Perhaps the greatest danger of our age is that this reaction may be carried too far, and that the great truths which lie embedded in these too large utterances may be neglected because they are not new, and men are a little tired of them; and because they are associated with much that is not true, and which has become, not altogether unjustly, repugnant to men's sentiments.

The most important instances of this kind are, perhaps, to be found in connection with the relations between competition and combination in trade and industry. But I will first refer briefly to the relations between protection and free trade in foreign commerce, because these have a longer and more fully-developed history.

It is a constant source of wonder to Englishmen that protection survives and thrives, in spite of the complete refutations of protectionist arguments with which English economists have been ready to supply the rest of the world for the last fifty years or more. I believe that these refutations failed chiefly because some of them implicitly assumed that whatever was true as regards England, was universally true; and if they referred at all to any of the points of difference between England and other countries, it was only to put them impatiently aside, without a real answer to the arguments based on them. And further, because it was clearly to the interests of England that her manufactures should be admitted free by other countries, therefore, any Englishman who attempted to point out that there was some force in some of the arguments which were adduced in favour of protection in other countries, was denounced as unpatriotic. Public opinion in England acted like the savage monarch who puts to death the messenger that comes running in haste to tell him how his foes are advancing on him; and when John Stuart Mill ventured to tell the English people that some arguments for protection in new countries were scientifically valid, his friends spoke of it in anger—but more in sorrow than in anger—as his one sad departure from the sound principles of economic rectitude. But killing the messengers did not kill the hostile troops of which the messengers brought record; and the arguments which the Englishmen refused to hear, and therefore never properly refuted, were for that very reason those on which protectionists relied for raising a prejudice in the minds of intelligent and public-spirited Americans against the scientific soundness and even the moral honesty of English economics.

The first great difficulty which English economists had, in addressing themselves to the problems of cosmopolitan economics, arose from the fact that England was an old country—older than America in every sense, and older than the other countries of Europe in this sense, that she had accepted the ideas of the new and coming industrial age more fully and earlier than they did. In speaking of England, therefore, they drifted into the habit of using, as convertible, the two phrases—"the commodities which a country can now produce most easily," and "the commodities which a country has the greatest natural advantages for producing," that is, will always be able to produce most easily. But these two phrases were not approximately convertible when applied to other countries; and when List and Carey tried to call attention to this fact, Englishmen did little more than repeat old arguments, which implicitly assumed that New England's inability to produce cheap calico had the same foundation in natural laws as her inability to produce cheap oranges. They refused fairly to meet the objection that arguments which prove that nothing but good can come from a constant interchange of goods between temperate and tropical regions, do not prove that it is for the interest of the world that the artisans who are fed on American grain and meat should continue always to work up American cotton for American use three thousand miles away. Finding that their case was not fairly met, the protectionists naturally thought it stronger than it was, and honestly exaggerated it in every way. One of my most vivid recollections of a visit I made in 1875, to study American protection on the spot, is that of Mr. Carey's splendid anger, as he exclaimed that foreign commerce had made even the railways of America run from east to west, rather than from north to south.

England had passed through the stage of having to import her teachers from other lands. But her genius for freedom had attracted to her shores the pick of the skilled artisans of the world; she had received the best lessons from the best instructors, and seldom paid them any fee, beyond a safe harbour from political and religious persecution. And modern Englishmen could not realize, as Americans, and even Germans, could, fifty years ago the difficulties of a manufacturer taking part in starting a new industry, when he came to England to beg or steal a knowledge of the trade, and to induce skilful artisans to come back with him. He seldom got the very best; for they were sure of a comfortable life at home, and were perhaps not without some ambition of rising to be masters themselves. He had to pay their travelling expenses, and to promise them very high wages; and when all was done, they often left him to become the owners of the 160 acres allotted to every free settler; or, the bitterest pill of all, they sold their skill to a neighbouring employer who had been looking on at the experiment, and, as soon as it showed signs of prosperity, stepped in, improved on the first experiments, and reaped a full harvest on a soil that had been made ready by others.

Again, the pioneer manufacturer had to bring over specialized machinery, and specialized skill to take care of it. If any part went wrong, or was superseded, the change cost him ten times as much as his English competitor. He had to be self-sufficing: he could get no help from the multitude of subsidiary industries, which in England would have lent him aid at every turn. He had a hundred pitfalls on every side: if he failed, his failure was full of lessons to those who came after; if he succeeded, the profits to himself would be trivial, as compared with those to his country. When he told the tale of his struggles, every word went home to his hearers; and when the English economists, instead of setting themselves to discover the best method by which his country might help him in his experiment, said he was flying in the face of Nature, and called him a selfish schemer for wanting any help at all, they put themselves out of court.

But the failure of English economists to allow for the special circumstances of new countries did not end here. They saw that protective taxes in England had raised the price of wheat by their full amount (because the production of wheat obeys the law of diminishing return; and in an old country, such as England, increased supplies could be raised only at a more than proportionately increased cost of labour); that the high price of bread had kept a large part of the population on insufficient rations; that it had enriched the rich at the expense of a much greater loss to the rest of the nation; and that this loss had fallen upon those who were unable to lose material wealth without also losing physical, and even mental and moral strength; and that even those miseries of the overworked factory women and children, which some recent German writers have ascribed exclusively to recklessness of manufacturing competition in its ignorant youth, were really caused chiefly by the want of freedom for the entry of food. They were convinced, rightly, as I believe, that the benefits claimed for protection in England were based, without exception, on false reasoning; and they fought against it with the honest, but also rather blind, energy of a religious zeal.

Thus they overlooked the fact that many of those indirect effects of protection which aggravated then, and would aggravate now, its direct evils in England, worked in the opposite direction in America. For, firstly, the more America exported her raw produce in return for manufacture, the less the benefit she got from the law of increasing return as regards those goods

that she manufactured for herself; and thus her case was contrasted with England, who could manufacture them more cheaply for her own use the more of her manufactures she sent abroad to buy raw produce; and for this and other reasons a protective tax did not nearly always raise the cost of goods to the American consumer by its full amount. And, secondly, protection in America did not, as in England, tax the industrial classes for the benefit of the wealthy class of landlords. On the contrary, in so far as it fell upon the exporters of American produce, it pressed on those who had received large free gifts of public land; and there was no *primâ facie* injustice in awarding to the artisans, by special taxation, a small part of the fruits of that land, the direct ownership of which had not been divided between farmers and artisans, as it equitably might have been, but had been given exclusively to the former.

I have touched on but a few out of many aspects of the problem. But perhaps I may stop here, and yet venture to express my own opinion on the controversy. It is, that fifty years ago it might possibly have been not beyond the powers of human ingenuity to devise schemes of protection which would, on the whole, be beneficial to America, at all events if one regarded only its economic and neglected its moral effects; but that the balance has turned strongly against protection long ago. In 1875 I walked up and down some of the streets of nearly all the chief American cities and said to myself as I went—The adoption of free trade, so soon its first disturbances were over, would strengthen this firm, and weaken that; and I tried to strike a rough balance of the good and evil effects of such a change on the non-agricultural population. On the whole, it seemed to me the two were about equally balanced. Taking account, therefore, of the political corruption which necessarily results from struggles about the tariff in a democratic country, and taking account also of the interests of the agricultural classes, I settled in my own mind the question as to which I had kept an open mind till I went to America, and decided that, if an American, I should unhesitatingly vote for free trade. Since that time the advantages of protection in America have steadily diminished, and those of free trade have increased; I can see no force in Prof. Patten's new defence of protection as a permanent policy. I have already implied that I believe that many of those arguments that tell in favour of protection as regards a new country tell against it as regards an old one. Especially for England a protective policy would, I believe, be an unmixed and grievous evil.

But this expression of my own opinion is a digression. My present purpose in discussing protection is to argue that, if the earlier English economists had from the first studied the conditions of other countries more carefully, and abandoned all positions that were at all weak, they could have retained the controversy with their opponents within those regions where they had a solid advantage. They would thus have got a more careful hearing when they claimed that, even though labour migrated more freely between the west and the east of America than between England and America, yet it was unwise to spend so much trouble on protecting the nascent industries of the East against those of England, and none on protecting the nascent industries of the west against those of the east; or, again, when they urged that, the younger an industry was, and the more deeply it needed help, the more exclusively would its claims have to stand on its own merits; while its older and sturdier brothers could supplement their arguments by a voting power which even the most honest politicians had to respect, and by a power of corruption which would tend to make politics dishonest.

Had the English economists been more careful and more many-sided, they would have gradually built up a prestige for honesty and frankness, as well as for scientific thoroughness, which would have inclined the popular ear to their favour, even when their arguments were difficult to follow. Intellectual thoroughness and sincerity is its own reward; but it is also a prudent policy when the people at large have to be convinced of the advisability of a course of action against which such plausible fallacies can be urged as that "protection increases the employment of domestic industries," or that "it is needed to enable a country in which the rate of wages is generally high to carry on trade with another in which it is generally low." The arguments by which such fallacies can be opposed have an almost mathematical cogency, and will convince, even against his will, any one who is properly trained for such reasonings. But the real nature of foreign trade is so much disguised by the monetary transactions in which it is enveloped, that a clever sophist has a hundred opportunities of throwing dust in the eyes of ordinary people, and especially the working classes, when urging the claims of protection as affording a short cut to national prosperity; and, to crown all, he contrasts America's prosperity with English prophecies of the ruin that protection would bring on her.

It is true that Ricardo himself, and some of those who worked with him, were incapable of supposing that a doctrine can be made more patriotic by being made less true; and, so far as their limits went, they examined the good and evil of any proposed course, and weighed the good and evil against one another in that calm spirit of submissive interrogation with which the chemist weighs his materials in his laboratory. But they were few in number, and their range of inquiry was somewhat narrow; while many of those Englishmen who were most eager to spread free trade doctrines abroad had not the pure scientific temper.

Now at length, however, there seems to be the dawn of a brighter day in the growth of large numbers of many-sided students, in England and other countries, and notably in America itself, where the problems of protection can be studied to most advantage—students who are not, indeed, without opinions as to what course it is most expedient to follow practically, but who are free from party bias, and have the true scientific delight in ascertaining a new fact or developing a new argument, simply because they believe it to be new and true, and who welcome it equally whether it tells for or against the practical conclusion which, on the whole, they are inclined to support.

But I must leave the subject of competition from outside a nation, and pass to that of competition within. Here the past counts for less; the present and the future have to work for themselves without very much direct aid from experience. For, rapid as are the changes which the last few years have seen in the conditions of foreign trade, those which are taking place in the relations of different groups of industry within a country are more rapid still, and more fundamental. The whirligig of Time brings its revenges. It was to England's sagacity and good fortune in seizing hold of those industries in which the law of increasing return applies most strongly that she owed in a great measure her leading position in commerce and industry. Time's revenge was that that very law of increasing return furnished the chief motive to other countries, and especially America, to restrict their commerce with her by protective duties to home industries. And Time's counter-revenge is found in this—that England's free trade has prevented the law of increasing return from strengthening combinations of wealthy manufacturers against the general weal here to the same extent as it has in countries in which protection has prevailed, and notably America.

The problem of the relations between competition and combination is one in which differences of national character and conditions show themselves strongly. The Americans are the only great people whose industrial temper is at all like that of the English; and yet even theirs is not very like. Partly because of this difference of temper, but more because of the differences in the distribution of wealth and in the physical character of the two countries, the individual counts for much more in American than in English economic movements. Here, few of those who are very rich take a direct part in business; they generally seek safe investments for their capital; and again, among those engaged in business the middle class predominates, and most of them are more careful to keep what they have, than eager to increase it by risky courses. And lastly, tradition and experience are of more service and authority in an old country than in one which, like America, has not yet even taken stock of a great part of her natural resources, and especially those mineral resources, the sudden development of some of which has been the chief cause of many recent dislocations of industry.

In England, therefore, the dominant force is that of the average opinion of business-men; and the dominant form of association is that of the joint-stock company. But in America the dominant force is the restless energy and the versatile enterprise of a comparatively few very rich and able men, who rejoice in that power of doing great things by great means that their wealth gives them; and who have but partial respect for those who always keep their violins under glass cases. The methods of a joint-stock company are not always much to their mind; they prefer combinations that are more mobile, more elastic, more adventurous, and often more aggressive. For some

purposes they have to put up with a joint-stock company; but then they strive to dominate it, not be dominated by it. Again, since distances in America are large, many local monopolies are possible in America which are not possible in England; in fact, the area of a local monopoly there is often greater than that of the whole of England. A local coal combination, for instance, means quite a different thing there from what it does in England, and is more powerful every way.

Again, partly, but not solely, because they are so much in the hands of a few wealthy and daring men, railways, both collectively and individually, are a far greater power in America than in England. America is the home of the popular saying that, if the State does not keep a tight hand on the railways, the railways will keep a tight hand on the State; and many individual railways have, in spite of recent legislation, a power over the industries within their territories such as no English railway ever had: for the distances are great, and the all-liberating power of the free ocean befriends America little.

It is this change of area that is characteristic of the modern movement. In Adam Smith's time England was full of trade combinations, chiefly of an informal kind, indeed, and confined to very narrow areas; but very powerful within those areas, and very cruel. Even at the present day the cruellest of all combinations in England are, probably, in the trades that buy up small things, such as fish, and dairy and garden produce in detail, and sell them in retail; both producers and consumers being, from a business point of view, weak relatively to the intermediate dealers. But even in these trades there is a steady increase in the areas over which such combinations and partial monopolies extend themselves. New facilities of transport and communication tell so far on the side of the consumer, that they diminish the *intensity* of the pressure which a combination can exert; but, at the same time, they increase the *extension* of that pressure, partly by compelling, and partly by assisting, the combination to spread itself out more widely. And in England, as in other Western countries, more is heard every year of new and ambitious combinations; and of course many of them remain always secret.

But it is chiefly from America that a cry has been coming with constantly-increasing force for the last fifteen years or more, that in manufactures free competition favours the growth of large firms with large capitals and expensive plants; that such firms, if driven into a corner, will bid for custom at any sacrifice; that, rather than not sell their goods at all, they will sell them at the prime cost—that is, the actual outlay required for them, which is sometimes very little; that, when there is not enough work for all, these manufacturers will turn their bidding recklessly against one another, and will lower prices so far that the weaker of them will be killed out, and all of them injured; so that when trade revives they will be able, even without any combination among themselves, to put up prices to a high level; that these intense fluctuations injure both the public and the producers; and the producers being themselves comparatively few in number, are irresistibly drawn to some of those many kinds of combinations to which, nowadays, the name trust is commonly, though not quite accurately, applied; and that, in short, competition burns so furiously as to smother itself in its own smoke. It is a Committee of the American Congress that reports that "combination grows out of, and is the natural development of, competition, and that in many cases it is the only means left to the competitors to escape absolute ruin."

The subject is one on which it would be rash to speak confidently. We of this generation, being hurried along in a whirl of change, cannot measure accurately the forces at work, and it is probable that the best guesses we can make will move the smiles of future generations; they will wonder how we could have so much over-estimated the strength of some, and underestimated the strength of others. But my task is to try to explain what it is that economists of this generation are thinking about competition in relation to combination; and I must endeavour to reproduce their guesses, hazardous though this may be.

To begin with, I think that it is the better opinion that popular rumour, going now as ever to extremes, has exaggerated some features of the movement towards combination and monopoly, even in America. For instance, though it is said that there are a hundred commodities the sale of which in America is partly controlled by some sort of combination, many of these combinations turn out to be of small proportions, and others to be weak and loose. Again, the typical instances which are insisted on by those who desire to magnify the importance of the movement are nearly always the same, and they have all had special advantages of more or less importance.

This is specially true of the only trust which can show a long record of undisputed success on a large scale—the Standard Oil trust. For, firstly, the petroleum in which it deals comes from a few of Nature's storehouses, mostly in the same neighbourhood: and it has long been recognized that those who can get control over some of the richest natural sources of a rare commodity are well on their way towards a partial monopoly. And, secondly, the Standard Oil Trust has many of those advantages which have been long recognized as enabling large railway companies to get the better of their smaller neighbours; for, directly or indirectly, it has in some measure controlled the pipe lines and the railways which have carried its oil to the large towns and to tidal water.

On the other hand, we must remember that the future of a young and vigorous movement is to be measured, not so much by what it has achieved, as by what it has learnt; and that every unsuccessful attempt to hold together a trust has been a lesson as to what to avoid, taught to men who are wonderfully quick to learn. In particular, it is now recognized that a very large portion of the failures in the past have been due to attempts to charge too high a price; that this high price has tempted those on the inside to break faith, and has tempted those on the outside to start rival works, which may bleed the trust very much unless it consents to buy them up on favourable terms; and, lastly, that this high price irritates the public: and that, especially in some States, public indignation on such matters leads to rapid legislation that strikes straight at the offenders, with little care as to whether it appears to involve principles of jurisprudence which could not be applied logically and consistently without danger. The leaders in the movement towards forming trusts seem to be resolved to aim in the future at prices which will be not very tempting to any one who has not the economies which a large combination claims to derive, both in producing and in marketing, from its vast scale of business and its careful organization; and to be content with putting into their own pockets the equivalent of these economies in addittion to low profits on their capital. There are many who believe that combinations of this kind, pursuing a moderate policy, will ultimately obtain so great a power as to be able to shape, in a great measure, the conditions of trade and industry.

It may be so, but these eulogists of trusts seem to claim for them both that individual vigour, elasticity, and originating force which belong to a number of separate firms, each retaining a true autonomy, and that strength and economy which belong to a unified and centralized administration. Sanguine claims of this kind are not new; they have played a great part in nearly all the bold schemes for industrial reorganization which have fascinated the world in one generation after another. But in this, their latest form, they have some special features of interest to the economic analyst.

They have a certain air of plausibility, for the organizers of trusts claim that they see their way to avoiding the weak points in ordinary forms of combination among traders, which consist in the fact that their agreements can generally be evaded without being broken. For instance, the most remarkable feature in the history of English railways during the present generation is, not their tendency to agree on the fares and freights to be charged over parallel lines—for that has long been a foregone conclusion—it is the marvellously effective competition for traffic which such railways have maintained, both of a legitimate kind, by means of improved conveniences offered to the public as a whole, or of an illegitimate kind, by means of those special privileges to particular traders which we are now, at last, seriously setting ourselves to stop by law.

It is difficulties of this kind which the modern movement towards trusts aims specially at overcoming. Trusts have very many forms and methods, but their chief motive in every case is to take away from the several firms in the combination all inducements to compete by indirect means with one another; and in every case the chief instrument for this purpose is some plan for pooling their aggregate receipts, and making the gains of each depend on the gains of all, rather than on the amount of business it gets for itself. But here the dilemma shows itself. If each establishment is left to its own devices, but has very little to lose by bad management, it is not likely long to remain well managed, and anyhow the trust does not gain much of the special economy resulting from production on a very large scale.

For this a partial remedy can sometimes be found in throwing as much of its work as possible on to those establishments which are best situated, have the best and most recent appliances and the ablest management, and, perhaps, closing entirely some of the others. But when once the pooling has begun, the combination is on an inclined plane, and every step hurries it on faster towards what is virtually complete amalgamation and consolidation. The recent history of trusts shows a constant tendency to give a more and more absolute power to the central executive and to reduce the heads of the separate establishments more and more nearly to the position of branch managers. In some cases the only substantial difference between such a trust and a consolidated joint-stock company is that it is nominally left open to the several parties contracting to claim their separate property after the lapse of a certain number of years, while some are already preparing to dissolve and reconstitute themselves formally as joint stock companies.

This tendency has been helped on by the action of the legislature and the law courts, and since this action can be traced back in some measure to the imperfect analysis of competition in the older economic writings, it has a special interest for us here. There seems to have been set up a false antithesis between competition and combination. For instance, if 100 workmen agreed to act together, as far as possible, in bargaining for the sale of their labour, they were denounced as combining to limit freedom, even when they did not interfere in any way with the liberty of other workmen, but merely deprived the employers of the freedom of making bargains with the 100 workmen, one by one. But the employer himself was allowed to unite in his own hands the power of hiring a hundred or twenty hundred men, and if he had not enough capital of his own he might take others into private, if not into public, partnership with him. Now, no trades union was likely to be as compact a combination, governed by as single a purpose, as a public or private firm, still less as an individual large employer; and therefore, there was not only a class injustice, but also a logical confusion, in prohibiting combinations among workmen, on the ground that free competition was a good, and that combination, being opposed to free competition, was, for that reason, an evil.

It was an additional grievance to the workmen that employers had all manner of facilities for combination, of which they made full use, as is vigorously urged by Adam Smith, to whom the working classes owe more than they know. And it was this social injustice, rather than the logical inconsistency of economists and legislators, that led workmen to claim—and for the greater part successfully—that nothing should be illegal if done by workmen in combination which would not be illegal if done by any one of them separately—a principle which works well practically in the particular case of workmen's combinations if applied with moderation; though it has no better claim to universal validity than the opposite doctrine.

But at present it is with the latter that we are concerned—the doctrine, namely, that a use of the rights of property which would be "combination in restraint of competition" if the ownership of the property were in many hands, is only a free use of the forms of competition when the property is all in a single hand. This doctrine has resulted in prohibitions of pooling between railways which were allowed to amalgamate, and in the prohibition of combination on the part of a group of traders to coerce others to act with them, or to drive others out of the trade, though all the while no attempt was made to hinder a single very wealthy firm from obtaining the despotic control of a market by similar means.

But to the economists of to-day the whole question appears both more complex and more important than it seemed to their predecessors, so they are inquiring in detail how far it is true that the looser forms of combination are specially dangerous in spite of their weakness, and even to some extent because of their weakness; how far the greater stability and publicity, and sense of responsibility and slowness of growth, of a single consolidated firm make it less likely to extend its operations over a very wide area, and less likely to make a flagrantly bad use of its power; and, lastly, how far it may be expedient to prohibit actions on the part of loose combinations, while similar actions on the part of individuals and private firms are allowed to pass in silence, because no prohibition against them could be effectual.

It is a sign of the times that the American Senate passed, on April 8 last, a Bill of Senator Sharman's, of which the second Section begins thus: "Every person who shall monopolise, or attempt to monopolise, or conspire with any other person or persons to monopolise, any part of the trade or commerce among the several States, or with foreign nations, shall be deemed guilty of a misdemeanour." This clause is interesting to the constitutional lawyer on account of the skill with which it avoids any interference by the central authority with the internal affairs of the separate States; and though, partly for this reason, it is perhaps intended to be the expression of a sentiment that may help to guide public opinion, rather than an enactment which will bear much direct fruit, yet it is of great interest to the economist as showing a tendency to extend to the action of individuals a form of public criticism which has hitherto been almost confined to the action of combinations.

To return, then, to the tendency of trusts towards consolidation. It is probable that the special legislative influences by which it has been promoted may be lessened, but that other causes will remain sufficiently strong to make a combination, which has once got so far as any sort of permanent pooling, tend almost irresistibly towards the more compact unity of a joint stock company. If this be so, the new movement will go more nearly on old lines than at one time seemed probable; and the question will still be the old one of the struggle for victory on the one hand between large firms and small firms, and on the other between departments of the Government, imperial or local, and private firms. I will then pass to consider the modern aspects of this question, ever old and ever new, but never more new and never more urgent than to-day.

To begin with, it is now universally recognized that there is a great increase in the number and importance of a class of industries, which are often called monopolies, but which are perhaps better described as *indivisible* industries. Such are the industries that supply gas or water in any given area, for only one such company in any district can be given leave to pull up the streets. Almost on the same footing are railways, tramways, electricity supply companies, and many others. Now, though there are some little differences of opinion among us as to the scale on which the owners of such undertakings when in private hands should be compensated for interference with what they had thought their vested rights, we are all agreed that such right of interference must be absolute, and the economists of to-day are eagerly inquiring what form it is most expedient for this interference to take. And here differences of opinion show themselves. The advantages of a bureaucratic government appeal strongly to some classes of minds, among whom are to be included many German economists and a few of the younger American economists who have been much under German influence. But those in whom the Anglo-Saxon spirit is strongest, would prefer that such undertakings, though always under public control, and sometimes even in public ownership, should whenever possible be worked and managed by private corporations. We (for I would here include myself) believe that bureaucratic management is less suitable for Anglo-Saxons than for other races who are more patient and more easily contented, more submissive and less full of initiative, who like to take things easily and to spread their work out rather thinly over long hours. An Englishman's or an American's life would involve too much strain to make them happy, while the Englishman would fret under the constraints and the small economies of their lives. Without therefore expressing any opinion as to the advantages of the public management of indivisible undertakings on the Continent, the greater part of the younger English and American economists are, I think, inclined to oppose it for England and America. We are not sure that we could exchange our own industrial virtues for those of the Continent if we wished to, and we are not sure that we do wish it. And though we recognize that the management of a vast undertaking by a public company has many of the characteristics of bureaucratic management, yet we think the former is distinctly the better suited for developing those faculties by which the Anglo-Saxon race has won its position in the world. We believe that a private company which stands to gain something by vigorous and efficient management by promptness in inventing, as well as in adopting and perfecting improvements in processes and organization, will do much more for progress than a public department.

Again, inferior as is a public company to a small private firm in its power and opportunities of finding out which among its employés have originating and constructive ability, a public department has much less still. And lastly it is more liable to have the efficiency of its management interfered with for the

purpose of enabling other persons to gain the votes of their constituents on questions in which it has no direct concern; and as a corollary from this, it tends to promote the growth of political immorality, and it suffers from that growth.

There is certainly a growing opinion among English and American economists that the State must keep a very tight hand on all industries in which competition is not an effective regulator; but this is the expression of a very different tone of thought from that which is leading so many German economists towards what is called State Socialism. In fact, so far as I can judge, English economists at all events are even more averse to State management than they were a few years ago; the set of their minds is rather towards inquiring how the advantages claimed for State management, without its chief evils, can be obtained even in what I have called indivisible industries; they are considering how a resolute intervention on the part of the State may best check the growth of *Imperia in Imperio*, and prevent private persons from obtaining an inordinate share of the gains arising from the development, through natural causes, of what are really semi-public concerns, at the same time that it leaves them sufficient freedom of initiative and sufficient security of gain by using that freedom energetically to develop what is most valuable in the energy and inventiveness of the Anglo-Saxon temper.[1]

But, though we dislike and fear the present tendency towards a widening of the area of public management of industries, we cannot ignore its actual strength. For more forethought and hard work are needed to arrange an effective public control over an undertaking than to put it bodily into the hands of a public department; and there is always a danger that in a time of hasty change the path of least resistance will be followed.

By way of illustration of the inquiries that have had their origin in this fear of public management, as contrasted with public control and public ownership, I would here mention a notion which has been suggested partly by the relations of some municipalities to their tramways, gas and water works. At present it is in a very crude form, and not ready for immediate application; but it seems to have occurred independently to a good many people, and it may have an important future. It is that a public authority may be able to own the franchise and, in some cases, part of the fixed capital of a semi-public undertaking, and to lease them for a limited number of years to a corporation who shall be bound to perform services, or deliver goods, at a certain price and subject to certain other regulations, some of which may perhaps concern their relations to their employés; and, further, that competition for the franchise shall turn on the price or the quality, or both, of the services or the goods, rather than the annual sum paid for the lease. Competition as to quality is, from the consumer's point of view, often just as beneficial as competition is to price, and sometimes more so. And in industries which obey the Law of Increasing Returns, as very many of these indivisible industries do, a reduction of price or an improvement of quality will confer on the consumer a benefit out of all proportion to the extra cost involved.[2]

But I have lingered too long over those industries which I have called indivisible, and I must pass to those in which competition exerts a pretty full sway. The first point to be observed is that competition in bargaining and competition in production stand in very different relations to the public interest; and that one of the great advances in modern analysis consists in the emphasis which it lays on the distinction between the two. Competition in bargaining constitutes a great part of competition in marketing, but is not the whole of it. For under marketing is included the whole of the effective organization of the trade side of a business; and most of this performs essential services for the public, and is, in fact, of the same order as production commonly so called. But a great part of marketing consists of bargaining, of manœuvring to get others to buy at a high price and sell at a low price, to obtain special concessions or to force a trade by offering them. This is, from the social point of view, almost pure waste; it is that part of trade as to which Aristotle's dictum is most nearly true, that no one can gain except at the loss of another. It has a great attraction for some minds that are not merely mean; but nevertheless it is the only part of honest trade competition that is entirely devoid of any ennobling or elevating feature. A claim is made on behalf of large firms and large combinations that their growth tends to diminish the waste, and on the whole perhaps it does. The one solid advantage which the public gain from a combination powerful enough to possess a local monopoly is that it escapes much waste on advertising and petty bargaining and manœuvring. But its weakness in this regard lies in the fact that to keep its monopoly it must be always bargaining and manœuvring on a large scale. And if its monopoly is invaded, it must bargain and manœuvre widely in matters of detail as well as in larger affairs.

Still less can we fully concede, without further proof, the claim which has been urged on behalf of such combinations, that they will render industry more stable and diminish the fluctuations of commercial activity. This claim, though put forward confidently and by many writers, does not appear to be supported by any arguments that will bear examination. On the one hand some industries which are already aggregated into large and powerful units, such as railway companies, give exceptionally steady employment; and others, such as the heavy iron and the chemical industries, exceptionally unsteady. And when combinations succeed in steadying their own trades a very little, they often do it by means which diminish production and disturb other trades a very great deal. The teaching of history seems to throw but little light on the question, because the methods of regulation which are now suggested have not much in common with those of earlier times, while the causes which govern variations in prices have changed their character completely.

Let us then next turn to the economies of production on a large scale. They have long been well known, and our forefathers certainly did not underrate their importance. For, though the absence of any proper industrial census in England prevents us from getting exact information on the subject, yet there seems no doubt that the increase in the average size of factories has gone on, not faster, but slower than was thought probable a generation or two ago. In many industries, of which the textile may be taken as a type, it has been found that a comparatively small capital will command all the economies that can be gained by production on a large scale; and it seems probable that in many industries in which the average size of businesses has been recently increasing fast, a similar position of maximum economy will shortly be attained without any much further increase in size.

Those reductions in the expenses of production of commodities which have been claimed by the eulogists of trusts and other large combinations, as tending to show that their gains are not at the expense of the public, turn out generally to have been at least equalled by the reductions in the expenses of production in similar industries in which there was no combination. And this count in their eulogy, though it may truly stand for something, seems to have been much exaggerated.

After all, what these very large public firms and combinations of firms have done has generally been only to turn to good account existing knowledge, and not to increase that knowledge. And this brings us to the main reason for regarding with some uneasiness any tendency there may be towards such consolidations of business. Observation seems to show, what might have been anticipated *à priori*, that though far superior to public departments, they are, in proportion to their size, no less inferior to private businesses of a moderate size in that energy and resource, that restlessness and inventive power, which lead to the striking out of new paths. And the benefits which the world reaps from this originality are apt to be underrated. For they do not come all at once like those gains which a large business reaps by utilizing existing knowledge and well-proven economies; but they are cumulative, and not easily reckoned up. He who strikes out a new path by which the work of eight men is rendered as efficient as that of ten used to be, in an industry that employs 100,000 men, confers on the world a benefit equal to the labour of 20,000 men. And this benefit may in many cases be taken as running for many years. For though his discovery might have been made later by some one else of equal inventive power, yet this some one else, starting with that discovery in hand, is likely to make another improvement on it.

[1] Among the younger English economists who have written on the subject of combinations, trusts, and Government interference, I would specially refer to Mr. Rae and Prof. Foxwell. Most of the other young American economists have written on it instructively from various points of view, and in Mr. Baker's "Monopolies and the People," to which I am myself much indebted, the English reader will find condensed into a short compass an account of the general position of these questions in America, together with some bold and interesting suggestions for reform. Some useful documents relating to trusts have recently been published in a Consular Report by our Foreign Office [5896–32].

[2] This belongs to a class of questions relating to monopolies, &c., the more general and abstract aspects of which can be best shown by the diagrammatic method.

I believe that the importance of considerations of this kind is habitually underrated in the world at large; and that the older economists, though fully conscious of them, did not explain with sufficient clearness and iteration the important place which they take in the claims of industrial competition on the gratitude of mankind.

The chemist in his laboratory can make experiments on his own responsibility: if he had to ask leave from others at each step he would go but slowly, and though the officials of a company may have some freedom to make experiments in detail, yet even as regards these they seldom have a strong incentive to exertion; and in great matters the freedom of experimenting lies only with those who undertake the responsibility of the business.

It may indeed be admitted that some kinds of industrial improvements are getting to depend on the general increase of scientific knowledge rather than on such experiments as can only be made by business men. And this, which is an important fact so far as it goes, may be used as a convenient introduction to the next point that I want to make in the analysis of competition. It is that the motives which induce business men to compete for wealth are not altogether as sordid as the world in general, and I am forced to admit, economists in particular have been wont to assume.

The chemist or the physicist may happen to make money by his inventions, but that is seldom the chief motive of his work. He wants to earn somehow the means of a cultured life for himself and his family, but, that being once provided, he spends himself in seeking knowledge partly for its own sake, partly for the good that it may do to others, and last, and often not least, for the honour it may do himself. His discoveries become collective property as soon as they are made, and altogether he would not be a very bad citizen of Utopia just as he is. For it would be a great mistake to suppose that the constructors of Utopias from the time of Plato downwards have proposed to abolish competition. On the contrary, they have always taken for granted that a desire to do good for its own sake will need to be supplemented by emulation or competition for the approbation of others.

But business men are very much of the same nature as scientific men; they have the same "instincts of the chase," and many of them have the same power of being stimulated to great and even feverish exertions by emulations that are not sordid or ignoble. This part of their nature has however been confused with and thrown into the shade by their desire to make money. The chief reason why the scientific man does not care much for money is that in scientific work the earning of much money is no proof of excellence, but sometimes rather the reverse. On the other hand, in business a man's money-earning power, though not an accurate test of the real value to the world of what he has done, is yet often the best available. It is that test which most of those, for whose opinion he cares, believe to be more trustworthy than the highly-coloured reports the world hears from time to time of the benefits which it is just going to derive from a new invention or plan of organizing that is just going to revolutionize a branch of industry. And so all the best business men want to get money, but many of them do not care about it much for its own sake; they want it chiefly as the most convincing proof to themselves and others that they have succeeded.

These are the very men for whom the older economists were most eager to claim freedom of competition as needful to evoke them to do fully their high work for the world. But they seem to have made the error of running together and treating as though they were one, two different positions.

The first is that industrial progress depends on our getting the right men into the right places and giving them a free hand, and sufficient incitement to exert themselves to the utmost. And the second position is that nothing less than the enormous fortunes which successful men now make and retain would suffice for that purpose. This last position seems to be untenable.

The present extreme inequalities of wealth tend in many ways to prevent human faculties from being turned to their best account. A good and varied education, freely prolonged to those children of the working classes who showed the power and the will to use it well, an abundance of open-air recreation even in large towns, and other requisites of a wholesome life—such things as these might, most of us are inclined to think, be supplied by taxes levied on the rich, without seriously checking the accumulation of material capital; and with the effect of increasing rather than diminishing the services which competition renders to society by tending to put the ablest men into the most important posts, the next ablest into the next most important, and so on, and by giving to those in each grade freedom sufficient or the full exercise of their faculties.

It is quite true that where any class of workers have less than the necessaries for efficiency, an increase of income acts directly on their power of work. But when they already have those necessaries, the gain to production from a further increase of their income depends chiefly on the addition that it makes, not to their power of working, but to their will to exert themselves. And all history shows that a man will exert himself nearly as much to secure a small rise in income as a large one, provided he knows beforehand what he stands to gain, and is in no fear of having the expected fruits of his exertions taken away from him by arbitrary spoliation. If there were any fear of that he would not do his best; but if the conditions of the country were such that a moderate income gave as good a social position as a large one does now; if to have earned a moderate income were a strong presumptive proof that a man had surpassed able rivals in the attempt to do a difficult thing well, then the hope of earning such an income would offer to all but the most sordid natures, inducements almost as strong as they are now when there is an equal hope of earning a large one.

On all this class of questions modern economists are inclined to go a little way with the socialists. But all socialist schemes, and especially those which are directly or indirectly of German origin, seem to be vitiated by want of attention to the analysis which the economists of the modern age have made of the functions of the undertaker of business enterprises. They seem to think too much of competition as the exploiting of labour by capital, of the poor by the wealthy, and too little of it as the constant experiment by the ablest men for their several tasks, each trying to discover a new way in which to attain some important end. They still retain the language of the older economists, in which the employer, or undertaker, and the capitalist are spoken of, as though they were, for all practical purposes, the same people. The organ of the German school of English socialists prints frequently in thick type the question, "Is there one single useful or necessary duty performed by the capitalist to-day which the people organized could not perform for themselves?" It would be just as reasonable to ask if there is a single victory to which Julius Cæsar or Napoleon conducted their troops, which the troops properly organized could not have equally well won for themselves; or whether there is a single thing written by Shakespeare which could not have been equally well written by any one else who, as Charles Lamb said, happened to "have the mind to do it." It is quite true that many business men earn large incomes by routine work. It is just in these cases that Co-operation can dispense with middlemen and even employers. But the German socialists have been bitter foes of Co-operation; though this antagonism is less than it was.

The world owes much to the socialists, as it does to every set of enthusiasts among whom there are honest men; and many a generous heart has been made more generous by reading their poetic aspirations. But before their writings can be regarded as serious contributions to economic science, they must make more careful and exact analysis of the good and the evil of competition; and they must suggest some reasonably efficient substitute for that freedom which our present system offers to constructive genius to work its way to the light, and to prove its existence by attempting difficult tasks on its own responsibility, and succeeding in them. For those who have done most for the world have seldom been those whom their neighbours would have picked out as likely for the work. They must not, as even Mr. Bellamy and other American socialists do, in spite of their strong protestations to the contrary, assume implicitly a complete change of human nature; and propound schemes which would much diminish the aggregate production, but which they represent as enabling every family to attain an amount of material well-being which would be out of reach of the aggregate income if England or America were divided out equally among the population.

But though the socialists have ascribed to the virtues inherent in the human breast, and to the regulating force of public opinion, a much greater capacity for doing the energizing work of competition than they seem really to have; yet, unquestionably, the economists of to-day do go beyond those of earlier generations in believing that the desire of men for the approval of their own conscience, and the esteem of others, is an economic force of the first order of importance, and that its strength is steadily increasing with the increase and the diffusion of knowledge, and

with the constant tendency of what had been regarded as private and personal issues to become public and national.

Public opinion acts partly through the Government. The enforcement of the law in economic matters occupies the time of a rapidly increasing number of people; and though its administration is improving in every way, it fails to keep pace with the demands resulting from the growing complexity of economic organization, and the growing sense of responsibility of public opinion. A part of this failure is due to a cause which might easily be remedied; it is that the adjustment of punishment to offences is governed by traditions descending from a time when the economic structure of England was entirely different. This is most conspicuous with regard to the subtler, or, as they are sometimes called with unconscious irony, the more gentlemanly forms of commercial fraud on a large scale; for which the punishment awarded by the law courts is often trivial in comparison with the aggregate gains which the breakers of the law, whose offences can seldom be proved, make by their wrong-doing; and it is still more trivial in comparison with the aggregate injury which such wrongdoing inflicts on the public. Many of the worst evils in modern forms of competition could be diminished by merely bringing that part of the law which relates to economic problems of modern growth into harmony with that which relates to the old-fashioned and well-matured economic questions relating to common picking and stealing. And somewhat similar remarks apply to the punishments for infringements of the Factory Acts.

But at best the action of the law must be slow, cumbrous, and inelastic, and therefore ineffective. And there are many matters in which public opinion can exercise its influence more quickly and effectively by a direct route, than by the indirect route of first altering the law. For of all the great changes which our own age has seen in the relative proportions of different economic forces, there is none so important as the increase in the area from which public opinion collects itself, and in the force with which it bears directly upon economic issues.

For instance, combinations of labour on the one side, and of employers on the other, are now able to arrange plans of campaign for whole trades, for whole counties, for the whole country, and sometimes even beyond. And partly on account of the magnitude of the interests concerned, partly because trade disputes are being reduced to system, affairs which would be only of local interest are discussed over the whole kingdom.

The many turbulent little quarrels which centred more often about questions of individual temper than of broad policy, are now displaced by a few great strikes, as to which public opinion is on the alert; so that a display of temper is a tactical blunder. Each side strives to put itself right with the public; and requires of its leaders above all things that they should persuade the average man that their demands are reasonable, and that the quarrel is caused by the refusal of the other side to accept a reasonable compromise.

This change is increasing the wisdom and the strength of each side; but the employers have always had fairly good means of communication with one another; it is the employed that have gained most from cheap means of communication by press, by railway, and by telegraph, and from improvements in their education and in their incomes, which enable them to make more use of these new and cheaper facilities. And while the employers have always known how to present their case to the public well, and have always had a sympathetic public, the working classes are only now beginning to read newspapers enough to supply an effective national working class opinion, and they are only now learning how to present their case well, and to hope much from, or care much for, the opinion of those who are neither employers nor of the working classes.

I myself believe that in all this the good largely predominates over the evil. But that is not the question with which I am specially concerned at present. My point is that, in the scientific problem of estimating the forces by which wages are adjusted, a larger place has to be allowed now than formerly to the power of combination, and to the power of public opinion in judging, and criticizing, and aiding that combination; and that all these changes tend to strengthen the side of the employés, and to help them to get a substantial though not a great increase of real wages; which they may, if they will, so use as to increase their efficiency, and therefore to increase still further the wages which they are capable of earning, whether acting in combination or not.

And thus public opinion has a very responsible task. I have spoken of it as the opinion of the average man; and he is very busy, and has many things to think about. He makes great mistakes, but he learns by all of them. He has often astonished the learned by the amount of ignorance and false reasoning which he can crowd into the discussion of a difficult question; and still more by the way in which he is found at last to have been very much in the right on the main issue. He is getting increased power of forming a good and helpful opinion, and he is being educated in mind and in spirit by forming it, and by giving it effect. But in the task which he is undertaking there are great difficulties ahead.

In an industrial conflict each side cares for the opinion of the public at large, but especially for that of those whose sympathy they are most likely to get: in the late South Wales strike, for instance, the railway companies were specially anxious about the good opinion of the shippers, and the engine drivers about that of the colliers. And there is some fear that when party discipline becomes better organized, those on either side will again get to care less for any public opinion save that of their own side. And if so, there may be no great tendency towards agreement between the two sides as to what are reasonable demands.

It is true that there is always the action of outside competition tending to visit with penalties either side, which makes excessive use of any tactical advantage it may have obtained. As we have just noticed, the shrewdest organizers of a trust are averse to raising the price of its wares much above the normal or steady competition price. And the first point which courts of conciliation and arbitration have to consider is, what are the rates of wages on the one hand and of profits on the other, which are required to call forth normal supplies of labour and capital respectively; and only when that has been done, can an inquiry be properly made as to the shares in which the two should divide between them the piece of good or ill fortune which has come to the trade. Thus the growth of combinations and partial monopolies has in many ways increased, and in no way diminished, the practical importance of the careful study of the influences which the normal forces of competition exert on normal value.

But it must be admitted that the direct force of outside competition in some classes of wages disputes is diminishing; and though its indirect force is being increased by the increased power which modern knowledge gives us of substituting one means of attaining our ends for another, yet on the whole the difficulty of deciding what is a reasonable demand is becoming greater. The principles on which not only the average man, but also an expert court of conciliation or arbitration should proceed in forming their judgment, are becoming, in spite of the great increase of knowledge, more and more vague and uncertain in several respects.

And there are signs of a new difficulty. Hitherto the general public has been enlightened, and its interests protected by the fact that the employers and employed when in conflict have each desired to enlighten the public as to the real questions at issue; and the information given on one side has supplemented and corrected that on the other: they have seldom worked together systematically to sacrifice the interests of the public to their own, by lessening the supply of their services or goods, and thus raising their price artificially. But there are signs of a desire to arrange firm compacts between combinations of employers on the one side and of employés on the other to restrict production. Such compacts may become a grievous danger to the public in those trades in which there is little effective competition from foreign producers: a danger so great that if these compacts cannot be bent by public opinion they may have to be broken up by public force.

It is, therefore, a matter of pressing urgency that public opinion should accustom itself to deal with such questions, and be prepared to throw its weight against such compacts as are injurious to the public weal, that is, against such compacts as are likely to inflict on the public a real loss much greater than the gain to that trade; or in other words, are of such a nature that if their principle were generally adopted in all trades and professions, then all trades and professions would lose as buyers more than they would gain as sellers.

I must now close this imperfect and fragmentary study. I have endeavoured to give some illustrations of the changes which are coming over economic studies. I believe that the great body of modern economists think that the need of analysis and general reasoning in economics is not less than our predecessors supposed, but more. And this is because we think economic problems

more difficult than they did. We are recognizing more clearly than they did that all economic studies must have reference to the conditions of a particular country and time. Economic movements tend to go faster than ever before, but, as Knies pointed out, they tend also to synchronize; and the economists of our western countries have much more to learn now than fifty years ago from the contemporary history of other countries; but in spite of the many great benefits which we are deriving from the increase of our historical knowledge, the present age can rely less than any other on the experience of its predecessors for aid in solving its own problems.

Every year economic problems become more complex; every year the necessity of studying them from many different points of view and in many different connections becomes more urgent. Every year it is more manifest that we need to have more knowledge and to get it soon in order to escape, on the one hand, from the cruelty and waste of irresponsible competition and the licentious use of wealth, and on the other from the tyranny and the spiritual death of an ironbound socialism.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY CAPTAIN NOBLE, C.B., F.R.S., F.R.A.S., F.C.S., M.INST.C.E., PRESIDENT OF THE SECTION.

IN taking over the chair of this Section from my distinguished predecessor, I cannot but feel myself to some extent an intruder into the domain of mechanical science, and I am conscious that the office which I have the honour to hold would have been more worthily filled by one of the great mechanicians who have won for the town in which we hold our meeting so widespread a reputation.

I can truly say the claims on my time are so considerable that I should not have ventured to appear before you in the character of President of this Section had it not been for my desire to afford what little support might be in my power to my friend the President of the British Association, with whom for so long a period I have been associated by so many ties.

I believe I should have consulted best both my own feelings and your patience by merely opening the Section in a formal manner, and proceeding at once to the business of the meeting. One of my predecessors, however, has pointed out that Sir F. Bramwell, whose authority is too great to be disputed, has ruled that to depart from the time-honoured practice of an address is an act of disrespect to the Section—a ruling which has, without cavil, been accepted.

I therefore propose to direct your attention, by a few brief remarks, to that branch of mechanical science with which I am best acquainted. I shall endeavour to show the great indebtedness of the naval and military services to mechanical science during the period with which I have been more or less connected with them, and the complete revolution which has in consequence resulted in every department and in every detail.

But before commencing with my special subject, it is impossible that I should pass over in silence the great work which has excited so much interest in the engineering world, and which, since we last met, has, with formalities worthy of the occasion, been opened by H.R.H. the Prince of Wales.

It is in no way detracting from the merit of the distinguished engineers who have with so much boldness in design, with such an infinity of care in execution, with so much foresight in every detail, given to the country this great monument of skill, if I venture to point out that, without the great advance of mechanical and metallurgical science during the present generation, and the co-operation of a host of workers, a creation like that of the Forth Bridge would have been an impossibility.

The bridge has been so frequently and so fully described that it is unnecessary in this address I should do more than draw your attention to some of its main features.

The bridge, with its approach-viaducts, has a total length of 8296 feet, or nearly a mile and six-tenths; and this length comprises two spans of 1710 feet, two of 680½ feet, fifteen of 160 feet, four of 57 feet, and three of 25 feet.

The deepest foundation is 90 feet below high-water mark, and the extreme height of the central position of the cantilever is 361 feet above the same datum, making the extreme total height of the bridge 451 feet.

The actual minimum headway in the channels below the centre of the main spans at high-water spring tides is a little over 150 feet, and the rail level is about 6 feet higher.

The weight of steel, nearly all riveted work, is 54,076 tons, and the amount of masonry and concrete 4,057,555 cubic feet.

It is difficult, even for experts, fully to appreciate the stupendous amount of work indicated by these figures. During the Paris Exhibition the Eiffel Tower justly excited considerable admiration, and brought its designer into much repute; but that great work sinks altogether into insignificance when compared with the Forth Bridge.

Conceive, as I have heard described, the Eiffel Tower built, not vertically, but horizontally; conceive it further built without support, and at a giddy height over an arm of the sea. Such a work would do little more than reach half across one of the main spans of this great bridge.

Those only who have had work of a similar nature can fully appreciate the innumerable experiments that must have been made, and the calculations that must have been gone through to secure the maximum attainable rigidity both with respect to the strains induced vertically by the railway traffic and its own weight, and horizontally by the force of gales.

The anxiety as to the security of the erection might well daunt the most skilful engineer. We are told that, apart from the permanent work, many hundreds of tons of weight in the shape of cranes, temporary girders, winches, steam boilers, rivet furnaces, and riveting machines, miles of steel-wire rope, and acres of timber staging were suspended from the cantilevers. A heavy shower of rain would in a few minutes give an additional weight of about 100 tons; and in their unfinished state, while approaching completion, the force of any gale had to be endured.

I trust that, as the Forth Bridge has been a great engineering, it may likewise prove a financial success, and I feel sure that all who hear me are rejoiced that it has pleased Her Majesty to confer the distinguished honours she has awarded to Sir John Fowler and Sir B. Baker—honours, I may add, that have rarely been more worthily bestowed.

Let me turn now to the subject on which I propose to address you; and I shall first advert to the change which within my own recollection has taken place in that service which has been the pride and glory of the country in time past, and on which we must rely in the future as our first and principal means at once of defence and attack.

To give even an idea of the revolution which our navy has undergone, I must refer in the first instance to the navy of the past. I must refer to those vessels which in the hands of our great naval commanders won for England victories which left her at the close of the great wars supreme upon the sea.

A "first-rate" of those days (I will take the *Victory* as a type) was a three-decker 186 feet in length, 52 feet in breadth, with a displacement of 3500 tons, and she carried an armament of 102 guns, consisting of thirty 42- and 32-pounders, thirty 24-pounders, forty 12-pounders, and two 68-pounder carronades (the heaviest of her guns was a 42-pounder), and she had a complement of nearly 900 men. When we look at the wonderful mechanism connected with the armaments of the fighting-ships of the present day, it is difficult to conceive how such feats were accomplished with such rude weapons.

With the exception of a few small brass guns, the guns were mere blocks of cast iron, the sole machining to which they were subjected consisting in the formation of the bore and the drilling of the vent.

A large proportion of nearly every armament consisted of carronades—a piece which was in those days in great favour. They threw a shot of large diameter from a light gun with a low charge, and their popularity was chiefly due to the rapidity with which they could be worked. The great object of every English commander was, if it were possible, to bring his ship alongside that of the enemy; and under these circumstances the low velocity given by the carronades became of comparatively small moment, while the ease of working and the large diameter of the shot were factors of the first importance.

The carriages on which the rude weapons I have described were placed were themselves, if possible, even more rude. They were of wood, and consisted of two cheeks with recesses for the trunnions, which were secured by cap squares, the cheeks being connected by transoms, and the whole carried by trucks. The gun was attached to the vessel's side, and the recoil controlled by breeching. The elevation was fixed by quoins which rested

on a quoin bed, and handspikes were used either for elevating or for training.

It is obvious that, to work smartly so rude a machine, a very strong gun's crew was required. Indeed, the gun and its carriage were literally surrounded by its crew, and I may refer those who desire to acquaint themselves with the general arrangements of what was once the most perfect fighting-machine of the first navy in the world, to the frontispiece of a book now nearly forgotten—I mean Sir Howard Douglas's "Naval Gunnery."

The mechanical appliances on board these famed war-vessels of the past were of the simplest possible form, and such as admitted of rapid renewal or repair. There was no source of power except manual labour; but, when handled with the unrivalled skill of British seamen, the handiness of these vessels, and the precision with which they were manœuvred, was a source of never-ending admiration.

Those who have seen, as I have done, a fleet like the Mediterranean squadron enter a harbour, such as Malta, under full sail, and have noted the precision with which each floating castle moved to her appointed place, the rapidity with which her canvas was stowed, have seen a sight which I consider as the most striking I have witnessed, and infinitely more imposing than that presented under like circumstances by modern vessels, any one of which could in a few minutes blow out of the water half a dozen such men-of-war as I have been just describing.

I must not, however, omit to mention two advantages possessed by the old type of war-vessels, which, if we could reproduce them, would greatly please modern economists. I mean, their comparatively small cost, and the length of time the vessels remained fit for service.

When the *Victory* fought the battle of Trafalgar she had been afloat for forty years, and her total cost, complete with her armament and all stores, was probably considerably under £100,000. The cost of a first-rate of the present day, similarly complete, would be nearly ten times as great.

The most improved battle-ships of the period just anterior to the Crimean war differed from the type I have just described, mainly by the addition of steam power, and for the construction of these engines the country was indebted to the great pioneers of marine engineering, such as J. Penn and Sons, Maudslay Sons and Field, Ravenhill, Miller, and Co., Rennie Bros., &c., not forgetting Messrs. Humphreys and Tennant, whose reputation and achievements now are even more brilliant than in these earlier days.

Taking the *Duke of Wellington*, completed in 1853, as the type of a first-rate just before the Crimean war, her length was 240 feet, her breadth 60 feet, her displacement 5830 tons, her indicated horse-power 1999, and her speed on the measured mile 9·89 knots. Her armament consisted of 131 guns, of which thirty-six 8-inch and 32-pounders were mounted on the lower deck, a similar number on the middle deck, thirty-eight 32-pounders on the main deck, and twenty short 32-pounders and one 68-pounder pivot gun on the upper deck.

Taking the *Cæsar* and the *Hogue* as types of second- and third-rate line-of-battle ships, the former, which had nearly the displacement of the *Victory*, had a length of 207 feet, a breadth of 56 feet, and a mean draught of 21. She had 1420 indicated horse-power, and her speed on the measured mile was 10·3 knots. Her armament consisted of twenty-eight 8-inch guns and sixty-two 32-pounders, carried on her lower, main, and upper decks. The *Hogue* had a length of 184 feet, a breadth of 48 feet 4 inches, a mean draught of 22 feet 6 inches: she had 797 indicated horse-power, and a speed of $8\frac{1}{2}$ knots. Her armament consisted of two 68-pounders of 95 cwt., four 10-inch guns, twenty-six 8-inch guns, and twenty-eight 32-pounders of 56 cwt.—sixty guns in all.

Vessels of lower rates (I refer to the screw steam frigates of the period just anterior to the Crimean war) were, both in construction and armament, so closely analogous to the line-of-battle-ships that I will not fatigue you by describing them, and will only allude to one other class, that of the paddle-wheel steam frigate, of which I may take the *Terrible* as a type. This vessel had a length of 226 feet, a breadth of 43 feet, a displacement of about 3000 tons, and an indicated horse-power of 1950. Her armament consisted of seven 68-pounders of 95 cwt., four 10-inch guns, ten 8-inch guns, and four light 32-pounders.

It will be observed that in these armaments there has been a very considerable increase in the weight of the guns carried. As I have said, the heaviest guns carried by the *Victory* were the 42-pounders of 75 cwt., but in these later armaments the 68-pounder of 95 cwt. is in common use, and you will have noted that the carronades have altogether disappeared. But as regards improvements in guns or mounting, if we except the pivot-guns, with respect to which there was some faint approach to mechanical contrivance to facilitate working, the guns and carriages were of the rude description to which I have alluded.

In one respect, indeed, a great change had been made. Shell-fire had been brought to a considerable state of perfection, and the importance ascribed to it may be traced in the number of 10-inch and 8-inch shell-guns which entered into the armament of the *Duke of Wellington* and the other ships I have mentioned. Moorsom's concussion fuse and other similar contrivances lent great assistance to this mode of warfare, and its power was soon terribly emphasized by the total destruction of the Turkish squadron at Sinope by the Russian fleet. In that action, shell-fire appears to have been almost exclusively used, the Russians firing their shell with rather long-time fuses in preference to concussion, with the avowed object of there being time before bursting to set fire to the ship in which they lodged.

It is curious to note in the bygone discussions relative to shell-fire the arguments which were used against it; among others it was said that the shell would be more dangerous to those who used them than to their enemies. There was some ground for this contention, as several serious catastrophes resulted from the first attempts to use fused shells. Perhaps the most serious was that which occurred on board H.M.S. *Theseus*, when seventy 36- and 24-pounder shells captured from a French storeship and placed on the quarter-deck for examination exploded in quick succession, one of the fuses having by some accident been ignited. The ship was instantly in flames; the whole of the poop and after-part of the quarter-deck were blown to pieces. The vessel herself was saved from destruction with the greatest difficulty, and forty-four men were killed and forty-two wounded.

This accident was due to a neglect of obvious precautions, which would hardly occur nowadays, but I have alluded to the circumstance because the same arguments, or arguments tending in the same direction, are in the present day reproduced against the use of high explosives as bursting charges for shells. To this subject I myself and my friend and fellow labourer, Mr. Vavasseur, have given a good deal of attention, and the question of the use of these shells and the best form of explosive to be employed with them is, I believe, receiving attention from the Government. The importance of the problem is not likely to be overrated by those who have witnessed the destruction caused by the bursting of a high explosive shell, and who appreciate the changes that by their use may be rendered necessary, not only in the armaments, but even in important constructional points of our men-of-war.

Shortly before the termination of the long period of peace which commenced in 1815, the attention of engineers and those conversant with mechanical and metallurgical science, seems to have been strongly directed towards improvements in war material. It may easily be that the introduction of steam into the navy may have had something to do with the beginning of this movement, but its further progress was undoubtedly greatly accelerated by the interest in the subject awakened by the disturbance of European peace which commenced in 1854.

Since that date—whether we have regard to our vessels of war, the guns with which they and our fortresses are armed, the carriages upon which those guns are mounted, or the ammunition they employ—we shall find that changes so great and so important have been made that they amount to a complete revolution. I believe it would be more correct to say several complete revolutions. It is at least certain that the changes which were made within the period of ten years following 1854, were far more important and wide-spreading in their character than were all the improvements made during the whole of the great wars of the last and the commencement of the present century.

Indeed, it has always struck me as most remarkable that during the long period of the Napoleonic and earlier wars, when the mind of this country must have been to so large an extent fixed on everything connected with our naval and military services, so little real progress was made.

Our ships, no doubt, were the best of their class, although, I believe, we were indebted for many of our most renowned models to vessels captured from our neighbours. They were fitted for sea with all the resources and skill of the first seamen of the world, and when at sea were handled in a manner to command universal admiration. But their armaments were of

the rude nature I have described, and so far as I can see possessed little, if any, advantage over those of nearly a couple of centuries earlier. It is not improbable that the great success which attended our arms at sea may have contributed to this stagnation.

The men who with such arms achieved such triumphs, may well be forgiven for believing that further improvement was unnecessary, and it must be remembered that the practice of engaging at very close quarters minimised to a great extent the most striking deficiencies of the guns and their mountings.

I need scarcely, however, remind you that were two vessels of the old type to meet, one armed with her ancient armament, the other with modern guns, it would be vain for the former to attempt to close. She would be annihilated long before she approached sufficiently near to her antagonist to permit her guns to be used with any effect.

It would be quite impossible, within reasonable limits of time, to attempt to give anything like an historical account of the changes which have taken place in our ships of war during the last thirty-five years, and the long battle between plates and guns will be fresh in the memory of most of us. The modifications which the victory of one or the other impressed on our naval constructions are sufficiently indicated by the rapid changes of type in our battle-ships, and by the number of armour-clads once considered so formidable, but seldom now mentioned except to adorn the tale of their inutility. The subject also requires very special knowledge, and to be properly handled must be dealt with by some master of the art, such as our Director of Naval Construction.

Let me now compare with the vessels of the past those of the present day, and for my purpose I shall select for comparison as first-rates the *Victoria* and the *Trafalgar*. The *Victoria* has a length of 340 feet, a breadth of 70 feet; she has a displacement of about 10,500 tons, an indicated horse-power of 14,244, and she attained a speed on the measured mile of 17½ knots; she has a thickness of 18 inches of compound armour on her turrets, a similar thickness protects the redoubt, and her battery-deck is defended with 3-inch plates. Her armament consists of two 16¼-inch 110-ton guns, one 10-inch 30 ton gun, twelve 6-inch 5-ton guns, twelve 6-pounder and nine 3-pounder quick-firing guns, two machine guns, and six torpedo guns.

The *Trafalgar* has a length of 345 feet, or very nearly double the length of the *Victory*, a displacement of 12,000 tons, an indicated horse-power of 12,820, and a speed on the measured mile of a little over 17¼ knots. Her armament consists of four 68-ton guns, six 4·7-inch quick-firing guns, six 6-pounder, and nine 3-pounder quick-firing guns, six machine and six torpedo-guns.

Comparing the armament of the *Victoria* with that of the *Victory*, we find, to quote the words of Lord Armstrong—which when evaluating the progress we have made will bear repetition —that while the heaviest gun on board the *Victory* was a little over 3 tons, the heaviest on board the *Victoria* is a little over 110 tons. The largest charge used on board the *Victory* was 10 lbs., the largest on board the *Victoria* close on 1000 lbs.; the heaviest shot used in the *Victory* was 68 lbs., in the *Victoria* it is 1800 lbs. The weight of metal discharged from the broadside of the *Victory* was 1150 lbs., from that of the *Victoria* it is 4750 lbs. But having regard to the energy of the broadside, the power of each ship is better indicated by the quantity of powder expended than by the weight of metal discharged, and while the broadside fire from the *Victory* consumed only 355 lbs. of powder, that from the *Victoria* consumes 3120 lbs.

These figures show in the most marked manner the enormous advances that have in every direction been made in the construction and armament of these marine monsters; but it is when we come to the machinery involved in our first-rates that the contrast between the past and the present is brought most strongly into prominence.

I have alluded to the simplicity of the arrangements on board the old battle-ships, but no charge of this nature can be made against the present. The *Victoria* has no less than twenty-four auxiliary steam-engines in connection with her main engines, viz. two starting, two running, eight feed, eight fan, for forced draught, and four circulating water engines. She has in addition thirty steam engines unconnected with her propelling engines, viz. six fire and bilge engines, two auxiliary circulating engines, four fan engines for ventilating purposes, two fresh-water pumping engines, two evaporating fuel engines, one workshop, one capstan, and five electric-light engines, four air-compressing and three pumping engines for hydraulic purposes.

She has further thirty-two hydraulic engines, including two steering engines, four ash hoisting engines, two boat engines, four ammunition lifts, two turret-turning engines, one topping winch, two transporting and lifting engines, two hydraulic bollards, and fourteen other engines for performing the various operations necessary for the working of her heavy guns, making a grand total of eighty-eight engines. This number is exclusive of the machinery in the torpedo and other steam-boats, and of the locomotive engines in the torpedoes carried, which are themselves engines of a most refined and delicate character.

At an earlier point in my address I alluded to the incomparable seamanship of our bygone naval officers. Seamanship will, I fear, in future naval battles no longer play the conspicuous part it has done in times past. The weather-gage will belong not to the ablest sailor, but to the best engineer and fastest vessel, but the qualities of pluck, energy, and devotion to their profession which distinguished the seamen of the past have, I am well assured, been transmitted to their descendants, and I am glad to have the opportunity of expressing my admiration of the ability and zeal with which the naval officers of the present day have mastered, and the skill with which they use, the various complicated, and in some cases delicate machinery which mechanical engineers have placed in their hands.

I pass now to a class of vessel—the fast protected cruisers—intended to take the place and perform the duties of the old frigates. Of these I will take as types H.M.S. *Medusa* and the Italian cruiser *Piemonte*. The *Medusa* has a length of 265 feet, a breadth of 41 feet, a displacement of 2800 tons, and her engines have 10,010 indicated horse-power. Her armament consists of six 6-inch breech-loading guns, ten 3-pounders, four machine guns, and two fixed and four turning torpedo tubes. The *Piemonte* has a length of 300 feet, a breadth of 38 feet, a displacement of 2500 tons, and her engines of 12,981 indicated horse-power developed on the measured mile a speed of 22·3 knots, or about 26 miles. Her armament, remarkable as being the first instance of an equipment composed altogether of quick-firing guns, consists of six 6-inch 100-pounders, and six 4·7-inch 45-pounders, all with large arcs of training, ten 6-pounder Hotchkiss, four Maxim-Nordenfelt machine-guns, and three torpedo guns.

These vessels have a steel protective deck, with sloping sides from stem to stern, protecting the vitals of the ship; above and below the armour-deck the vessels are subdivided into a large number of water-tight compartments, and a portion of the vessel's supply of coal is employed to give additional protection.

With respect to the *Piemonte* the engines (vertical triple expansion) were designed and constructed by Messrs. Humphreys, Tennant, and Co. They are, in order that they may be wholly below the water line, of exceedingly short stroke, (27 inches), and the behaviour of the engines, both on their trials here and in the very severe weather to which the vessel was exposed on her passage out, amply justify these eminent engineers in their somewhat bold experiment.

I might describe other cruisers, both larger and smaller than those I have selected, but I must not fatigue you, and will only in this part of my subject draw your attention to these triumphs of engineering ingenuity and skill, I mean the torpedo boats, which (whether or not locomotive, torpedoes continue to hold their own as engines of destruction), are destined, I believe, to play no insignificant part in future naval warfare.

Let me illustrate the marvels that have been achieved by the great English engineers who have brought these vessels to their present state of perfection by giving you a few particulars concerning one or two of them.

A first-class torpedo boat by Yarrow has a length of 135 feet, a breadth of 14 feet, a displacement of 88 tons, and with engines of 1400 indicated horse-power attains a speed of a little over 24 knots.

A slightly larger boat, built for the Spanish Government by Thorneycroft, has a length of 147 feet 6 inches, a breadth of 14 feet 6 inches, and with engines of 1550 indicated horse-power, has attained a speed of a little over 26 knots.

It is interesting to note that the engines of the first-named torpedo boat develop nearly exactly the same power as those of the 90-gun ship, the *Cæsar*, and the engines of the second-named but little less than that developed by the *Duke of Wellington*, two vessels which you will remember I have taken as types of the second- and first-rate men-of-war of thirty-five years ago.

The weight of the engines of the *Duke of Wellington* and the *Cæsar* would be approximately 400 tons and 275 tons, while that of the torpedo boats is about 34 tons.

But if these results are sufficiently remarkable, the economy attained in the consumption of coal is hardly less striking.

The consumption of coal in the early steam battle-ships was from 4 to 5 lbs. per indicated horse-power per hour, and occasionally nearly reached 8 pounds.

At the present time in good performances the coal consumption ranges from $1\frac{1}{2}$ to $1\frac{3}{4}$ lbs. per indicated horse-power per hour under natural draught, and from 2 to $2\frac{1}{4}$ lbs. per hour with forced draught.

In war-ships the engines are designed to obtain the highest possible power on the least possible weight, and this for a comparatively short time, and, further, have to work at such various powers, that the question of economy must be a secondary consideration.

With the different conditions existing in the mercantile marine, more economical results may be expected, and I believe I shall not be far wrong in assuming that in special cases $1\frac{1}{3}$ lbs. may possibly have been reached; but I have not been able to obtain exact information on this head.

Turning now to the guns, let me refer first to those which were in use thirty-five years ago, and which formed the armaments of the ships of those days, and of the fortresses and coast defences of the United Kingdom and colonies.

The whole of these, with the exception of a few very light guns, were made of cast-iron. I have already alluded to the small amount of machine work (not of a very refined character) expended on them. Although the heaviest gun in use was only a 68-pounder, there were no less than sixty different natures of iron ordnance. Of the 32-pounder alone there were as many as thirteen descriptions, varying in length and weight. Of these thirteen guns, again, there were four separate calibres ranging from 6·41 inches to 6·3 inches, and as the projectile was the same for all, the difference fell on the windage. This varied, assuming gun and projectile to be accurate, from about 0·125 to 0·250, so that it may easily be conceived the diversity of the tables of fire for this calibre of gun were very great. And although from the simple nature of the guns, and the absence of anything like mechanical contrivance connected with them, it was quite unnecessary to give to them the care and attention that are absolutely indispensable in guns of the present day, it must not be supposed that they were altogether free from liability to accident and other defects.

I had occasion recently to look into the question of the guns employed in the siege of Sebastopol, and found that in that great siege no less than 317 iron ordnance were used by this country. At the close of the siege it was found that 8 had burst, 101 had been condemned as unserviceable, while 59 were destroyed by the enemy's fire.

The 95 cwt. 68-pounder gun seems to have been about the largest gun that could safely be made of cast-iron, and that in it the limit of safety was nearly reached, was shown by the fact that a serious percentage of this calibre burst or otherwise failed. With the spherical shot the column of metal per unit of area to be put in motion by the charge was small, and to this the guns probably owed their safety.

When the same charge was used, and cylinders representing double, treble, or quadruple the normal weight of the shot were fired, the end was rapidly reached, the guns frequently bursting before cylinders four or five times the weight of the shot were employed.

But the fact that a stronger and more reliable material than cast-iron was necessary, was shortly to be emphasized in a much more striking manner. The great superiority of rifled to smooth-bored ordnance in every respect, in power, in range, in accuracy, in destructive effect of shrapnel and common shell, was in this country demonstrated by Lord Armstrong and others. This led to numerous attempts to utilize cast-iron for rifled ordnance. The whole of these efforts resulted in failure. Although the charges were feebler than with smooth-bored guns, these experimental guns burst one after the other with alarming rapidity, generally before many rounds had been fired. The matter was not made much better when the expedient was adopted of strengthening these guns by hoops or rings shrunk on externally. Failures with this arrangement were little less frequent, the cast-iron bursting under the jackets, and the only plans in which cast-iron was used with any success were those proposed respectively by Sir W. Palliser and Mr. Parsons, who inserted, the one a coiled wrought-iron, and the other a steel tube in a cast-iron gun block.

But the country that suffered most severely from the use of cast-iron was the United States. Their great civil war took place just when efforts were being made in every country to introduce rifled artillery. Naturally every nerve was strained to manufacture these guns, and naturally the resources that came most readily to hand were first employed.

A report presented by the Joint Committee on Ordnance to the United States Senate in 1869 gives the history of these guns, which were nearly all either cast-iron or cast-iron reinforced with hoops in the way I have described. I have heard the existence of internal strains disputed, but in this report we read that ten guns burst, that is, flew to pieces, when lying on chocks, without ever having had a shot fired from them, and 98 others cracked or became ruptured under like conditions.

In the "Summary of Burst Guns" in the same report, it is stated that 147 burst and 21 were condemned as unserviceable; 29 of them being smooth-bore and 139 rifled ordnance. But perhaps the most striking passage is that which relates that in the action before Fort Fisher *all* the Parrott guns in the fleet burst, and that by the bursting of five of these guns during the first bombardment, 45 men were killed and wounded, while only 11 men were killed or wounded by the enemy's fire.

The muzzle velocity given by the smooth-bored, cast-iron guns may be taken approximately at 1600 f.s., and at the maximum elevation with which they were generally fired their range was about 3000 yards. The 32-pounder, with a charge of one-third the weight of the shot and an elevation of 10°, gave nearly 2800 yards, and the 68-pounder, with a charge of about one-fourth, nearly 3000 yards. The same gun, with an eccentric shot, and an elevation of 24°, gave a maximum range of 6500 yards.

But it must not be supposed because the range tables gave 3000 yards as practically the extreme range of the ordnance of 35 years ago, that our guns possessed any high efficiency at that distance. At short distances, from 300 to 500 yards, dependent on the calibre, the smooth-bored guns were reasonably accurate, but the errors multiplied with the distance in so rapidly increasing a ratio, that long before a range of 3000 yards was attained the chance of hitting an object became extremely small.

It is desirable to give some idea of the accuracy, or, rather, want of accuracy, of these guns.

In 1858 I was appointed secretary to the first Committee on Rifled Cannon, and the early experiments showing how extraordinary was the accuracy of the new weapons, it became a matter of importance to devise some means of comparing in this respect the old and the new guns.

The plan I proposed was one which has since been followed by the artillerists of nearly all countries. It was to calculate the probable error in range and the probable error in deflection, and from these data the area within which it would be an even chance that any given shot would strike; or, in other words, that area within which, out of a large number of rounds, half that number would fall. This area was for the smooth-bored gun at a range of 1000 yards, 147·2 yards long by 9·1 yards broad, or 1339·5 square yards, while the similar area for the rifled gun at the same range was 23·1 yards long by 0·8 yards broad, or an area of 18·5 square yards. But the great decrease of accuracy due to an increase of range with the smooth-bore guns is especially remarkable. Experiments showed that with the smooth-bored gun an increase of range of only 350 yards more than doubled the error in deflection, and made the area selected for comparison 206 yards long by 20·2 broad, or 4161 square yards, as nearly as possible trebling the area for an increase in range of 35 per cent.

But I have not done yet. These experiments were made with the same lots of powder carefully mixed, and the irregularities in velocity would be such as are due to manufacturers' errors only. But the variations in the energy developed by the gunpowder employed have still to be considered. In 1860, being then an associate member of the Ordnance Committee, I carried on for the Government the first electro-ballistic experiments made in this country. My attention was early called to the great variation in energy developed by powders recently made and professedly of the same make, and I pointed out that in my experiments the variations between one lot of powder and another amounted occasionally to 25 per cent. of the total energy developed. It is unnecessary to say that on service, and when

powder had been subjected to climatic influences, the variations would have been much greater.

The variations in energy of new powder were chiefly due to the method of proof then in use, the Eprouvette mortar, than which nothing can be conceived better adapted for passing into the service powders unsuitable for the guns of that time.

But with the want of accuracy of the gun itself, and the want of uniformity in the propelling agent, it may easily be conceived that a limit was soon reached beyond which it was mere waste of ammunition to fire at an object even of considerable size, and we can appreciate the reasons which led our naval commanders, whenever possible, to close with their enemy.

When we come to consider guns of the present day, the first point that attracts our attention is the enormous increase in the size and weight of the larger natures. It may fairly be asked, indeed, if, weight for weight, the modern guns are so much more powerful than the old, and, if we have command of such great ranges, why such heavy guns should be necessary.

The answer to this, of course, is that it has been considered essential to have guns capable of piercing at short distances the thickest armour which any ship can carry, and this demand has led us from guns of 5 tons weight up to guns of 110 and 120 tons weight, and to the development of the important mechanical arrangements for working them, to which I shall presently refer.

On the principles which guide the construction of these large guns I shall say little, both because the subject is too technical to be dealt with in an address, and because the practice of all nations, though differing in many points of detail, in essentials is closely accordant.

On three points of construction we lay particular stress in this country. These points are: That the gun shall be strong enough to resist the normal working pressure, even if the inner tube or barrel be completely split. That whether we regard the gun as a whole, or the parts of which it is composed, the changes of form should be as little abrupt as possible, and that any sharp angle or corner must be absolutely avoided.

As in principles of construction, so in material employed, is the practice of the great gun-making nations closely agreed. The steel employed is ductile and subjected to severe specifications and tests, which differ slightly one from the other, but exact, in effect, qualities of steel substantially the same. So far as I know, the application of the tests in this country is more severe than in any other, and I take this opportunity of entering my protest against the statement which I have seen more than once in the journals of the day—that English gun-steel is in any way inferior to any that is produced in any part of the world. Sheffield has in no respect lost its ancient reputation in the art of steel-making, and to my certain knowledge has supplied large quantities of steel, admitted to be of the first quality, to gun-makers of the Continent. The steel made by Sir J. Whitworth & Co. has likewise long been in great repute both at home and abroad, and looking at the care devoted to the subject by the Government, and the eagerness with which improvements in the quality and mode of manufacture are sought for and acted on by the steel-makers, we may be absolutely certain that to the best of our knowledge the most suitable material is used in the construction of our guns.

As many of you are aware, the mild steel which is used for the manufacture of guns is after forging and rough-boring subjected to the process of oil-hardening, being subsequently annealed, by which process it is intended that any detrimental internal strain should be removed. This process of oil-hardening, introduced first by Lord Armstrong in the case of barrels, is now almost universally adopted for all gun forgings. Of late, however, there has been considerable discussion as to whether or not this oil-hardening is necessary or desirable; and while admitting the increase of the elastic limit due to the process, it is asked whether the same results would not be obtained by taking a steel with, for example, a higher percentage of carbon, and which should give the same elastic limit, and the same ductility. The advocates of oil-hardening urge that steel with low carbon, duly oil-hardened to obtain the elastic limit and strength desired, is more reliable than steel in which the same results are reached by the addition of carbon. Those who maintain the opposite view point to the uncertainty of obtaining uniform results by oil-hardening, to the possibility of internal strains, and to the costly plant and delay in manufacture necessary in carrying it out. The question raised is undoubtedly one of great importance, but it appears to me to be one concerning which it is quite within our power in a comparatively short time, by properly arranged experiments, to arrive at a definite conclusion.

Sir F. Abel has in his Presidential Address given us so masterly a *résumé* of the present state of the steel question in its metallurgical and chemical aspects that it is unnecessary for me to add anything on this head. I will only remark that in selecting steel for gun-making, individually I should prefer that which is on the side of the low limit, to that which is near the high limit, of the breaking weight prescribed by our own and other Governments. I have this preference because, so far, experience has taught us that these lower steels are safer and more reliable than the stronger—and in guns we do not subject, and have no business to subject, the steel to stresses in any way approaching that which would produce fracture.

Of course if our metallurgists should give us a steel or other metal which with the same good qualities possesses also greater strength, such a material would by preference be employed, but it must not be supposed that the introduction of such new material would enable us, to any great extent, to reduce the weight of our guns. As a matter of fact, the energy of recoil of many of our guns is so high that it is undesirable in any case materially to reduce their weight. As an illustration, I may mention that some time ago, in re-arming an armour-clad, the firm with which I am connected was asked if by using the ribbon construction it would be possible, while retaining the same energy in the projectile, to reduce the weight of the main armament by three tons per gun. The reduction *per se* was quite feasible, but when the designs came to be worked out it was found that, on account of the higher energy of recoil, no less than 4 tons weight per gun had to be added to strengthen the mounting, the deck, and the port pivot fastenings.

The chamber pressures with which our guns are worked do not generally exceed seventeen tons per square inch, or say 2500 atoms. It must not be supposed that there is any difficulty in making guns to stand very much higher initial tensions; but little would be gained by so doing. Not only can a higher effect be obtained from a given weight of gun if the initial pressure be kept within moderate limits, but with high pressures the erosion (which increases very rapidly with the pressure) would destroy the bores in a very few rounds.

In fact, even with the pressures I have named, the very high charges now employed in our large guns (1060 lbs. have frequently been fired in a single charge), and the relatively long time during which the high temperature and pressure of explosion are maintained, have aggravated to a very serious extent the rapid wear of the bores. In these guns, if the highest charge be used, erosion, which no skill in construction can obviate, soon renders repair or relining necessary. Reduced charges, of course, allow a materially prolonged life of the bore, and there is also a very great difference in erosive effect between powders of different composition, but giving rise in a gun to the same pressures. Unfortunately, the powder which has up to the present been found most suitable for large guns is also one of the most erosive, and powder-makers have not so far succeeded in giving us a powder at once suitable for artillery purposes, and possessing the non-eroding quality so greatly to be desired.

An *amide* powder made by the Chilworth Company, with which I have, not long ago, experimented, both gave admirable ballistic results, and at the same time its erosive effect was very much less than that of any other with which I am acquainted. It is by no means certain that the powder would stand the tests which alone would justify its admission into the service, but the question of erosion is a very serious one, and has hardly, I think, received the attention its importance demands. No investigation should be neglected which affords any prospect of minimising this great evil.

On the introduction of rifled artillery the muzzle velocities, which you will remember had been with smooth-bore guns and round shot about 1600 f.s., were, with the elongated projectiles of the rifled gun, reduced to about 1200 f.s. In the battle between plates and guns these velocities were with armour-piercing projectiles gradually increased to about 1400 f.s., and at about this figure they remained until the appointment by the Government of a Committee on Explosives. By the experiments and investigations of this committee it was shown that, by improved forms of gunpowder and other devices, velocities of 1600 f.s. could be obtained without increasing the maximum pressure, and without unduly straining the existing guns. Similar advances in velocity were nearly simultaneously made abroad,

but in 1877 my firm, acting on independent researches on the action of gunpowder made by myself in conjunction with Sir F. Abel, constructed 6-inch and 8-inch guns which advanced the velocities from 1600 to 2100 f.s., and this great advance was everywhere followed by a reconstruction of rifled artillery.

With the present powder the velocities of the powerful armour-piercing guns, firing projectiles considerably increased in weight, may be taken at from 2000 to 2100 f.s. The distance of 3000 yards, which I said practically represented the extreme range of smooth-bored guns, is attained with an elevation of only 2° in the case of the 68-ton gun, and of $3\frac{1}{2}°$ in the 4·7-inch quick-firing gun, while at 10° the ranges are 9800 and 5900 yards respectively, and, as an instance of extreme range, I may mention that with a 9·2-inch gun a distance of over 13 miles has actually been reached.

Nor is the accuracy less remarkable. Bearing in mind the mode of comparison which I have already explained, at 3000 yards range the 68-ton gun would put half its shot within a plot of ground 7·2 yards long by 0·3 broad, and the 4·7-inch gun within a plot 19 yards long by 1·3 broad; or, to put it in another form, would put half their rounds in vertical targets respectively 0·92 yards broad by 0·34 yards high and 1·3 yards broad by 1·6 yards high.

But it cannot be assumed that we are at the end of progress. Already, with the amide powder we have obtained nearly 2500 f.s. in a 6-inch gun with moderate chamber pressures, and with the cordite originated by the Committee on Explosives, of which Sir F. Abel is president, considerably better results have been obtained. I have elsewhere pointed out that one of the causes which has made gunpowder so successful an agent for the purposes of the artillerist is that it is a mixture, not a definite chemical combination; that it is not possible to detonate it; that it is free, or nearly so, from that intense rapidity of action and waves of violent pressure which are so marked with nitro-glycerine and other kindred explosives.

We are as yet hardly able to say that cordite in very large charges is free from this tendency to detonation, but I think I may say that up to the 6-inch gun we are tolerably safe; at least, so far, I have been unable, even with charges of fulminate of mercury, to produce detonation. I need not remind you that cordite is smokeless, and that smokeless powder is almost an essential for quick-firing guns, the larger natures of which are day by day rising in importance.

I now come to the third part of my subject—the modes which are now adopted of mounting and working the ordnance I have described. I have alluded to the carriages, which, at the beginning of the century, were made of wood, and were worked solely by handspikes. Thirty-five years ago they were but little changed, although in the case of pivot guns screws for giving elevation, and blocks and tackle for training had been introduced, but timber was still the material employed. A strong prejudice long existed in both services against iron for gun carriages, as it was believed that iron carriages would be more difficult to repair, and that the effect on the crew of splinters would be much more serious.

But when the experiment of firing at both natures was made at Shoeburyness, with dummies to represent the crews, it was found both that the wooden carriage was far more easily disabled than the wrought iron, and that the splinters from the wooden carriages were far more destructive.

In all other respects, the superiority of wrought iron as regards unchangeability, durability, and strength, was so apparent, that iron, and later steel, rapidly displaced wood. No gun carriages, not even field, are now made of that material. It is impossible, within moderate limits, to give even a sketch of the various forms of mountings that have, as the science of artillery has progressed, been designed to meet the constantly changing conditions of warfare. I shall confine myself to the description of certain types of carriages, dividing these generally into three classes, viz., those for guns of the largest class, which require power to work them; those for guns of medium size, in which, by special arrangements, power is dispensed with; and those for guns of a smaller class, which are particularly arranged for extremely rapid fire.

With respect to the first class. On the adoption of heavily armed, revolving turrets of the Cowper-Coles type, in which the guns are trained for direction by revolving the turret, the first idea which naturally presented itself was to utilize steam power for this heavy work. It was, however, soon recognized that, on account of its elasticity steam did not give the necessary steadiness and control of movement essential for accuracy of aim, and water under pressure was employed as the means of transmitting the power from the steam-engine to the machinery for rotating the turret and working the guns.

On land, where an accumulator can be employed, a small steam-engine kept constantly at work is used; but at sea, where accumulators, whether made to act by the pressure of steam, air, or springs, are inadmissible, a very much larger engine is employed sufficiently powerful to supply water to perform all the operations ever carried on together. When little or no work is required, the engine automatically reduces its speed till it merely creeps, so that little or no power is consumed.

The mode of mounting the guns differs somewhat according as they are intended to be placed in a barbette or in a turret. Our guns have gradually been increased in length, and are now so long (our largest has a length of nearly 45 feet) that it is impossible to provide an armoured turret of sufficient size to protect the forward part of the gun, and under these circumstances it is a grave question whether it is worth while to devote so much armour to the protection of what is after all the strongest part of the gun.

Of the eight new battle-ships now building, seven are to have their guns mounted *en barbette*, and one is to be provided with armoured turrets. In either case the guns and their machinery are carried on revolving turntables of practically the same form. These turntables are placed in an armoured redoubt, and the guns, when horizontal, are entirely above the armour, but in the case of the ship provided with turrets the breech ends of the guns are covered in, with the turrets placed as an addition on the turntables.

The extra weight required thus to protect the breech ends of the guns is for this ship about 550 tons.

As the hydraulic machinery for these new ships differs but slightly from that fitted on ships of the *Rodney* and *Nile* classes, the same description will cover all these vessels. The armoured barbette battery at each end of the ship is made of a pear shape, in order to provide for a pair of ammunition hoists and hydraulic rammers at its narrower end.

These ammunition hoists come right up into the armoured barbette and descend to the shell-room and magazine decks, forming the channel by which the projectiles and charges are rapidly supplied to the guns; and it must be remembered that the weight to be lifted for a single round, including powder and projectile, with the necessary cases, considerably exceeds a ton. The cage in each hoist is worked by hydraulic cylinders with double wire-ropes, and in case of breakage, automatic safety gear is fitted to arrest and lock the cage.

While on the ammunition deck the cages are charged simultaneously from either side, and when hoisted to the battery-deck are automatically slowed, and then stopped at the proper position for loading the guns, much depends upon the service of ammunition by these hoists being protected from interruption, and in the event of derangement of the cage, independent tackle, worked by an hydraulic capstan, is provided to take its place, and a few rounds can also be stowed within the battery.

In intimate connection with the ammunition hoists are the hydraulic rammers on the ammunition deck for charging the cages, and in the battery for loading the guns. To reduce their length within reasonable limits they are made telescopic, and they are fitted with indicators to show when the charges are home.

In the shell-rooms hydraulic cranes and traversing bogies are fitted to convey the shell to the base of the ammunition hoist, so that a projectile is transported from the place where it is stowed to the shot-chamber of the gun without manual labour of any sort except that of moving the various levers to set the hydraulic machinery in motion. In the magazines hydraulic bollards are provided for hoisting and transporting the powder-cases by means of overhead runners. Hand-gear is provided as an alternative in both magazine and shell-rooms.

Each turntable carrying the guns and their fittings is rotated by a pair of entirely independent three-cylindered engines, each engine being of sufficient power to rotate the turntable at the speed of one revolution per minute. The gear for controlling them is worked from two or three look-out stations, at either or any of which the officer has to his hand the means of elevating, training, sighting, and firing either one or both guns. The turning-engines are fitted with a powerful spring break, which will hold in a seaway, but which is taken off automatically when the water is admitted to start the engines. Easy control is obtained by

the use of servo-motor valves, so that the handwheel is small and requires but little power to move it. It only remains to describe as shortly as possible the system of mounting the guns on the turntable. The guns are trunnionless, to allow them to be as close together as possible, with the view of reducing to the smallest possible size the diameter of the turntables. The carriages are cradles of steel grooved to correspond with rings turned on the guns, and with straps by which the guns are secured to the cradles. The carriages are mounted without rollers or wheels on slides formed of steel beams of great strength, pivoted at their front ends and supported on hydraulic presses, by which they are bodily raised or lowered to give the guns elevation or depression. In the case of the turret this system gives the smallest possible port. The loading of the gun is effected while the gun is at extreme elevation, a position which is easily determined by dropping the slide on to fixed stops, and which gives the best protection for the breech mechanism, for the hoist and rammers. The operations of unlocking the breech-block, withdrawing it, traversing it, inserting a loading tray, and, after completing the loading, performing the same operations in reverse order, are all done by hydraulic power, and the fittings are so devised that, unless the gun is properly locked and run out, it cannot be fired.

In certain foreign vessels provided with the hydraulic breech mechanism, a valve has been arranged which makes in their proper order, and in that order only, the eight or ten movements necessary to open and close the breech of the gun, but this system has not been adopted in our own navy.

The sights are carried on the top of the turntable, or, in the case of a turret, on the turret roof, and are worked automatically by an arc attached to the gun slide, gearing into cog-wheels, with shafting reaching to each sighting position.

The system of recoil press adopted on all these ships is that which lends itself most readily to employment also as a running-in-and-out press. It consists of a simple cylinder carried in the middle of the slide, having working in it a ram with piston, attached at the front end to the carriage. Spring-loaded valves are placed in the recoil ram piston and at the end of the cylinder, and by these the water escapes when the gun recoils. The water which passes through the cylinder valves runs to the exhaust-pipe, while that which passes through the piston valve remains in the front of the cylinder, and prevents the gun charging out again. When the recoil press is used to run the gun in and out, these valves are inoperative, as they are loaded much above the working pressure in the hydraulic mains. The high pressure of recoil does not enter the hydraulic mains, as the supply to the rear of the press, where alone the high pressure of recoil exists, is made backwards and forwards, through a valve which shuts itself automatically when not in use.

Before leaving the working by power of heavy guns, there is one example of mounting a pair of guns *en barbette* which, although it has many points in common with the system I have just described, has also some points of difference, which it may be worth while to note.

Objections have sometimes been urged to the fixed loading station on the ground that it is necessary to bring the guns to it and lock them there until sponged and loaded, thereby involving, not only a loss of time, but, under certain conditions, exposing them more to the enemy's fire.

In ships of the *Re Umberto* type, what is termed an all-round loading is obtained by bringing up the ammunition through a central hoist to the deck below the turntable. From this central hoist it is transferred to two other hoists, which are carried on the turntable behind the guns. The transfer is made by hand for the powder, and by sliding down a tray for the projectile, this work being performed by men on the deck below the turntable. The hydraulic rammers are fixed to the turntable, and are very much shortened by being made with more rams. In spite of this arrangement, however, the hoists are rather cramped, and the breech mechanism has to be made to pass from behind the gun, so as to permit the gun to recoil, and the gun is rather further forward than usual when run out.

With these reservations, however, the system has advantages: the reduction in the armour required to protect the turntable and its machinery is considerable, and the redoubt being round instead of pear-shaped, presents a smaller and stronger surface to the enemy when broadside on.

I very much doubt, however, whether with this system there can be any advantage in rapidity of fire. Training to the loading station is in our navy very quickly done, and the turntable is rotated while the guns are being run in or out.

It is hardly necessary to say that hydraulic machinery for guns was worked out by my friend and late partner Mr. George Rendel, and up to the end of 1881 all details connected therewith were made under his management.

I ought perhaps to give you some idea of the rate at which these heavy guns worked by power can be fired.

In the case of the *Benbow*, with the 110-ton gun the time from "load" to "ready" was $2\frac{1}{2}$ minutes. In the firing trials of the *Trafalgar* four rounds were fired from one of her 68-ton guns in 9 minutes 5 seconds. In the *Colossus*, when under command of Captain Cyprian Bridge, the average from one round to another was 1 minute 45 seconds, and on one occasion, steaming at 8 knots per hour past a target at a distance of 1500 yards, she fired four rounds in six minutes, striking the target three times.

Of the mountings which are worked solely by manual power, the whole range for naval service is covered by the carriages of the type designed by Mr. Vavasseur. No single description can be made to cover all the varieties of these mountings which have been worked out to meet the diverse conditions which have arisen in the re-arming of old ships, and the fitting out of new vessels on modern and novel designs. The very general adoption of breech-loading ordnance brought with it the necessity for a mounting which would give easier access to the breech of the gun than was obtained with the long low gun-slide employed with the muzzle-loading guns. The main features of the type, therefore, are: a high slide, very short, so as not to project beyond the breech of the gun, a short low carriage carrying on either side the recoil presses, and a shield to afford protection both to the carriage and the gun crew.

The increased importance of rapid-fire guns has led in later carriages to a strong armour plate being built into the mounting as part of its structure, and to this must be added the shield above mentioned, so that the total protective thickness of plate is very considerable.

By means of a worm wheel sliding on a keyed shaft the movement of the gun for elevation or depression can be made up to the instant of firing—a decided and very important advance on the older methods.

The arrangement of the recoil-cylinders is peculiar. They are fitted with a pair of pistons with rotating valves, so adjusted as to be open when the gun is in the firing position, and to be gradually closed during recoil by studs running along rifled grooves in the cylinders; by this ingenious contrivance the area of the ports of the valves is increased and then decreased in proportion to the variation of the velocity of recoil, so that the liquid passes from one side of the piston to the other at as nearly as possible a constant velocity and under a constant pressure. The velocity of the flow through the ports, and therefore the pressure of the liquid, varies with the energy of the recoil of the gun, so that the length of the recoil is with all charges practically the same.

Even a blank charge produces nearly full recoil, and on one occasion caused one of these mountings to be reported as unserviceable, and unfit to fire a shotted round. Constant length of recoil has the advantage over constant pressure in the recoil-presses that, in the event of an unusually heavy recoil, a higher pressure in the recoil-press would in the former case be the only result, and would do no harm, as the pressure would still be much below the test-pressure; but in the latter case there would be an increased length of recoil, and, unless considerable margin were allowed, a possible destruction of the slide.

Most frequently the Vavasseur mountings are made with central pivots, and there is then little tendency for the movements of the vessel to affect the mounting, and as the weight is borne upon a ring of live rollers the greatest ease of training is obtained.

In the larger sizes the centre pivot is increased in size, and made hollow so as to provide for the passage through the centres of a powder hoist, which, after rising high enough, curves to the rear under the gun and delivers its charge at the point where it can most conveniently be drawn out for insertion in the gun. In this case a foot plate is also provided as a rear attachment to the slide, and from this the crew work the gun. This foot plate is provided with boxes for eight or ten projectiles, which are therefore ready for use at any moment and in any position of training. These mountings are fitted in the belted cruisers of

the *Orlando* class, one being carried at the fore and one at the after end of each ship.

As a sufficient proof of the value of these mountings and of the ability which has been displayed in their design, I may mention that practically all countries have adopted these carriages for modern guns, either without any alteration or with comparatively unimportant modification.

In discussing our modern ordnance I only alluded to quick-firing guns, because in their case the gun and mounting are so closely connected, the efficiency of the system depending as much upon the one as the other, that a separate description of either would be incomplete, and they are more easily described together. The great success which attended the small Hotchkiss and Nordenfelt three- and six-pounder guns led me to consider whether the same principle could not be applied to large guns, and we designed and made at Elswick the 4·7 inch and 5·5 inch quick-firing guns which were so successfully tried by the *Excellent* at Portsmouth. Subsequently, with the co-operation of Mr. Vavasseur, various improvements were made, and for the sake of uniformity in calibre a 6-inch was substituted for the 5·5-inch gun.

One of the peculiarities of these guns is in the form of the breech-screw which, while on the principle of the interrupted screw, is made conical, so as to simplify the action of opening and closing—the principle of the ordinary rifle cartridge has been extended to the ammunition for these guns. This not only allows extremely rapid loading, but secures safety from premature explosions in rapid firing. The cartridges are fired electrically, and, not having their own ignition, there is no danger of exploding them either when stowed in the magazine or if accidentally dropped in the handling.

To follow the rapid movements of a torpedo boat it is essential that there should be the most perfect control over the gun and mounting, and the most effective mode of rapid fire is to keep the gun always on the object aimed at, allowing the gun itself to fire as the breech is closed. The captain stands at the side of the gun, shielded by a guard-plate from the recoil, his shoulders braced against a shoulder-piece which is unaffected by the recoil; his eye aligns the sights; with one hand he works the elevating or training wheel, and with the other grasps the firing-trigger, or, for rapid firing, the training-wheel may be thrown out of gear, and direction given by the shoulder-piece alone. The mounting is a centre pivot, and, being on live rollers, turns with the least effort. The gun has no trunnions, but slides in a carriage which envelopes it like a sleeve. The trunnions are on this carriage, so that the two are together pivoted like an ordinary gun in a fixed lower carriage. There is no preponderance when the gun is in the forward position, and the recoil lasts for so short a time that the disturbance of the centre of gravity is not felt on the elevating-gear or shoulder-piece. The lower side of the carriage is formed into a recoil press, the piston-rod of which is attached to a horn on the rear of the gun.

There is also a spring-box, with rod attachments to the horn, by which the gun is instantly run out as soon as the recoil is expended. Efficient shields are provided to protect the crew. The revolving weight of the gun and mounting is 5 tons; yet, with the shoulder against the shoulder-piece, it can be swung through 90° in 2 seconds, and with the gear can be trained through the same arc in 5 seconds. It is possible to fire from this gun at the rate of 10 to 12 rounds per minute, and on one occasion 10 rounds were fired in 47 seconds; but perhaps the most striking experiment with the gun was made at Shoeburyness, when five rounds were fired in 31 seconds at a 6′ × 6′ target at 1300 yards, all of which struck the object aimed at.

A trial has also been recently made between two cruisers, the one armed with ordinary breech-loading, the other with quick-firing artillery, from which it appears that when firing at a target the latter, in a given time, was able to discharge about six times the quantity of ammunition fired by the former. I need not impress upon you the significance of these facts or the importance of quick-firing armaments, especially if firing shell, possibly charged with high explosives, against the unarmoured portions of cruisers or other vessels.

The accuracy and the shell power of rifled guns have naturally had their effect upon the mountings for the land service, experiments having conclusively shown that batteries armed with guns placed in ordinary embrasures would soon be rendered untenable. Among the expedients that have been adopted or suggested to meet the altered conditions, the system of making the gun disappear behind a parapet or into a pit, with which the name of Colonel Moncrieff has been so long and so honourably associated, is more and more coming into favour as the most effective mode of protection for the gun and its mounting, as well as for the gun detachment. During the last ten years much attention has been devoted to the designing of various mountings on this system for all weights of guns from 3 up to 68 tons.

In the earliest carriages of this type the gun was raised by the descent of a balance weight, but the most successful arrangement is that in which compressed air is employed for the purpose. The 9·2-inch and 10-inch hydro-pneumatic mountings are the largest sizes as yet adopted into the English service, and a description of them will serve for that of the type generally.

The gun on this system is raised by compressed air stored in several chambers, and acting through the medium of a fluid upon a recoil ram.

On the recoil of the gun the liquid is driven from the cylinder by the incoming ram into the lower parts of the air chambers, so that as much as is required of the energy of recoil is stored up by the compression of the air, and is used to raise the gun for the next round. The gun is raised up and lowered on two heavy beams pivoted to the lower carriage. Two long light elevating rods, pivoted at one end to the breech of the gun, at the other to the lower carriage, hold the gun in correct position as it rises or falls; the elevation is changed by moving the position of the lower ends of the elevating rods. This can be done when the gun is down without disturbing it, and consequently with very little labour. The effect of the change is apparent after the gun rises, when any slight correction can be made if desired. Generally these mountings have been made with overhead shields placed a little below the level of the top of the gun pit, and entirely closing it. There is an aperture through which the gun rises, but which can be closed when the gun is out of action.

In the case of the 10-inch gun the total weight of the revolving mass is 80 tons. Only two men are required at the hand-wheels to revolve it—in fact, it is within the power of *one* man to do the whole work. The ordinary speed of training is 90° in 1¼ minute, while the time required to raise the gun to the firing position is 20 seconds. The speed of rising might be considerably increased, but, taking the weight of the mass in motion into account, it does not appear to be desirable to accelerate it.

At Maralunga, Spezia, in March of the present year, the first 68-ton disappearing mounting, manufactured for the Italian Government, was tried with most satisfactory results. Fifteen rounds were fired in all, some of them being made to give greatly increased energy of recoil, with the view of proving the gun and mounting.

The gun was worked entirely by hand-power, and on land no difficulty is experienced in thus dealing with it, while the system possesses the advantage that it is always ready for use should it be required, but no great alteration is necessary to adapt the mounting for use with hydraulic power.

In this case the water from the recoil press is driven through spring-loaded valves instead of into air chambers. There is, therefore, no storing up of the recoil energy, and to raise the gun to the firing position, water pressure from an accumulator kept charged by a steam-pumping engine in the usual way is employed. These guns and mountings are too large to be easily covered by an overhead shield, but they are provided with shields at the front and rear to protect the gun detachments.

Another very successful mounting for land service has been made for guns when the site is such that it is permissible to place them *en barbette*. The gun is entirely above the parapet, but the detachment is protected while loading and working the gun by a broad sloping shield carried on the gun carriage and recoiling with it. The shield is inclined so that any splinters, &c., striking it, may be deflected in an upward direction.

The carriage runs back on a long slide inclined at 5°, and at the end of the recoil is caught by a spring catch, which retains it in the run in position until the loading is finished. To load, the gun is put at extreme elevation, so that the breech may be as much under protection as possible, the charge being rammed home with a hand rammer worked by rope tackle. The slide is mounted on front and rear rollers, and has an actual central pivot. The recoil is controlled by a single Vavasseur recoil cylinder placed in the centre of the slide, and giving a constant length of recoil for all charges, so that the spring catch to retain the gun at extreme recoil for loading is always reached.

To run out after loading, the spring catch is released, and the

incline of the slide is sufficient to cause the gun to run out, which it does smartly, but is checked and brought to rest quietly by means of a controlling ram placed at the end of the recoil press.

But I must conclude. I trust I have said enough to satisfy you as to the indebtedness of the naval and military services to mechanicians and to mechanical science, but you will also understand that within the limits of an address it is impossible to give a complete survey of so large a subject, and that there are important fields I have left wholly untouched.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY JOHN EVANS, D.C.L., LL.D., D.SC., TREAS.R.S., PRES.S.A., F.L.S., F.G.S., PRESIDENT OF THE SECTION.

IN the year 1870 I had the honour of presiding over what was then the Department of Ethnology in the Biological Section of the British Association at its meeting in Liverpool. Since that time twenty years have elapsed, during the greater portion of which period the subjects in which we are principally interested have been discussed in a Department of Anthropology forming part of the organization of the Biological Section; although since 1883 there has been a new Section of the Association, that of Anthropology, which has thus been placed upon the same level as the various other sciences represented in this great parliament of knowledge. This gradual advance in its position among other branches of science proves, at all events, that, whatever may have been our actual increase in knowledge, Anthropology has gained and not lost in public estimation, and the interest in all that relates to the history, physical characteristics, and progress of the human race is even more lively and more universal than it was twenty years ago. During those years much study has been devoted to anthropological questions by able investigators, both in England and abroad; and there is at the present time hardly any civilized country in the world in which there has not been founded, under some form or another, an Anthropological Society, the publications of which are yearly adding a greater or less quota to our knowledge. The subjects embraced in these studies are too numerous and too vast for me to attempt even in a cursory manner to point out in what special departments the principal advances have been made, or to what extent views that were held as well established twenty years ago have had either to be modified in order to place them on a surer foundation, or have had to be absolutely abandoned. Nor could I undertake to enumerate all the new lines of investigation which the ingenuity of students has laid open, or the different ways in which investigations that at first sight might appear more curious than useful have eventually been found to have a direct bearing upon the ordinary affairs of human life, and their results to be susceptible of application towards the promotion of the public welfare. I may, however, in the short space of time to which an opening address ought to be confined, call your attention to one or two subjects, both theoretical and practical, which are still under discussion by anthropologists, and on which as yet no general agreement has been arrived at by those who have most completely gone into the questions involved.

One of these questions is: What is the antiquity of the human race, or rather what is the antiquity of the earliest objects hitherto found which can with safety be assigned to the handiwork of man? This question is susceptible of being entirely separated from any speculations as to the genetic descent of mankind; and, even were it satisfactorily answered to-day, new facts might to-morrow come to light that would again throw the question entirely open. On any view of probabilities, it is in the highest degree unlikely that we shall ever discover the exact cradle of our race, or be able to point to any object as the first product of the industry and intelligence of man. We may, however, I think, hope that from time to time fresh discoveries may be made of objects of human art, under such circumstances and conditions that we may infer with certainty that at some given point in the world's history mankind existed, and in sufficient numbers for the relics that attest this existence to show a correspondence among themselves, even when discovered at remote distances from each other.

Thirty-one years ago, at the meeting of this Association at Aberdeen, when Sir Charles Lyell, in the Geological Section, called attention to the then recent discoveries of Palæolithic implements in the Valley of the Somme, his conclusions as to their antiquity were received with distrust by not a few of the geologists present. Five years afterwards, in 1864, when Sir Charles presided over the meeting of this Association at Bath, it was not without reason that he quoted the saying of the Irish orator, that "they who are born to affluence cannot easily imagine how long a time it takes to get the chill of poverty out of one's bones." Nor was he wrong in saying that "we of the living generation, when called upon to make grants of thousands of years in order to explain the events of what is called the modern period, shrink naturally at first from making what seems so lavish an expenditure of past time. Throughout our early education we have been accustomed to such strict economy in all that relates to the chronology of the earth and its inhabitants in remote ages, so fettered have we been by old traditional beliefs, that even when our reason is convinced, and we are persuaded that we ought to make more liberal grants of time to the geologist, we feel how hard it is to get the chill of poverty out of our bones."

And yet of late years how little have we heard of any scruples in accepting as a recognized geological fact that, both on the Continent of Europe and in these islands, which were then more closely connected with that continent, man existed during what is known as the Quaternary period, and was a contemporary of the mammoth and hairy rhinoceros, and of other animals, several of which are either entirely or locally extinct. It is true that there are still some differences of opinion as to the exact relation in time of the beds of river gravel containing the relics of man and the Quaternary fauna to the period of great cold which is known as the Glacial period. Some authors have regarded the gravels as pre-Glacial, some as Glacial, and some as post-Glacial; but, after all, this is more a question of terms than of principle. All are agreed, for instance, that in the eastern counties of England implements are found in beds posterior to the invasion of cold conditions in that particular region, though there may be doubts as to how much later these conditions may have prevailed in other parts of this country. All, too, are agreed that since the deposit of the gravels considerable changes have taken place in the configuration of the surface of the country, and that the time necessary for such changes must have been very great, though those in whose bones the chill of poverty still clings are inclined to call in influences by which the time required for the erosion of the river valleys in which the gravels occur may be theoretically diminished.

On the other hand, there have been not a few who, feeling that the evidence of the existence of the human race has now been satisfactorily established for Quaternary times, and that there is no proof that what has been found in the ordinary gravels belongs to anything like the first phases of the family of man, have sought to establish his existence in far earlier Tertiary times. In the view that earlier relics of man than those found in the river gravels may eventually be discovered, most of those who have devoted special attention to the subject will, I think, concur. But such an extension of time can only be granted on conclusive evidence of its necessity; and, before accepting the existence of Tertiary man, the grounds on which his family-tree is based require to be most carefully examined.

Let me say a few words as to the principal instances on which the believer in Tertiary man relies. These may be classified under three heads[1]: (1) the presumed discovery of parts of the human skeleton; (2) that of animal bones said to have been cut and worked by the hand of man; and (3) that of flints thought to be artificially fashioned.

On most of these I have already commented elsewhere.[2] Under the first head I may mention the skull discovered by Prof. Cocchi at Olmo, near Arezzo, with which, however, distinctly Neolithic implements were associated; the skeletons found at Castelnedolo, of which I need only say that M. Sergi, who described the discovery, regarded them as the remains of a family party who had suffered shipwreck in Pliocene times; and the fossil man of Denise, in the Auvergne, mentioned by Sir Charles Lyell, who may have been buried in more recent times under lava of Pliocene date. On these discoveries no superstructure can be built. The Calaveras skull seems to have better claims

[1] See A. Arcelin, "L'Homme Tertiaire," Paris, 20 rue de la Chaise, 1889.

[2] Trans. Herts. Nat. Hist. Soc., vol. i. p. 145; "Address to the Anthrop. Inst., 1883," *Anth. Journ.*, vol. xii. p. 565.

to a high antiquity. It is said to have been found at a depth of 153 feet in the auriferous gravels of California, containing remains of mastodon, and covered by five or six beds of lava or volcanic ashes. But here again doubts enter into the case, as well-fashioned mortars, stone hatchets, and even pottery, are said to occur in the same deposits. In the same way the discoveries of M. Ameghino at the mouth of the Plata, in the Argentine Republic, require much further corroboration.

The presumably worked bones which I have placed in the second category, such as those with incisions in them from St. Prest, near Chartres, the cut bones of Cetacea in Tuscany, the fractured bones in our own crag-deposits, and numerous other specimens of a similar character, have, by most geologists, been regarded as bearing marks entirely due to natural agencies. It seems more probable that in bones deposited at the bottom of Pliocene seas, cuts and marks should have been produced by the teeth of carnivorous fish, than by men who could only have lived on the shores of the seas, and who have left behind them no instruments by which such cuts as those on the bones could have been produced.

As to the third category, the instruments of flint reported to have been found in Tertiary deposits, those best known are from St. Prest and Thenay, in the north-west of France, and Otta, in Portugal.

These three localities I have visited; and though at the two former the beds in which the flints were said to have been found are certainly Pliocene, there is considerable doubt in some cases whether the flints have been fashioned at all, and in others, where they appear to have been wrought, whether they belong to the beds in which they are reported to have been found, and have not come from the surface of the ground. Even the suggestion that the flints of Thenay were fashioned by the Dryopithecus, one of the precursors of man, has now been retracted. At Otta the flakes that have been found present, as a rule, only a single bulb of percussion, and, having been found on the surface, their evidence is of small value. The exact geological age of the beds in which they have occurred is, moreover, somewhat doubtful. On the whole, therefore, it appears to me that the present verdict as to Tertiary man must be in the form of "Not proven."

When we consider the vast amount of time comprised in the Tertiary period, with its three great principal subdivisions of the Eocene, Miocene, and Pliocene, and when we bear in mind that of the vertebrate land animals of the Eocene no one has survived to the present time, while of the Pliocene but one—the hippopotamus—remains unmodified, the chances that man, as at present constituted, should also be a survivor from that period seem remote, and against the species *Homo sapiens* having existed in Miocene times almost incalculable. The *à priori* improbability of finding man unchanged, while all the other vertebrate animals around him have, from natural causes, undergone more or less extensive modification, will induce all careful investigators to look closely at any evidence that would carry him back beyond Quaternary times; and though it would be unsafe to deny the possibility of such an early origin for the human race, it would be unwise to regard it as established except on the clearest evidence.

Another question of more general interest than that of the existence of Tertiary man is that of the origin and home of the Aryan family. The views upon this subject have undergone important modification during the last twenty years. The opinions based upon comparative philology alone have received a rude shock, and the highlands of Central Asia are no longer accepted without question as the cradle of the Aryan family, but it is suggested that their home is to be sought somewhere in Northern Europe. While the Germans contend that the primitive Aryans were the blue-eyed dolichocephalic race, of which the Scandinavians and North Germans are typical examples, the French are in favour of the view that the dark-haired brachycephalic race of Gauls, now well represented in the Auvergne, is that of the primitive Aryans. I am not going to enter deeply into this question, on which Canon Isaac Taylor has recently published a comprehensive treatise, and Mr. Frank Jevons a translation of Dr. Schrader's much more extensive work, "The Prehistoric Antiquities of the Aryan Peoples." Looking at the changes that all languages undergo, even when they have the advantage of having been reduced into the written form, and bearing in mind the rapidity with which these changes are effected; bearing in mind, also, our extreme ignorance of the actual forms of language in use among prehistoric races unacquainted with the art of writing, I, for one, cannot wonder a something like a revolt having arisen against the dogmatic assertions of those who have, in their efforts to reconstruct early history, confined themselves simply to the comparative study of languages and grammar. But, notwithstanding any feeling of this kind, I think that all must admire the enormous industry and the varied critical faculties of those who have pursued these studies, and must acknowledge that the results to which they have attained cannot lightly be set aside, and that, so far as language alone is concerned, the different families, their provinces, and mutual relations have, in the main, become fairly established. The study of "linguistic palæontology," as it has been termed, will help, no doubt, in determining still more accurately the affinities of the different forms of language, and in fixing the dates at which one separated from another, as well as the position that each should occupy on the family-tree—if such a tree exists. But even here there is danger of relying too much on negative evidence: and the absence in the presumed original Aryan language of special words for certain objects in general use ought not to be regarded as affording absolute proof that such objects were unknown at the time when the languages containing such words separated from the parent stock. Not only Prof. Huxley, but Broca and others have insisted that language as a test of race is as often as not, or even more often than not, entirely misleading. The manner in which one form of language flourishes at the expense of another; the various ways in which a language spreads even otherwise than by conquest; the fact that different races, with totally different physical characteristics, are frequently found speaking the same language, or but slightly different dialects of it—all conduce to show how imperfect a guide comparative philology may be so far as anthropological results are concerned. Of late, prehistoric archæology has been invoked to the aid of linguistic researches; but here again there is great danger of those who are most conversant with the one branch of knowledge being but imperfectly acquainted with the other. The different conditions prevailing in different countries, the degrees of intercourse with other more civilized nations, and local circumstances which influence the methods of life, all add difficulties to the laying down of any comprehensive scheme of archæological arrangement which shall embrace the relics, whether sepulchral or domestic, of even so limited an area as that of Europe. We are all naturally inclined to assume that the record of the past is comparatively complete. But in archæology no more than geology does this appear to be the case. The interval between the period of the river-gravels and that of the caves, such as Kent's Cavern, in England, and those of the Reindeer period of the south of France, may have been but small; but our knowledge of the transition is next to none. The gap between the Palæolithic period and the Neolithic has, to my mind, still to be bridged over, and those who regard the occupation of the Belgian caves as continuous from the days of the reindeer down to late Neolithic times seem to me possessed of great powers of faith. Even the relations in time between the *kjökkenmöddings* of Denmark and the remains of the Neolithic age of that country are not as yet absolutely clear; and who can fix the exact limits of that age? Nor has the origin and course of extension of the more recent Bronze civilization been as yet satisfactorily determined; and until more is known, both as to the geographical and chronological development of this stage of culture, we can hardly hope to establish any detailed succession in the history of the Neolithic civilization that went before it. In the meantime, it will be for the benefit of our science that speculations as to the origin and home of the Aryan family should be rife; but it will still more effectually conduce to our eventual knowledge of this most interesting question if it be consistently borne in mind that they are but speculations.

Turning from theoretical to practical subjects, I may call attention to the vastly improved means of comparison and study that the ethnologists of to-day possess as compared with those of twenty years ago. Not only have the books and periodicals that treat of ethnology multiplied in all European languages, but the number of museums that have been formed with the express purpose of illustrating the manners and customs of the lower races of mankind has also largely increased. On the Continent, the Museums of Berlin, Paris, Copenhagen, and other capitals have either been founded or greatly improved; while in England our ethnological collections infinitely surpass, both in the number of objects they contain and in the method

of their arrangement, what was accessible in 1870. The Blackmore Museum at Salisbury was at that time already founded, but has since been considerably augmented. In London, also, the Christy collection was already in existence, and calculated to form an admirable nucleus around which other objects and collections might cluster; and, thanks in a great degree to the trustees of the Christy collection, and in a far greater degree to the assiduous attention and unbounded liberality of the keeper of the department, Mr. Franks, the ethnological galleries at the British Museum will bear comparison with any of those in the other European capitals. The collections of prehistoric antiquities, enlarged by the addition of the fine series of urns and other relics from British barrows explored by Canon Greenwell, which he has generously presented to the nation, and by other accessions, especially from the French caverns of the Reindeer period, is now of the highest importance. Moreover, for purposes of comparison the collections of antiquities of the Stone and Bronze periods found in foreign countries is of enormous value. In the ethnological department the collections have been materially increased by the numerous travellers and missionaries which this country is continually sending forth to assist in the exploration of the habitable world; and the student of the development of human civilization has now the actual weapons, implements, utensils, dress, and other appliances of most of the known savage peoples ready at hand for examination, and need no longer trust to the often imperfect representations given in books of travel. But besides the collection at Bloomsbury there is another most important Museum at Oxford, which that University owes to the liberality of General Pitt-Rivers. It is arranged in a somewhat different manner from that in London, the main purpose being the exhibition of the various modifications which ornaments, weapons, and instruments in common use have undergone during the process of development. The skilful application of the doctrine of evolution to the forms and characters of these products of human art gives to this collection a peculiar charm, and brings out the value of applying scientific methods to the study of all that is connected with human culture, even though at first sight the objects brought under consideration may appear to be of the most trivial character.

So far as the museums more intimately connected with anthropology are concerned, the advance that has been made has been equally well marked. The osteological collections both at the Royal College of Surgeons and at the Natural History Museum have received important accessions, especially in the craniological department; and the notable addition of the Barnard Davis collection to that previously existing in Lincoln's Inn Fields has placed the Museum of the College in the foremost rank. The Museums at Oxford and Cambridge have also received most important accessions: the one, of the Greenwell collection from British barrows; the other, of the Thurnam collection of skulls.

The value of the small hand-book for travellers, issued under the title of "Anthropological Notes and Queries," has been proved by the necessity for a new edition, towards which the British Association has made a grant. Some delay in the publication of the new issue has taken place, but I hope that the report of the Committee in charge of the work may give assurance of the book being now in a forward state.

The feasibility of assigning trustworthy marks for physical qualifications in candidates for posts either in the military or civil departments of the State has now for some time been attracting more or less of public attention, and the subject has been taken up by the Council of this Association. The result of their communications on this subject with the Government has been made known in their Report, and I need not enter into the history of the correspondence that has passed upon the question. Whatever course may at the present time be adopted, we may, I think, feel confident that eventually due weight will have to be attached to physical capacity in selection for appointments in the military branch of the public service, for which, indeed, at the present time a medical examination has to be passed. Thanks to the ingenuity of Mr. Francis Galton and others, we have now instruments at our command, not only for testing muscular force, breathing capacity, and other bodily characteristics, but also for ascertaining the closeness and rapidity of connection between the organs of seeing and hearing, and the action of the muscles required to be brought into play. In these experiments nervousness, no doubt, is to some extent a factor, but perhaps the rough-and-ready test of the South American commander was, for ascertaining the presence or absence of nervousness, even more effective. When promotion of some officer was about to be made upon the field, the general caused all the possible candidates to be arranged around him, each armed with a flint and steel and a cigarette, and he who first was satisfactorily smoking was promoted then and there.

Connected with the question of general physical capacity is that of the proper appreciation of colours, the absence of which is a fruitful source of danger, both by land and at sea. It is, indeed, impossible to say how often an apparently inexplicable accident may not have arisen from some form of colour-blindness, such as the inability to distinguish red from green, in a person in charge of a ship, a train, or of points on a railway. True, there are some forms of examination to be gone through, both by mariners and railway officials, with the view of testing their powers and correctness of vision; but it is very doubtful whether the tests employed or the manner in which the examinations are conducted can be regarded as in all respects satisfactory. For the purpose of investigating the phenomena, and, if possible, the physical causes of colour-blindness and allied defects of vision, and also with the view of suggesting improvements in the methods of determining the existence of such defects in candidates for maritime or railway employment, the Council of the Royal Society has appointed a Special Committee. Its labours, however, are not yet finished, and no report has hitherto been received from the Committee. I mention the subject as one in which all anthropologists will be interested, and the importance of which must be universally acknowledged. The most singular feature in the case is that the subject, though carefully investigated by several private inquirers, should have waited so long before being submitted to some public or quasi-public body for investigation.

The subjects of an anthropological survey of the tribes and castes in our Indian possessions, and of the continued investigation of the habits, customs, and physical characteristics of the North-Western tribes of the Dominion of Canada, were both recommended for consideration to the Council of this Association by the General Committee at the meeting at Newcastle. We have heard from the Report of the Council what has been done in the matter. The rapidity with which the various native tribes in different parts of the world are either modified, or in some cases exterminated, affords a strong argument for their characteristics, both physical and mental, being investigated without delay.

There are, indeed, now but few parts of the world the inhabitants of which have not, through the enterprise of travellers, been brought more or less completely within our knowledge. Even the centre of the dark African continent promises to become as well known as the interior of South America, and to the distinguished traveller who has lately returned among us anthropologists as well as geographers owe their warmest thanks. It is not a little remarkable to find so large a tract of country still inhabited by the same diminutive race of human beings that occupied it at the dawn of European history, and whose existence was dimly recognized by Homer and Herodotus. The story related by the latter about the young men of the Nasamones who made an expedition into the interior of Libya and were there taken captive by a race of dwarfs receives curious corroboration from modern travellers. Herodotus may, indeed, slightly err when he reports that the colour of these pygmies was black, and when he regards the river on which their principal town was situated as the Nile. Stanley, however, who states that there are two varieties of these pygmies, utterly dissimilar in complexion, conformation of the head, and facial characteristics, was not the first to rediscover this ancient race. At the end of the sixteenth century, Andrew Battel, our countryman, who, having been taken captive by the Portuguese, spent many years in the Congo district, gave an account of the Matimbas, a pygmy nation of the height of boys of twelve years old; and in later times Dr. Wolff and others have recorded the existence of the same or similar races in Central Africa. Nor must we forget that for a detailed account of an Acca skeleton we are indebted to the outgoing President of this Association, Prof. Flower. It is not, however, my business here to enter into any detailed account of African exploration or anthropology. I have made this incidental mention of these subjects rather from a feeling that in Africa, as well as in Asia and America, native races are in danger of losing their primitive characteristics, if not of partial or total extermination, and that there also the anthropologist and naturalist must take the earliest possible opportunities for their

researches. Already the day is past when the similitude drawn by Anaxilas between music and Africa holds good, and even Cornelius Agrippa could no longer maintain that he "sayeth not amisse: By God, sayeth he, Musicke is even like Affricke; it yearely bringeth foorth some straunge Beaste."[1]

I have, however, said enough on what I feel are somewhat vague and general topics, and will now ask you to devote your attention to the business of the Section, when, no doubt, many subjects of interest will be more particularly discussed.

[1] "Vanitie of Sciences," cap. 17.

NOTES.

Within the next few days the National Association for the Promotion of Technical and Secondary Education will issue a brief "Guide to Evening Classes in London," which is the first attempt to give a systematic account of the educational work carried on in such classes throughout the metropolis. The Guide will be classified according to subjects and districts, so that an intending student can see at a glance the place, day, and hour at which classes are available in any particular subject in the district in which he lives, as well as the fee, name of instructor, and other details. The price will be 6*d*., and the publishers will be Messrs. Cassell and Co.

The following is a list, in brief, of subjects on which the Dutch Society of Sciences at Haarlem invite research:—A history of the mathematical and physical sciences in Holland; isomorphism; minerals in the river and dune sands on the Dutch coast; the accessory sexual glands in mammalia; heat liberated in solution of various salts in water; decomposition of water or other liquids by disruptive electric discharges within or on the surface; influence of compression in different directions on specific inductive power; determination of the form and position of the reticular micrometers used by Lacaille at the Cape of Good Hope; influence of volume of molecules on pressure of a gas; relation between density and chemical composition of transparent bodies, and the index of refraction; modification of reflected light by magnetization of some other metal than iron; methods of obtaining and fixing new varieties in cultivated plants; *rôle* of bacteria in filtration of portable waters through a layer of sand; bacteria and azotized combinations in the soil; healing after grafting.

The Report of the Director of the Hong Kong Observatory for 1889 states that a self-recording anemometer, rain-gauge, and sunshine-recorder, have been erected by the Imperial Maritime Customs at the important station of South Cape, Formosa, and the observations are received monthly at the Observatory. Among the investigations in progress are: the collection of information respecting typhoons, from the logs of men-of-war stationed in those seas, and an investigation of the climate of Hong Kong from five years' observations; this latter work is nearly ready for press. The Report contains an interesting comparison of spectroscopic rain-band observations with the rainfall during the subsequent 24 hours; Dr. Doberck considers that the indications frequently foretold great thunderstorms which could not otherwise have been forecast from local observations. On May 29 and 30, 1889, the colony was visited by thunderstorms of unusual duration; above 22·5 inches of rain fell in 24 hours, causing floods and serious damage to property.

The Journal of the Franklin Institute for September contains several interesting papers. Few can speak with more authority on "Precious Stones" than Mr. George F. Kunz, and his lecture delivered in February last, before the Franklin Institute, is replete with information respecting them. Under the heading "Electricity in Warfare," Lieutenant Bradley A. Fiske, U.S.N., comments upon the present condition of the art, indicates in what ways electricity is now actually employed, and what is the direction of progress. Mr. Joseph M. Wilson, the President of the Institute, continues his paper on schools, with particular reference to trades schools, and gives an account of the method of work set forth in the Science and Art Directory and in the Prospectus of the Normal School of Science. Among the other papers are the following: On fresh-water wells of the Atlantic beach, by Mr. Persifor Frazer. On the strength of gear teeth, by Mr. Samuel Webber; and on the electrolytic method as applied to palladium, by Messrs. E. F. Smith and H. F. Keller.

The *Monthly Weather Review* issued by the Meteorological Service of the Dominion of Canada consists of telegraphic reports of observations received for the purpose of weather predictions and of reports of storms received by mail. Tables of temperature, pressure, wind, and precipitation are given, together with the records of sunshine and auroræ. The total number of storm warnings issued last month was 93, of which 77 or 82·8 per cent. were verified. Of the 77 warnings in connection with the direction of the wind, 66 or 85·7 per cent. were fully verified, and 72 or 93·5 partly verified. The steps made in the prediction of weather form an important factor nowadays in commercial life, and in Canada forecasts are posted up nightly at every telegraph station.

We have received from America the summary of the weather during the last month, and also the forecast for September. The review shows that on the whole fine weather has prevailed, although occasionally disturbed by a few storms. The first storm of note was central on the 14th, about lat. 55° N., longitude 25° W., and was accompanied by moderate to strong gales, and high seas; the second moved from Southern New England to Nova Scotia on the 27th. The forecast indicates fine weather, with occasional gales north of the 35th parallel. Less fog will be found along the transatlantic steamship routes, and little ice will be encountered off the Grand Banks. An accompanying chart gives a brief but complete statement as regards these dangerous storms. A new series of storm signals at Havana were commenced in August, 1889, and this year night-signals have been added, details of these being given in an accompanying table. There is also a list of charts that have been published and corrected during the month of August, and information respecting dangerous obstructions to navigation along the coast.

We have received from Mr. Edward Stanford, a *résumé* of the publications of the Ordnance Survey for England and Wales, with an introductory description of the survey by Major Francis P. Washington, R.E. The new 1-inch general map is reduced from the 6-inch maps, and will consist of 360 small sheets, 178 of which have been already published. This survey is well adapted for walking or driving purposes, and residential maps can be made up for 10, 15, or 20 miles round any centre. In this catalogue the particulars of each county are given in alphabetical order, and the mounting details will be found very useful to those who use maps to a great extent. At the end of the pamphlet there are some illustrations of various neat and handsome methods of mounting these maps for both library and schoolroom purposes.

A pamphlet on "Acoustics in Relation to Wind Instruments," by D. J. Blackley, consists of a series of lectures given by him to the students at the Royal Military School of Music, Kneller Hall, in May 1887. They have been revised and somewhat amplified, and now form a general sketch of the subject under consideration, and will be useful to those desirous of understanding the principles underlying the construction and use of wind-instruments, the illustrations of wave-motion given in them not being confined only to experiments with cylindrical tubes. There is an appendix on musical tch, which has been

written with special reference to military and orchestral wind bands.

IN the number of *La Nature* for September 6 there is a detailed account of an ingenious application of the properties of iodide of nitrogen to photometry. The photometer, invented by M. Lion, is based on the fact that equal surfaces of iodide of nitrogen, preserved under its mother-liquor, and exposed for equal times to lights of equal intensities, evolve equal quantities of nitrogen. Two vessels are connected by a differential manometer, and when the rate of evolution of the nitrogen is the same in each, the manometer is unaffected. It is stated that the iodide of nitrogen, kept in the mother-liquors in which it has been prepared, is perfectly safe to handle. In practice, owing to the difficulty of exactly balancing the two halves of the apparatus, a method analogous to "weighing by substitution" is employed. The accuracy attainable in the measurements is not stated. In the same number another photometer of considerable theoretical interest is described. It is the invention of MM. Seguy and Verschaffel, and was described by them on September 1 at the Academy of Sciences, Paris. It is based upon the principle of Crookes' radiometer, but the disks, instead of being free to rotate, are suspended by a silk fibre, and with an indicating needle and divided circle, form a torsion balance. An alum cell is placed in front of the instrument, which, as a photometer, appears to be very sensitive, indicating 1–100th of a standard candle. Moreover, two instruments can be constructed, which with light of the same intensity give the same readings, an important practical advantage. So long as these instruments are used to compare lights of the same quality, there seems to be no doubt that they can both be made to yield results of practical value, and comparable with each other. It appears doubtful, however, whether the same figure would be obtained with the chemical and with the mechanical photometer, if used to compare the illuminating powers of two sources of light that differed much in character, such as an arc lamp, and a candle flame.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♀) from West Africa, presented by Mrs. Roupell; a Sykes's Monkey (*Cercopithecus albigularis* ♀) from East Africa, presented by Mr. M. Tanner; two Bonnet Monkeys (*Macacus sinicus* ♂ ♀), a Toque Monkey (*Macacus pileatus* ♀), two Ring-necked Parrakeets (*Palæornis torquatus*) from India, presented by Mrs. Julie Rule; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. W. Dodson; a Grey Ichneumon (*Herpestes griseus* ♂) from India, presented by Master Stanley Kerfoot; a Brush-tailed Porcupine (*Atherura africana*) from West Africa, presented by the Liberian Government Concessions and Exploration Co., Lt.; a Common Viper (*Vipera berus*) from Hampshire, presented by Mr. W. H. B. Pain; a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, deposited; two Vinaceous Turtle Doves (*Turtur vinaceus*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on September 18 = 21h. 51m. 4s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4695	—	—	21 40 2	− 9 20
(2) G.C. 4734	—	—	21 55 27	+17 12
(3) 75 Cygni	5	Yellowish-red.	21 35 52	+42 47
(4) α Aquarii	3	Yellowish-white.	22 0 6	− 0 51
(5) θ Pegasi	3	White.	22 4 24	+ 5 39
(6) 249 α Schj.	6	Red.	21 37 23	+35 1
(7) R Scuti	Var.	Red.	18 41 36	− 5 50

Remarks.

(1 and 2) No record has yet been made of the spectrum of either of these objects. The first is described in the General Catalogue as a nebulous star, or a very small cluster; the second as "pretty bright; pretty small; round; brighter in the middle to a nucleus; mottled as if with stars; star south preceding." No very bright nebulæ are near the meridian at 10 o'clock during the present week.

(3) The spectrum of this star is a very interesting one of Group II. Instead of the spectrum being totally discontinuous, as in α Herculis and others, the bands 2, 3, 7 are well marked, whilst 4, 5, and 8 are so feeble as to be hardly visible. This species of spectrum has been explained by supposing that the meteor-swarm is still sparse and the carbon radiation consequently bright. When the positions of the feeble bands are considered, it will be seen that the explanation is complete. Band 8 extends from about λ 503·5 to λ 496, and this will therefore be partly masked by the extremity of the brightest carbon fluting starting near λ 517. Bands 4 and 5 both come within the range of the second carbon fluting, starting near λ 564, and they also will be partly obliterated when the carbon flutings are wide. None of the other bands, however, will suffer from masking in this way, and they therefore should remain dark. It will be interesting if this explanation can be tested by a direct observation of an unusual width or brightness of the carbon flutings.

(4) This star has a spectrum of Group III., and may be observed as a study of criteria.

(5) A star of Group IV. (Vogel).

(6) This is a typical star of Group VI., showing in addition to the ordinary carbon bands no less than six of the secondary bands, namely 2, 3, 4, 5, 7, and 8. Dunér remarks that the two latter are undoubtedly bands, and not lines; their wave-lengths are 551 and 528 respectively, the latter almost agreeing with E of the solar spectrum. The general spectrum consists of four zones—that is, there is a certain amount of light beyond the carbon band commencing at 474. An observation of the precise character of this band would be interesting; in comets it sometimes ends abruptly at 474, sometimes fades away gradually on both sides at 468, and sometimes has two maxima, one at 468 and one at 474.

(7) Like S Vulpeculæ, referred to last week, this is a variable of comparatively short range and short period, but whereas the spectrum of the former is known to be one of Group II., that of the latter has yet to be determined. Although the period is but 168 days, we have not as yet any record of the light-curve of the star, which promises to be an interesting one from the fact that the maximum is stated as 4·7–5·7 and the minimum as 6·0–8·5 (Gore). If the spectrum be one of Group II. the shortness of the period suggests that the bands should be rather narrow, and this may be made a test observation. There will be a maximum about September 23. A. FOWLER.

THE URANIA GESELLSCHAFT.—An interesting account of the Urania Institution at Berlin appears in the publications of the Astronomical Society of the Pacific, vol. ii., No. 9. The account was originally written for Prof. Holden by Dr. M. Wilhelm Meyer, the director of the institution. It appears that Prof. Foerster, the director of the Berlin Observatory, first proposed the formation of an observatory that should be open to the public, and his proposition was afterwards modified so as to include other branches of natural science. The project was warmly supported by Herr von Gossler, the Prussian Minister of Public Instruction, a grant of land was made, and in March 1888 a sort of joint stock company was formed having for its object simply the diffusion of knowledge. The idea having thus taken tangible form, the work of construction was begun. On July 2, 1889, the institution was opened to the public, and at the end of the year had been visited by 60,000 persons.

The astronomical department has been the main attraction from the beginning. It contains a twelve-inch refractor equatorially mounted, and electrically controlled and illuminated. The instrument is provided with a filar micrometer, a polarizing telescope, and a complete set of eye pieces ranging in power from 70 to 1300 diameters. Unfortunately neither spectroscopic nor photographic accessories have yet been supplied. Five other telescopes are possessed by the Urania Observatory, viz. a six-inch refractor, a four-inch refractor, a six-inch reflector, a two and a half inch transit instrument, and a comet seeker of five inches aperture.

The lecture theatre which forms a part of this magnificent institution, is fitted with every convenience and provided with a lantern having a light of 6000 candle-power for the projection of views on a screen. The lectures that are delivered are not all, however, devoted to astronomy, but cover the other subjects within the scope of the institution.

The Physical Department is even better supplied with apparatus than the Astronomical, and it is so arranged that visitors, by pressing different buttons, may view the spectra of various substances, the phenomena of polarization, and many electrical effects. The recent presentation of two complete phonographs by Mr. Edison gives the science collection of this Department a still higher value. A Microscopical Department is also included, and affords instruction to many.

An exceedingly well-illustrated journal, *Himmel und Erde*, has been published monthly since the foundation of the institution, and is issued free to all the members. From the recently-published Report, it appears that the cost of production of this journal considerably exceeds the receipts from subscribers, but we are glad to know that the Urania Institution is too firmly established to need its discontinuance. The number of visitors during the 268 days on which the doors of the institution have been opened is 95,000. Three hundred and thirteen lectures of about ninety minutes long have been delivered, and five hundred and eighty-two of thirty minutes duration.

Prof. Holden points out that the Lick Observatory, like the Urania Institution, is devoted to the advance of scientific knowledge, and we hope with him that the success of the latter may lead to the establishment of similar institutions in Europe and America. The opening of observatories would be much appreciated by the general public, for doubtless the Urania Gesellschaft owes much of its popularity to this step in the right direction that its directors have taken.

WASHBURN OBSERVATORY.—Mr. G. C. Comstock has issued the sixth volume of the "Publications of the Washburn Observatory," of which establishment he has been director since 1887. The first part contains observations with the meridian circle by Miss A. M. Lamb and Mr. Milton Updegraff. The second part is devoted to observations of double stars, by Mr. Comstock, and includes the measurement of double stars discovered at this observatory, and described in vols. i. and ii. of its *Publications*. The instrument employed for all of the measures was the $15\frac{1}{2}$-inch Clark equatorial telescope. As soon as the necessary apparatus is ready, a determination of the constant of aberration will be made by Loewy's method.

NEW ASTEROIDS (297) AND (298).—Two new minor planets were discovered on the 9th inst. by M. Charlois of Nice Observatory. One of them may prove to be Aschera (214).

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 8.—M. Duchartre in the chair.—On a sulphocarbide of platinum, by M. P. Schutzenberger. By passing a current of dry nitrogen charged with the vapour of carbon bisulphide over spongy platinum contained in a glass tube heated to about 400° C., the carbon bisulphide was entirely absorbed, and the platinum converted into a finely divided black powder. An examination of the product showed that it had the composition Pt_2S_2C, which may be graphically represented thus:—

$$C\begin{cases} \diagup\!\!\!\diagup Pt = S \\ \diagdown Pt = S \end{cases}$$

The powder is very dense, and appears entirely homogeneous when microscopically examined. Neither hydrochloric nor nitric acid have any action upon it, and even with *aqua regia* there is very little action. When heated to redness in dry oxygen the powder burns, with the formation of carbon dioxide, and sulphur monoxide and dioxide, leaving a residue of pure platinum.—New researches on the gadoline of M. de Marignac, by M. Lecoq de Boisbaudran. The results of a spectroscopic examination of gadoline are given. The substance is shown to have a characteristic spectrum, thus confirming the view held by M. de Marignac, viz. that it is a new element.—On a property of certain systems of forces, by M. L. Lecornu.—On the soluble ferment of urea, by M. P. Miguel. From various considerations the author has been led to believe that in ammoniacal fermentation, the microphytes always act on the urea by means of the soluble ferment discovered by M. Musculus (Annales de Micrographie, vols. 1 and 2), and that it is not necessary to adopt the hypothesis of the destruction of urea by an act of nutrition, in order to explain the alkaline fermentation of urine.—Post-embryonic development of the kidney of the *Ammocete* by M. L. Vialleton.—On modifications of ophitic rocks of Modon (Province de Séville) by M. Salvador Calderon.—On a carboniferous bed discovered at Quenon in Saint-Aubin-d'Aubigne by M. Bezier.—Revival of the activity of Vesuvius, by M. Wiet, Consul at Naples. This is an extract of a letter to the Foreign Minister, giving an account of the actual state of Vesuvius. Lava has been issuing from an opening formed last year, and is slowly descending the central cone. Prof. Maiorano has observed that the volcanic activity of the fumaroles has ceased, and that only a small column of smoke ascends from them. It is also noted that the smoke issuing from the various openings has a different appearance from the steam and vapour generally visible.—Additional note on the extension to Switzerland of the storm of August 19, by M. Bourgeat.

BRUSSELS.

Academy of Sciences, July 5.—M. Stas in the chair.—The following communications were made:—On the ten-monthly astronomical period, by M. F. Folie.—On the characteristic property of the common surface of two liquids, by M. G. van der Mensbrugghe. The author has previously studied the properties of the common surface of two liquids which do not mix, and now gives the results of a similar study of liquids having an affinity for each other, *e.g.* water and ether or alcohol.—On new observations of the canals of Mars, and on their duplication, by M. F. Terby.—Some observations made by Mr. Stanley Williams during April and May 1890 are shown to support Schiaparelli's conclusions with respect to the nature of the surface of Mars. A plate is given containing eight views of Mars made this year by the two above-named observers.—A coronula from the gulf of St. Lawrence, by M. P. J. Van Beneden.—The Actinozoa specimens obtained by Prof. Hensen in his Plankton expedition, by Edouard Van Beneden. A larva related to that found by Semper in 1867, by the same author.—On the constitution of benzopinacoline β, by M. Maurice Delacre.—On primary co-variants, by M. Jacques Deruyts.—On the biographical notice of G. A. Hirn, recently inserted in the *Bulletin de l'Académie*, by Prof. Dwelshauvers-Dery.—Contributions to the study of the Nebenkern, by Dr. E. Leclercq.

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THURSDAY, SEPTEMBER 25, 1890.

THE GOLDEN BOUGH.

The Golden Bough: a Study in Comparative Religion. By J. G. Frazer, M.A. In Two Volumes. (London: Macmillan and Co., 1890.)

THE object of this book is to offer a probable explanation of the priesthood of Nemi. The method adopted is to show that such barbarous customs as those associated with that priesthood were also carried on elsewhere; and "if we can detect the motives which led to its institution; if we can prove that these motives have operated widely, perhaps universally, in human society, producing in varied circumstances a variety of institutions specifically different but generically alike; if we can show, lastly, that these very motives, with some of their derivative institutions, were actually at work in classical antiquity; then we may fairly infer that at a remoter age the same motives gave birth to the priesthood of Nemi."

The author, Mr. Frazer, informs us in his preface that he has for some time been preparing a general work on primitive superstition and religion. We are glad to learn from the same source that his studies to this end have been systematized, encouraged, and influenced by Mr. W. Robertson Smith. The book shows from cover to cover how important this influence has been, and how thorough has been the work done; it is a perfect mine of early folk-lore, while the method of arrangement and the way in which the facts are marshalled along the different lines of inquiry, leaves nothing to be desired.

It must be understood, however, that this is not the general work to which we have referred above. It is an *excursus* on a special point, an attempt to solve the difficult problem connected with the hitherto unexplained rule of the Arician priesthood.

Having said thus much on the origin of the book, we may next proceed to remark that in such a case as this criticism pure and simple of the details is almost out of the question. We prefer rather to lay before the readers of NATURE a summary of the various steps employed in the argument, accompanied by references to those points which we have found of special interest.

The priesthood of Nemi is one of the most extraordinary character, and has no parallel in classical antiquity. The sacred grove and sanctuary of Diana Nemorensis, or Diana of the Wood, lay on the northern shore of the lake, right under the steep cliffs on which stands the modern village of Nemi. The lake and grove were sometimes called the lake and grove of Aricia, but the town of Aricia was distant three miles. There grew in this grove a certain tree, around which there might almost always be seen a strange figure prowling. The man carried a drawn sword, and persistently looked about him as if he expected every moment to be set upon by an enemy. He was the priest, and also a murderer; and "the man for whom he looked was sooner or later to murder him and hold the priesthood in his stead. Such was the rule of the sanctuary. A candidate for the priesthood could only succeed to office by slaying the priest; and having slain him he held office till he was himself slain by a stronger or a craftier."

The author begins by stating the few facts and theories bequeathed to us on the subject of the priesthood of Nemi. The first questions that he attempts to answer concern the title of this priest. Why was he called the king of the wood? Why was his office spoken of as a kingdom? To obtain an answer to the first question he has to go into the details of the facts, legends, &c., recorded of primitive man, and to see whether there were such beings as kings of rain, water, fire, &c., to match the Arician king who bore the name of king of the wood. In this search he brings out a wonderful array of interesting facts as regards sympathetic magic, rain making, sunshine making, controlling the wind, fighting the wind, &c.; and § 3, which treats of incarnate gods, is full of examples, "drawn from the beliefs and practices of rude peoples all over the world, which may suffice to prove that the savage, whether European or otherwise, fails to recognize those limitations to his power over nature which seem so obvious to us."

Having found instances of kings of rain, water, and fire, the author next looks for a king of the wood. Since the worship of trees played an important part among the religious ideas of the Aryan race in Europe, a king of the wood ought to be found closely connected with tree worship, and so it happens. Innumerable instances of this form of worship have been got together, showing the way in which trees were looked upon at an early stage of civilization. Men supposed that the trees had souls, that tree spirits could give rain and sun, and that the harvests were dependent on them. "In Sumatra, so soon as a tree is felled, a young tree is planted on the stump; and some betel and a few small coins are also placed on it. Here the purpose is unmistakable. The spirit of the tree is offered a new home in the young tree planted on the stump of the old one, and the offering of betel and money is meant to compensate him for the disturbance he has suffered." The may-pole of to-day is only an emblem of the old form of tree-worship—a survival of the belief in the fertilizing power of the tree spirit.

The author then discusses the manner in which the tree spirit is "conceived and represented as detached from the tree and clothed in human form, and even as embodied in living men and women," a great number of examples being given. He also gives instances of the double representation of the spirit of vegetation by a tree and a living man. Before concluding this chapter, the question is discussed as to whether these forms of tree-worship help to explain the priesthood of Aricia. He believes they do. "In the first place the attributes of Diana, the goddess of the Arician grove, are those of a tree spirit or sylvan deity. Her sanctuaries were in groves, indeed every grove was her sanctuary, and she is often associated with the wood-god Silvanus in inscriptions. . . . Like a tree spirit, she helped. . . . She was the patroness of wild animals. . . ." He then goes on to suggest that the king of the wood may have been, like the "king of May, the grass king, and the like, an incarnation of the tree spirit or spirit of vegetation, &c."

The next chapter, consisting of a little over 100 pages, deals with the peril of the soul. The royal and princely taboos which kings had to undergo in order to uphold their sacred character in the minds of their subjects are first described, among which we may mention the following. They were compelled to live in a state of seclusion.

Before strangers entered a district they (the strangers) had to undergo certain ceremonies, so as to be disarmed of their magical powers, which might do harm to the king. Great precaution must be observed at meals, in order that they might not be seen eating or drinking, &c. An interesting description of the Mikado's mode of life, written two hundred years ago, illustrates well some of these taboos.

A great number of instances relating to the various ideas of what the soul is and of what it can do are given. Thus, "it is a common rule with primitive people not to awaken a sleeper, because his soul is away and might not have time to get back; so if the man wakeneth without his soul he would fall sick." Some people believe a man's soul to be in his shadow, others in the reflection of his form by water; thus the "Zulus will not look into a dark pool, because they think there is a beast in it which will take away their reflections, so that they die." Very curious instances are given of people running after souls, the methods adopted for catching them, examples of the recall and recovery of souls, &c.

Chapter iii. concerns the "killing of the god." As the author showed in the preceding chapter that the divine priest or king had to undergo horrible taboos, so in this one he points out that, in consequence of the great value attached to his life, the only means of preserving it from inevitable decay necessitated a violent death. He applies this argument to the king of the wood. "He too had to be killed, in order that the divine spirit, incarnate in him, might be transferred in unabated vigour to his successor. The rule that he held office till a stronger should slay him might be supposed to secure both the preservation of his divine life in full vigour, and its transference to a suitable successor as soon as that vigour began to be impaired." In order to confirm the conjecture that the king of the wood was formerly put to death at the expiration of a set time, the author first of all finds the evidence that can be adduced of a custom of periodically killing his counterparts, the human representatives of the tree spirit. As an illustration of these we may mention that in Saxony and Thuringia there is a Whitsuntide ceremony called "chasing the wild man out of the bush," or fetching the wild man out of the wood, the tree spirit or spirit of vegetation being represented by the wild man.

The next step taken in the argument is to show that the "custom of killing the god, and the belief in his resurrection, originated, or at least existed, in the hunting and pastoral stage of society, when the slain god was an animal; and survived into the agricultural stage, when the slain god was the corn, or a human being representing the corn." To do this a great number of examples are taken into consideration; the spring customs of the European peasantry are referred to, among which the most important are known as "burying the carnival" and "driving or carrying out death." The ceremonies carried on in connection with Osiris, Adonis, Thammuz, Attis, and Dionysus by the Egyptians, Syrians, Babylonians, Phrygians, and Greeks, were similar to those in Northern and Western Europe demonstrating the death and resurrection of vegetation. We may here mention that although some writers identify Osiris with the sun, the author is inclined to treat him as a deity of vegetation. Here we fancy modern Egyptologists who are not dependent either upon Diodorus or Plutarch will join issue with him. In like manner Dionysus, though he is often conceived and represented in animal shape, is here understood to be a deity of vegetation, for "the custom of killing a god in animal form . . . belongs to a very early stage in human culture, and is apt in later times to be misunderstood. The advance of thought tends to strip the old animal and plant gods of their bestial and vegetable husk, and to leave their human attributes (which are always the kernel of the conception) as the final and sole residuum. In other words, animal and plant gods tend to become purely anthropomorphic."

In the remaining few pages of this chapter the spirit of vegetation is discussed in examples of the corn spirit; the various names given to this spirit in the different countries being the old man, the old woman, corn mother, maiden, &c. In all these cases the idea is that the spirit of the corn is driven out of the corn last cut or last threshed, and lives in the barn during the winter. Hence the idea brings us in presence of the Egyptian view that Osiris represents the latent Ra. At sowing-time he goes out again to the fields to resume his activity as an animating force among the newly sown corn.

In some cases human sacrifices were made to promote the fertility of the fields. Among many examples given is that of the Indians of Guayaquil (Ecuador), who sacrificed human blood and the hearts of men when they sowed their fields; and there are instances when the victims for these sacrifices were actually kept and fattened up in order that the crops might be good, and that their death might insure immunity from all disease and accidents.

The second volume begins with examples of the corn spirit being represented in animal forms, such as a gander, goat, hare, cat, and fox. During the course of this discussion the author connects this corn spirit in animal form with the ancient deities of vegetation—Dionysus, Demeter, Adonis, Attis, and Osiris. He first of all points out, with the help of numerous references to ancient authorities, how these deities were represented in animal form: Dionysus was represented as a goat and sometimes as a bull; Demeter as a pig, &c. He then argues that since the corn spirit was represented by animals, such as pigs, goats, &c., these animals are nothing more nor less than the corn gods in animal form.

The next point he wishes to prove is that the "custom of killing the god had been practised by peoples in the hunting, pastoral, and agricultural stages of society;" the gods whom the hunters and shepherds adored and killed were "animals pure and simple, not animals regarded as embodiments of other supernatural beings." Of the many examples given concerning this point, we will here give a short extract of the bear festival of the Ainos:—

"Towards the end of winter a young bear is caught and brought into the village. At first he is suckled by an Aino woman; afterwards he is fed on fish. When he grows so strong that he threatens to break out of the wooden cage in which he is confined, the feast is held. But it is a peculiarly striking fact that the young bear is not kept merely to furnish a good meal; rather he is regarded and honoured as a fetish or even as a sort of higher being."

A curious fact about these feasts is that at their conclusion the Ainos always apologize to their gods, saying that the bear has been treated well, only he got too strong for them to keep any longer.

Having thus shown that the custom of killing the god was practised in the hunting, pastoral, and agricultural times, the author points out another aspect of the custom, that of laying on the dying god all the accumulated misfortunes and sins of the whole people. He begins by showing us first how each individual got rid of his sins by transferring them to some person, animal, or thing; then he points out the methods adopted by the inhabitants of villages, towns, &c., for getting rid of their sins wholesale. Some used to drive them into the sea, others used to go through their own village and smash everything, so as to drive them out. Among the principal methods employed was that of the scapegoat. A goat, laden with the sins of the people, was sent out of the village. Sometimes a boat was used as a scapegoat, and sent adrift to sea, filled with provisions and branches of trees in which were placed all the sins and diseases of the people. Human beings were sometimes used as scapegoats and were sacrificed; and the employment of divine men or animals was by no means rare. Thus it appears "that human sacrifices of the sort I suppose to have prevailed at Aricia were, as a matter of fact, systematically offered on a large scale by a people whose level of culture was probably not inferior, if indeed it was not distinctly superior to that occupied by the Italian races at the early period to which the origin of the Arician priesthood must be referred. . . ."

Of the two questions asked at the commencement of this work—Why had the priest of Nemi to slay his predecessor? and Why, before doing so, had he to pluck the golden bough?—the first has been answered, and it only remains to find the answer to the second in the last chapter. The author first inquires what the golden bough was. He begins by mentioning some of the rules or taboos by which the life of the divine kings or priests is regulated, the two chief ones being that they must neither behold the sun nor touch the ground for a specified time. These taboos were intended to preserve the life of the divine person, together with the life of his subjects and worshippers, and the reason why they were suspended between heaven and earth was that their lives were then considered safe and free from any harm. In the description of the Mikado's mode of life it is stated that it would be prejudicial to his dignity and holiness to touch the ground with his feet, and that he should not expose his head to the sun, as its rays are not worthy to shine on it. During the course of this inquiry the author finds out that "these two rules—not to touch the ground and not to see the sun—are observed either separately or conjointly by girls at puberty in many parts of the world," and that they are kept in close confinement, the object of this seclusion being to neutralize "the dangerous influences which are supposed to emanate from them at such times." In these taboos the sun and earth were looked upon as media through which evils or diseases might be transferred, and in order to prevent bad consequences kings and women between certain ages had to undergo this period of isolation and confinement to minimize the chances of infection.

He next gives an account of the myth of a god, whose life "in a sense might be said to be neither in heaven nor earth, but between the two." This was the Norse Balder, the good and beautiful god, who was invulnerable, but who was eventually killed by having a piece of mistletoe thrown at him, and then burnt on a funeral pile. In this section the author traces out what he supposes to be the origin of this myth. He finds that its two main features, the pulling of the mistletoe and the burning of the god, were reproduced in the great midsummer festival of the Celts; and in Sweden there were midsummer fires, known as Balder's bale-fires, which "puts their connection with Balder beyond the reach of doubt, and makes it certain that in the former times either a living representative or an effigy of Balder must have been annually burned in them." He then remarks that "customs of this kind can be traced back on historical evidence to the middle ages, and their analogy to similar customs observed in antiquity goes with strong internal evidence to prove that their origin must be sought in a period prior to the spread of Christianity." May we not here suggest that these customs might have been carried on in the Egyptian temples, since we now know that some of them were oriented to the rising or to the setting sun at either the summer or winter solstice; and that the "manifestation of Ra" was a thing for kings to see? In fact a writer in mediæval times, as referred to on p. 258, vol. ii., "explains the custom of rolling the wheel to mean that the sun has now reached the highest point in the ecliptic and begins thenceforward to descend"; which is exactly what the temples were built for—in order to catch the first rays of the rising or the last rays of the setting sun at these times of the year.

The author then proves that at these solemn rites the fires were regularly made of oak-wood; and shows that since the connecting link of the oak with the mistletoe is given in this very myth, and that "Balder could be killed by nothing in heaven or earth except the mistletoe," then "as soon as we see that Balder was the oak the origin of the myth becomes plain." Thus it is shown that when the god had to be killed, and when the sacred tree had to be burnt, it was necessary in the first instance to break the mistletoe off the tree.

In the two following sections he deals with the "external soul in folk tales" and the "external soul in folk custom"; the former consists of examples selected with a view of illustrating both the characteristic features and the wide diffusion of this class of tales, while the latter shows us that the idea is "not a mere figment devised to adorn a tale, but is a real article of primitive faith, which has given rise to a corresponding set of customs."

In the last section we have a short general summing up, in which the author states the conclusion which he arrives at concerning this strange and recurring tragedy of the priesthood of Nemi. The priest of the Arician grove, or, as he was called, the king of the wood, personated, as we now see, the tree on which the golden bough grew. This tree most probably was the oak, so that he was the personification of the oak-tree. As an oak spirit his life and death was in the mistletoe on the oak, so that as long as the mistletoe remained intact he could not die. In order, therefore, to slay him, it was necessary to break the golden bough, or, in other words,

to cut down the mistletoe, and probably to throw it at him.

Although this work deals with an explanation of the priesthood of Nemi, yet, on the other hand, there is plenty of substance to be got out of it which might help others who are pursuing a similar line of research in other directions. It might be interesting, for instance, to know if there is any connection between the Norse god Balder and the following legend of the Druids, referred to in Flammarion's "Astronomical Myths":—

The night of November 1 was, to the Druids, one full of mystery, in which they annually celebrated the reconstruction of the world. On this day the Druidess nuns had to pull down and rebuild the roof of their temple as a symbol of the destruction and renovation of the earth. If any of these hapless nuns happened to drop any of the materials for this new roof, they were immediately pounced upon and torn to pieces by their companions, who were seized with a fanatic transport. It was also on this day, or rather on this night, that the Druids extinguished the sacred fire, and then all other fires were put out, and a primitive night reigned throughout the land. Then the phantoms of those who had died during the preceding year passed along to the west, and were carried away by boats to the judgment seat of the god of the dead.

Another point we may mention concerns the solemn festival of the Isia, which, like the corroborees of the Australians, lasted three days, and was celebrated in honour of the dead and of Osiris, the lord of the tombs. There is a curious uncertainty about the date of this festival, the author telling us that "from the fact that, when the calendar became fixed, Athyr fell in November, no inference can be drawn as to the date at which the death of Osiris was originally celebrated." Now the Egyptians paid great attention to astronomy, and it has been stated that the day this festival commenced was at the culmination of the Pleiades at midnight.

In drawing to a conclusion our notice of this most interesting study in comparative religion, we must again direct attention to the great amount of labour the author must have undertaken in order to bring before us in a logical order the examples and myths with which these volumes abound. As a book of reference it will be found most valuable, being supplemented by a good index.

W.

GOODALE'S "PHYSIOLOGICAL BOTANY."

Physiological Botany. I. Outlines of the Histology of Phænogamous Plants; II. Vegetable Physiology. By George Lincoln Goodale, A.M., M.D., Professor of Botany in Harvard University. Gray's Botanical Text-book (Sixth Edition), Vol. II. (London: Macmillan and Co., 1890.)

THE first volume of Asa Gray's "Botanical Text-book appeared in 1842, and, in its later editions, "Structural Botany" is still a valued hand-book. Prof. Goodale's "Physiological Botany" forms the second volume, and the series is to be completed by Prof. Farlow's "Introduction to Cryptogamic Botany," and by that fourth volume on the natural orders of Phanerogams which Asa Gray "hoped rather than expected" to contribute.

Prof. Goodale's volume consists of two parts—a group of chapters (192 pages) on the histology of plants, and a section of 281 pages dealing with physiology. The present notice will be confined to the latter part of the book.

The English translation of Sachs's "Vorlesungen" and Prof. Vines's excellent lectures have done much to help the English student of botanical physiology. But in such a large and growing subject we are not likely to be overdone with text-books; we were prepared to welcome a new one, and it was in no unfriendly spirit that we opened Prof. Goodale's pages. We may say at once that our hopes have been disappointed, and that, in spite of a good deal that is worthy of praise, it is far from being a satisfactory book.

A text-book may interest us in one of two ways: it may be written with the vigour and breadth which make such excellent reading of Sachs's "Experimental Physiologie," published some twenty-five years ago; or it may, without being the work of a master, earn our thanks as a repertory of well-gathered and well-handled facts. Prof. Goodale's book seems to us to possess neither qualification in a very high degree.

We are disappointed too in another way. The date on the title-page (1890) naturally led us to look for discussions on the more important points which have arisen during the last three or four years. For instance, we expected a full account of the nitrogen question, a full account of the transpiration question, and at least some account of such interesting work as that of Wortmann, Elfving, and Noll, on geotropic curvature. But these things are not to be found, for the simple reason that the author's preface is dated 1885: we think that the public may fairly ask for some indication, on the title-page, of this condition of things.

It is no doubt a difficult thing to partition out a large subject among a limited number of pages; no two men would do it in the same way, and probably no one would be quite satisfied with the manner of distribution fixed on by another. But Prof. Goodale has exceeded the limits which may be allowed to individuality. For instance, his account of geotropism is compressed into twenty-five lines,—hardly more than is given up to De Candolle's "floral clock," and not nearly so much as is allowed to an account of the hybridization of *Lilium lancifolium* and *L. auratum*. This result—namely, the production of Parkman's lily—is no doubt of interest, but it is surely of less value to the student of physiology than a full discussion of so wide-reaching a mode of growth as geotropism.

Again, in the matter of arrangement some improvement is to be desired. For instance, in chapter xii. (on vegetable growth) we pass directly from the histology of cell-division to an account of the auxanometer. Further on we come across a brief account of turgescence, but without a hint as to its importance in relation to growth. In the paragraph on tension, the author gives no idea of the biological value of the combination of forces in giving rigidity to growing parts. The series of changes known as the grand period of growth is but slightly sketched, and no one coming to the subject for the first time would have a guess at the importance of the phenomenon.

To return to what is said on geotropism. It would surely have been more in accordance with modern views

if an attempt had been made to show that geotropism, heliotropism, hydrotropism, &c., are all parts of one phenomenon: we find, however, no hint that these modes of growth are now regarded as so many different forms of reply to stimulus. Under geotropism, Knight's experiment is not even mentioned, and the student would probably never discover that gravitation *as a stimulus* had anything to do with the matter. Prof. Goodale (p. 392) believes that a negatively geotropic organ curves upwards because the "nutritive fluids" collect "in greater amount in the cells upon its lower side." In the case of positively geotropic organs he seems to believe in the ancient doctrine of plasticity, according to which a root bends down just as a tallow candle collapses in warm weather. He connects this view with the so-called absence of tension in roots; from this we should be led to suppose that he believes all roots to be positively geotropic, but this does not seem to be the case, for he says that "it is a significant fact that in the case of certain branches from roots the direction of growth is oblique."

The treatment of heliotropism is on the same level: he believes in De Candolle's exploded theory, which depends on the fact that growth is favoured by shade, and according to which the difference in illumination on the two sides of the organ is not a stimulus, but the direct cause of curvature.

In the chapter on the movements of plants the account of the clinostat is not good. Prof. Goodale omits to mention the especial merit of this instrument, viz. that it counteracts at one and the same time the effects of one-sided illumination and of the gravitation stimulus. The illustration of the clinostat is singularly unfortunate, being in fact Sachs's drawing of secondary roots bending, under the influence of centrifugal force, in Knight's experiment.

In the discussion on circumnutation it is a pity that no allusion is made to Wiesner's careful critique on the "Power of Movement in Plants." In the same way a modern account of twining plants should give references to Baranetzky's and Wortmann's papers.

Chapter ix., on the "Transfer of Water," is an improvement; still the heart of the matter is hardly touched, and the student who relies on this discussion will be but indifferently instructed. He will not, for instance, have any clear idea that the question whether or no the transpiration-current travels as water of imbibition is or ever was a problem deserving of especial attention.

Further on we find De Vries's experiments on the withering of stems cut in air, and on their preservation from withering when cut under water, but without any reference to von Höhnel's work on negative pressure, which has such important bearings on this point, and indeed on the whole question of water-transfer. In the section on the mechanism of stomata we miss the names of Schwendener and Leitgeb; and under the heading "Relation of Age of Leaves to Transpiration," there is no clear explanation of the relations of stomatal and cuticular transpiration.

In chapter x. a very fair account is given of the assimilation of carbon. The author deserves credit for giving the passage in which, in 1817, the word *chlorophyll* was proposed; just as in another part of the book he gives the passage in which the term *protoplasm* was first employed. With regard to chlorophyll we think it a pity that any encouragement should be given to the confusion between chlorophyll and chloroplasts by such a remark as the following: "The term chlorophyll originally applied to the pigment rather than to the substance which contains it, is now used indifferently to denote the coloring-matter and the portions of protoplasmic mass which are tinged by it." This statement is all the more unnecessary because he gives on the next page a useful table of the plastid-nomenclature of Schimper, Meyer, &c. Pringsheim's hypochlorin theories are reproduced, but without the word of warning that should accompany such speculations in a book intended for students.

There is a fair account of Timiriazeff's and of Engelmann's work on assimilation; but we doubt whether it would induce the beginner to appreciate the extraordinary interest and importance of Engelmann's researches. The section ends with an outline of the "early history" of assimilation, which contains some interesting quotations from Priestley and Ingenhousz.

The section on the "Appropriation of Nitrogen" suffers grievously from the fact that the nutrition of Fungi is left out of account. The "Synthesis of Albuminous Matter" is inadequately treated, and the same must unfortunately be said of the action of ferments; and with regard to the origin of alkaloids it would have been better to have given the well-known hypothesis that they are waste products, rather than to have left their meaning in complete obscurity.

Chapter xiv. is devoted to reproduction: the author seems to have been hampered with the fear of overlapping the forthcoming book on Cryptogams, as he confines himself almost entirely to the higher plants. He gives, in a footnote, some account of the reproduction of Spirogyra, Fucus, Nemalion, Funaria, Pteris, and Selaginella, but gives no idea of the connection between this latter form and the Spermaphytes. It is clear that without a free use of the lower forms it is impossible to give such a generalized view of the reproduction of plants as is appropriate in a physiological text-book.

In discussing the colours of flowers, it would have surely been better to have given H. Müller's interesting generalizations in place of the barren statistics of Kohler and Schübeler. The chapter concludes with some useful facts on hybridization.

The last chapter in the book consists of a few pages on germination. The greater part of this discussion might with advantage have been divided among those parts of the book which deal with the general conditions of plant life and with metabolism. Of the interesting growth-phenomena of germination, such as the protrusion of the radicle, the manner in which the cotyledons are freed from the seed-coats, &c., some account should have been given, even at the risk of overlapping vol. i. of the Text-book.

In spite of a general faultiness of the kind indicated the book is not without value. It is clearly written, and contains the substance of a large number of books and papers, references to which are given at the foot of the page (instead of at the end of the chapters) to the very great convenience of the reader. Many of these references to older papers are likely to be useful, and of salutary effect on the rising generation of botanists, who are somewhat inclined to overlook the work done in the days of their grandfathers.

The author deserves credit for his appendix, in which a series of simple physiological experiments are described, with a view to their repetition by the student.

FRANCIS DARWIN.

OUR BOOK SHELF.

Plant Organization: a Review of the Structure and Morphology of Plants by the written method. By R. Halsted Ward. (Boston, U.S.A.: Ginn and Co., 1890.)

THIS is nothing more than a series of blank charts, intended for students to fill in with the details of plant descriptions. The charts are prefaced by a few pages of letterpress, wherein are contained some of the author's views on plant morphology, together with general hints and a summary of the terminology to be used. We cannot say that the author's attempt to simplify the technical terms ordinarily made use of in descriptive work is altogether a success. For instance, the words "shingled" and "straddling" for *imbricate* and *equitant*, will hardly recommend themselves to teachers on this side of the Atlantic; nor are plants either epiphytic or parasitic on *rocks*. As to the blank charts which constitute the feature of the book, it can only be said that, as such things go, they are entirely praiseworthy. But are charts of this kind really necessary? A child just beginning the subject may profitably make use of the very simple schedules devised by the late Prof. Henslow; but by the time he has advanced so far as to be able to use these complicated and detailed ones, drawn up by Mr. Ward, we think he will do much better without being kept in leading-strings. The advantage gained by writing descriptions will be vastly enhanced if he be now permitted to think a little for himself.

F. W. O.

Geometrical Conics. Part I. The Parabola. By Rev. J. J. Milne, M.A., and R. F. Davis, M.A. (London: Macmillan and Co., 1890.)

IN this work a departure is made from the general order of the propositions adopted by most geometrical writers "so as to bring the argument into closer agreement with that found in analytical text-books, in order that both methods may be studied side by side." Instead of a series of detached propositions, the authors have made a continuous treatise, and by this means have been able to deal with some of the more important points more fully than they otherwise could have done.

This part, which treats of problems and theorems relating only to the parabola, is thoroughly well done, and contains many problems fully worked out which are absent from other similar books.

Those reading the subject for the first time ought to have no difficulty in grasping the various propositions and theorems, and at the end numerous examples on them, with hints and solutions, are added.

Short Logarithms and other Tables. By W. Cawthorne Unwin, F.R.S. Fourth Edition. (London: E. and F. N. Spon and Co., 1890.)

THE short tables given in this book will be found to serve the purpose for which they were intended, which is to facilitate practical calculations and to solve arithmetical problems in a very complete way. The logarithmetic table is very short, but, if used properly, the error need not exceed one per thousand; logarithms of three-figure numbers to 999, and of four-figure numbers to 2000, are given.

Amongst the other tables are: anti-logarithms, natural and logarithmetic trigonometric functions, functions of numbers, product of numbers, table of weights and measures, and conversion tables for English and metric measures. The last-named table is inserted specially for the use of engineers, as so many treatises on engineering are now being published in France and Germany in which the measures are given according to the metric system, and in consequence of which constants for the quick conversion of these measures are required.

W.

Elementary Algebra. By Charles Smith, M.A. (London: Macmillan and Co., 1890.)

THIS is a second edition of this well-known book, and differs from the first in some important particulars. It has been thoroughly revised, and the early chapters have been simplified and remodelled. Chapters on logarithms and scales of notation form a useful and valuable addition, and there is a great increase in the number of the examples. For beginners this work should prove invaluable, and even more advanced students would do well to glance over its pages.

W.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

British Association Procedure.

I SHOULD be sorry if Prof. Lodge or anyone else should suspect me of a desire to interfere with the opportunities which are afforded by meetings of the British Association for friendly intercourse between workers, and especially between the younger scientific men and their seniors, for I feel that those opportunities constitute one of the chief advantages of attending the meetings. But with the desire to avoid waste of time in merely journal business I should prefer that each Sectional Committee should be reduced to a small executive body to whom could be entrusted the task of arranging the programme for each day, and in a preliminary way other business, such as the selection of committees to carry out suggested new researches. The appointment of such Committees and the other business would be accomplished quickly enough at a meeting of the whole Section, and then opportunity would be given to all the members for expressing an opinion or offering suggestions. The plan at present adopted is neither one thing nor the other. The Sectional Committees are too large for the despatch of business, and yet may not include every desirable member of the Section. The demand for election to which I referred comes from a certain class of people whose single purpose is served when they get their names printed on the list.

I happen to have by me the journals of the Birmingham meeting, and the number of names on the Committees of the first three Sections I find as follows:—

	Section A	B	C
Vice-Presidents ...	6	9	7
Secretaries	4	5	4
Committee	52	51	93
Totals	62	65	104

When numbers like this are reached why pretend to draw a line at all?

WILLIAM A. TILDEN.

Birmingham, September 23.

The Exploration of Central Asia.

THE late notice in NATURE of August 14 (p. 378) with reference to the work of exploration now being carried out by the Russians around Kashgar, and that M. Grombchevsky, having received permission and funds to continue his work, was starting for Rudok, is not pleasant reading for Englishmen who know that part of the Himalayas. Rudok is a small place with a fort and Gonpa or monastery, and gives the name to the tract of country lying at the eastern end of the great Pangkong Lake, on the very confines of the territcry of the Kashmir State. In 1863 I carried the survey up to that extreme eastern limit, and succeeded by avoiding observation in getting within a very few

miles of Rudok itself. The news was soon conveyed there, and the Governor, a native of Lhassa, came out to meet me. He brought presents of tea, sheep, and goats, and was most civil, but begged that I would go back, as he would lose his appointment and be disgraced if it became known that he permitted me to advance further. His politeness disarmed opposition, and my orders, given in writing, were not to get into collision with the Tibetans. We drank a good deal of tea, made in their mode churned with butter and salt, which was always simmering in his tent, and I managed to persuade him to let me ascend a conspicuous peak a few miles further on, and from which I obtained a magnificent panorama of the lake-dotted plain to the eastward of Rudok. We parted excellent friends, and I presented him with a single-barrelled pistol, in return for the presents he had given us. I feel sure that had I been able to get back there the following year, I could, starting from other points, have got very much further to the eastward, and returned again *viâ* Rudok itself. I was, however, sent to another and equally interesting part of the Himalayas.

This country of Rudok is now, in 1890, to be visited, examined, and reported upon by the Russians. Twenty-seven years have gone by since I was on its very threshold. In the interval we have had political officers appointed Residents in Leh, we have seen many journeys made by English officers and English traders to Yarkand, and yet no one has penetrated into Rudok and all that unknown country on the north and north-east which is much nearer. It does not say much for our British energy that a Russian is now to enter this area, and is now perhaps surveying within almost, I may say, a stone's-throw of our own border, which we have made no attempt to get into and know. Perhaps M. Grombchevsky may not succeed, perhaps he may lose his life, but that does not detract from the activity and devotion the Russians are at present giving to the exploration of Central Asia down to the Himalayan chain, or prevent their doing so. If they from their base can do this, why can we not ourselves? We have been content to send in natives of India, but this is not the same thing as sending European officers, for in one case the information obtained is purely topographical, no actual knowledge of physical features is gained, nothing from a military point of view, and no personal acquaintance is made with the people which might be of political or other advantage hereafter. Proceeding to the north-west of Ladak, where the Russians have lately been exploring, it appears extraordinary, with the knowledge the Government of India possessed of the vast system of glaciers of the Mustakh, south of the main range, that no attempt has been made during the past twenty-five years to finish that tract of country, and map the glaciers which descend on the north or Yarkand side, and trace the rivers flowing from them, which would be easy to accomplish, and with little or no danger of interruption. This I consider would be of far more importance and of infinitely greater interest scientifically than spending thousands of rupees on large-scale surveys of Indian hill stations and cantonments, or the resurvey of parts we know well on larger scales.

Although the Indian Survey and the Quarter-Master-General's department have made us acquainted with vast tracts of country, yet much more might have been explored if persistent efforts of every kind, along the line of the Himalayas from Kashmir to Assam had been made during the past thirty years, and if the Government of India had given encouragement to officers who were able to survey and to make the most of their opportunities to do so. I can remember when many such good opportunities have been lost, owing to a contrary policy, for fit men ready to go have not been wanting; also, when such opportunities have been taken, and at a time when the Government would not have given their sanction had it been applied for, as, for instance, when Mr. Johnstone, an uncovenanted assistant of the Kashmir Survey party proceeded to Yarkand alone, and returned in safety in 1864, bringing back a large addition to the then complete blank of intervening country, and fixing with some exactitude the position of the large cities he visited. We thus have left and are leaving to Russian subjects, who have the good wishes and countenance of their Government, to survey tracts of country lying upon our line of frontier, and we shall probably see them the first of European nations to plant their feet in Lhassa. They go to work on the right system, for much more can be done by single individuals in a quiet way, with a few carriers and attendants, than by organizing large unwieldy missions, with a little army of camp-followers and sepoys, such as it was proposed to send from Darjiling some few years ago. Such preparations become magnified into an army with aggressive aims, certain to arouse political difficulties; it is a burden on the resources of the country it has to pass through, and the possibility of misunderstandings and quarrels arising over the collection of and payment for the same.

We might have been working for years past to the northward, in many directions, by small exploring parties, and have now possessed an intimate knowledge of the physical features of the country, and its zoology, fauna, and flora, such as the Russians do in their thorough manner, but which our Government appears not to understand the value of, eminently unscientific as it is. After all, disagreeable though it be to see opportunities lost, those who do appreciate scientific methods of work must thank Russian explorers, such as Prejevalsky, and now Grombchevsky, for the light they have, in the last few years, thrown on the geography and natural history of Central Asia, from Siberia south towards British India. H. H. G-A.

Variability in the Number of Follicles in Caltha.

IT is easy to understand, supposing a tendency to variability, that characters of little value (as the colours of certain domesticated animals) might vary considerably, because not kept in check by natural selection. If it does not matter to a species whether it is unicolorous or spotted, for instance, one can see how both varieties may coexist without any tendency to the formation of a new species, and it might be rather an advantage than otherwise that individuals should differ from one another. But those parts connected with so important a function as the reproduction of the species would, one might suppose, be rigidly guarded over by the survival of the fittest, and any great variability in the number of offspring would hardly be expected within the limits of a species.

That such variability exists, however, we have abundant proof. The variability in the number of follicles in the Ranunculaceæ is astonishing. Coulter ("Manual of Botany of Rocky Mountain Region") gives the pods of *Caltha* as from 5 to 12; but this does not nearly represent the amount of variation. *Caltha leptosepala*, DC., is very abundant at West Cliff, Colorado, and this year I examined a number of specimens of the flowers, and counted the follicles, with the following result:—

Follicles.	Specimens.
2	1
3	7
4	4
5	11
6	3
7	11
8	10
9	7
10	4
11	5
12	1
13	5
14	3
15	1
Total	73

It thus appears that 73 flowers presented as many as 14 variations in the number of follicles, and curiously, the odd numbers are more numerous than the even, in the proportion of 47 to 26.

Miss Lowther and Miss Byington, of West Cliff, were good enough to search for variations other than those tabulated above, and they succeeded in finding specimens with 1, 18, 23, and 25 follicles respectively. T. D. A. COCKERELL.

3 Fairfax Road, Bedford Park, Chiswick, W., September 16.

The Origin of Mélinite and Lyddite.

(Picric Acid.)

IN your issue of the 4th inst. (p. 444) there occurs the following sentence:—

"Although picric acid compounds were long since experimented with as explosive agents, it was not until a very serious accident occurred, in 1887, at some works near Manchester, where the dye had been for some time manufactured, that public attention was directed in England to the powerfully explosive nature of this substance itself."

As this sentence forms part of this year's great annual scientific manifesto, with which Presidents of the British Association for the Advancement of Science are wont to favour your readers, I trust your love of scientific precision will help me to point out that, "*prior*" to the very serious accident near Manchester, public attention "*was*" directed in England to the powerfully explosive nature of this substance itself through the medium of a very serious publication in London, or rather through the medium of two very serious publications—viz. a patent and a paper read before the Chemical Society, as you will see from the following statement,[1] which I drew up last spring at the request of and, as I hoped, for the use of my distinguished fellow-inventor, the President of the Government Committee on Explosives, and now President of the British Association for the Advancement of Science. H. SPRENGEL.

54 Denbigh Street, S.W., September 13.

A Recently Established Bird Migration.

BURIED in the heart of a newspaper article of the 4th inst., on incorporated Worthing, is a statement which, if it may be relied on, seems to me of curious, if not unique, interest, inasmuch as it dates very closely what seems now an annual migration of birds. After speaking of West Tarring as dividing with Lancing the title of the capital of English Figland, the journalist (*Daily Telegraph*, September 4) goes on to say, "There it was that Thomas A'Becket planted the first slip—now a mouldering stump—whence, it is said, all these shady alleys, redolent of syrupy sweetness, derive their origin. There is no handsomer shrub-tree than the fig, spreading forth its many-veined, broad leaves in grateful shade, while the fruit, varying from juicy green acorns to great purple bulbs—I bought some yesterday four inches in length—peer boldly forth from every available twig. Even that discriminating bird, the Italian beccafico, has become aware, in some mysterious way, of the existence of the Worthing fig-gardens, and comes over to spend a pleasant six weeks among them, just as we go for change of air to Switzerland or the Black Forest. This is the time for his arrival, and if I may judge by certain well-picked figs on the Tarring trees, I should say that he had taken up his quarters somewhere in the immediate vicinity of the noble thirteenth century church."

We may reasonably allow a century or so from the time of Henry II., before the fig-tree would be sufficiently acclimatized and established at Worthing to attract such visitors. And then, always supposing that it is the Italian beccafico (*Motacilla curruca*, Linn.) which comes, it seems probable that he follows fig-harvest along the Riviera, and up from Southern to Northern France; though how so delicate and toothsome a mouthful manages to run the gauntlet of the continual potting which almost exterminates bird life over great breadths of that long journey is more difficult to understand. And then is it possible that a bold spirit of adventure, rather than any well-grounded certainty of knowledge, led the first comers across the Channel? Because it is a strange fact vouched for by more than one observer, and which goes dead against the old unerring instinct theory, that occasionally in the autumn migration, long streams of our emigrants make boldly out to sea from our westernmost coast where there is no land nearer than the east coast of America, and the whole flight must needs perish.

But as this whole question of bird migration is still one of the most dimly-lighted regions in the whole arcana of natural history, and its beginnings in most cases go far back into immemorial time, I trust—despite the great demands just now of the British Association reports on your valuable space—that you will kindly give some competent ornithologist, resident at, or a visitor to Worthing, the opportunity of confirming, if the fact is so, that the Italian fig-pecker has formed the habit of attending fig ripening there since the time of Thomas A'Becket.

HENRY CECIL.

Bregner, Bournemouth, September 9.

The Common Sole.

MR. CUNNINGHAM, in his valuable "Treatise on the Common Sole," recently published, remarks (p. 125), "Why I have failed to obtain soles in the first year of their growth, after the stage of those found at Mevagissey in May, I cannot understand." It may be of interest to those who are studying this subject to know that, among the investigations organized by the Royal Dublin Society, and intrusted to my care on board the s.s. *Fingal* off the west of Ireland, during the past summer, the working out of the life-history of sea fish took a prominent place.

In August, soles born in February and March were not found in shallow water, though careful search was made for them. Outside 50 fathoms we began to meet them. In 80 fathoms we took them in abundance, and also found them in the stomachs of other fish captured by the trawl in similar depths.

WILLIAM SPOTSWOOD GREEN,
H.M. Inspector of Irish Fisheries.

Dublin Castle, September 22.

A Meteor.

AT about 7.49 p.m. on the 14th inst., I saw from the garden of the Pavilion Hotel, Folkestone, an unusually large and bright meteor descend towards the north-west point of the horizon. The long and full tail left behind, like that of a large rocket, enabled one to trace its path, which at its highest point was about 6° or 8° north of Arcturus. The meteor, which was very much larger apparently than Jupiter, descended very slowly along a slightly wavy line of a mean inclination of about 75° to the horizon. The end of its path was hidden by houses on the "Bayle." J. PARNELL.

Pavilion Hotel, Folkestone, September 19.

THE WHITE RHINOCEROS.

WRITING of his sporting adventures on the River Se-whoi-whoi (a confluent of the Umniati) in Southern Mashuna-land, Mr. F. Selous, in the *Field* of August 16, says as follows:—

"It was within a mile of this spot that, two years previously [*i.e.* in 1883], I shot two white rhinoceroses (*Rhinoceros simus*), the last of their kind that have been killed (and, perhaps, that *ever will be killed*) by an Englishman. They were male and female, and I preserved the skin of the head and the skull of the former for the South African Museum in Cape Town, where they now are. I shall never cease to regret that I did not preserve the entire skeleton for our own splendid Museum of Natural History at South Kensington; but when I shot the animal I made sure I should get finer specimens later on in the season. However, one thing and another prevented my visiting the one spot of the country where I knew that a few were still to be found, and now those few have almost, if not quite all, been killed; and, to the best of my belief, the great white, or square-mouthed, rhinoceros, the largest of modern terrestrial mammals after the elephant, is on the very verge of extinction, and in the next year or two will become absolutely extinct. If in the near future some student of natural history should wish to know what this extinct beast really was like, he will find nothing in all the museums of Europe and America to enlighten him upon the subject but some half-dozen skulls and a goodly number of the anterior horns."

The skin of the head of the male white rhinoceros shot by Mr. Selous on the occasion spoken of above was forwarded by the authorities of the South African Museum to Mr. E. Gerrard, Jun., of Camden Town, to be mounted for their collection. Mr. Gerrard, knowing the rarity of specimens of this animal, was kind enough to allow the mounted head to be exhibited at a meeting of the Zoological Society of London in 1886, along with a corresponding head of the (so-called) black rhinoceros (*R. bicornis*), so that an easy comparison might be made between them.

The differences between the external forms of the heads of the two African rhinoceroses, though not, perhaps, so striking as the well-known differences in their skulls, are sufficiently obvious on comparison. I will venture to point them out in the pages of NATURE, in the hope that the attention of the several exploring parties now traversing Mashuna-land and Matabeli-land may be called to this subject, and that, in case of a straggling

[1] We have not considered it necessary to print this statement.—ED.

survivor of the white rhinoceros being met with, it may be carefully preserved for the National Collection at South Kensington.

As will be seen by the outline drawings of the heads,[1] the points by which this part of the two animals may be distinguished present themselves very appreciably. In the first

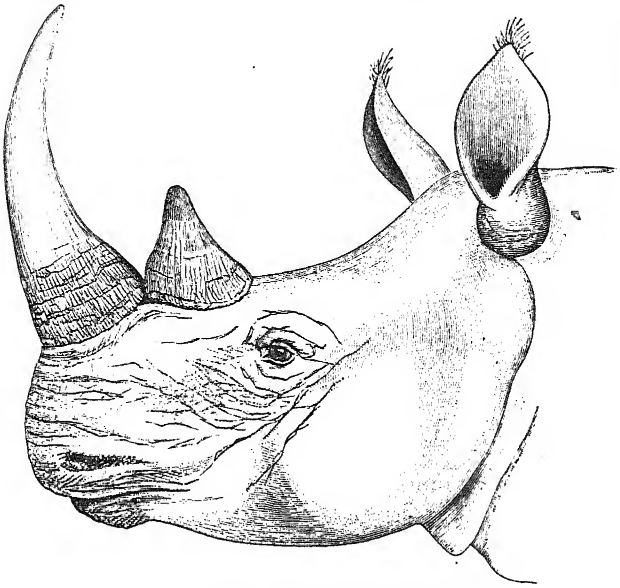


FIG. 1.—Head of *Rhinoceros simus*.

place, as is already well known, the "white" or "square-mouthed" rhinoceros (as it is much better called) is distinguished by its short upper lip. In *R. bicornis* the central portion of the upper lip is far extended, and forms a quasi-prehensile organ. This is sufficiently manifest in the drawing, but may be still better seen in the living example of the same animal now in the Zoological Society's Gardens.

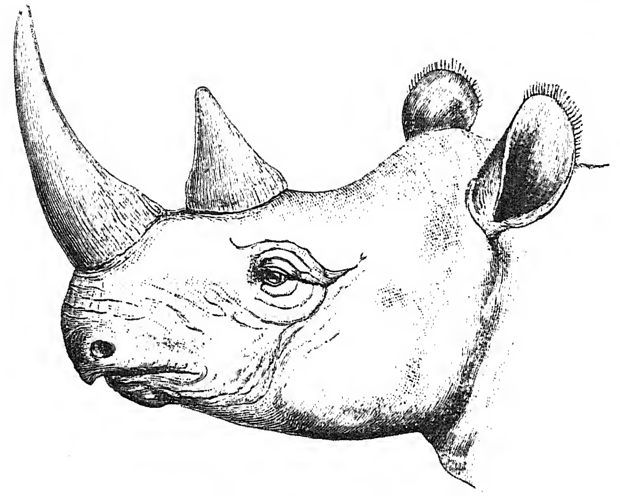


FIG. 2.—Head of *R. bicornis*.

A second point in which the heads of the two African rhinoceroses differ materially is in the size and shape of the ears. In *R. bicornis* (Fig. 2) the ear-conch is much rounded at its extremity, and edged by a fringe of short black hairs which spring from the margin. In *R. simus* (Fig. 1) the ear-conch is much more elongated and sharply pointed at its upper extremity, where the hairs which clothe its margin constitute a slight tuft. While the upper portion of the ear-conch is much more expanded in *R. simus* (than in *R. bicornis*), in the lower portion the two margins are united together for a much greater extent, and form a closed cylinder which rises about 3 inches above the base.

A third point in which the two species appear to differ is in the shape of the nostrils, which in *R. simus* are elongated in a direction parallel with the mouth, while in *R. bicornis* they are more nearly of a circular shape. Again, the eye in *R. simus* appears to be placed further back in the head than in *R. bicornis*.

In conclusion, I wish to call special attention to what Mr. Selous has already said—that no museum in Europe or America possesses a specimen of this huge animal, and to point out that the country, in which alone (as is possible but by no means certain) the last stragglers exist, being now within the British Empire, it is clearly our duty to endeavour to obtain and preserve examples of the great white or square-mouthed rhinoceros for the use and information of posterity.

P. L. SCLATER.

[1] Reduced from P.Z.S., 1886, Pl. xvi.

RECENT RESEARCH AMONG FOSSIL PLANTS.

AN instructive *résumé* of recent work among fossil plants is given by the Marquis de Saporta in the *Revue générale de Botanique*, vol. ii., 1890. It appears that mosses were almost certainly represented in the Palæozoics, a species allied to Polytrichum having been discovered at Commentry, in France. Rarely as the fructification of ferns is preserved in the Coal-measures, twenty species are now investigated, confirming the view that the Palæozoic species differed widely from the present. Half of them are most nearly related to the Marattiaceæ, whilst others show affinities with the Osmundaceæ, Gleicheniaceæ, and Hymenophyllum, the vast order of Polypodiaceæ, and the Cyatheæ being unrepresented. Among the most striking discoveries in the Coal-measures is a fern trunk several yards in length, with its fronds attached. The view that the Calamarias were in part Gymnosperms is all but universally abandoned, and the close affinity of the Lepidodendrons and Sigillarias and their cryptogamic nature everywhere admitted, so that a long controversy is ended, and the truth of Prof. Williamson's contentions definitely established. Links in the chain of evolution between Cryptogams and Gymnosperms still elude our search, and the earliest vegetation of which we have any complete knowledge already presents well-developed Gymnosperms in the shape of the deciduous Cordaïtes, a few Cycads and obscure Taxads allied to Ginkgo. At the same time, we get rid of the very puzzling Spirangium, so often regarded as a possible Palæozoic Angiosperm, but now relegated by MM. Renault and Zeiller to the animal kingdom as the egg of some member of the shark family.

Under the apparently totally dissimilar climatic conditions of the Mesozoic, the overgrown luxuriant vegetation of the coal period is replaced by forests of dry scale-leaved Coniferæ, with undergrowths of small-leaved ferns and Cycads. Fructification shows the presence of Cycadeæ in the infra-Lias, and Polypodieæ in the mid-Jurassic. The researches of Count Solms into the organization of the obscure and extinct Cycad Bennettites, bid fair to clear up another important and hitherto insoluble problem—the true botanical position of Williamsonia. Work in the past year or so has been destructive to a great deal of even recent literature on the geological history of plant evolution, the foundations of all speculative writing on this subject having as yet proved most treacherous sand.

The first appearance of Dicotyledons, once supposed

to coincide with the Tertiary period, is pushed back farther and farther into the Secondary; a flora in the United States, otherwise Jurassic in facies, containing no less than seventy-five species, or more than 20 per cent. of Phanerogams, according to Lester Ward. In England the mysterious Wealden, which from analogy should preserve rich fossil floras shedding light on the origin of Angiosperms, yields little but tubers and stems of Equisetum, scraps of ferns and conifers, and a unique liliaceous stem; while our Greensands, Gault, and Chalk afford little or nothing from which the existence of flowering plants during their deposition could be inferred. The veil which has proved absolutely impenetrable in our country, and has so long enshrouded the dawn of dicotyledonous vegetation, seems, however, about to be lifted, and we wait with the utmost interest the publication of the infra-Cretaceous floras of the Potomac by Prof. Fontaine, and of the oldest European Dicotyledons, from the beds of Gault age in Portugal, by Saporta. Though, however, the forms will be revealed, a long time must probably elapse before we can hope to rightly interpret them.

J. STARKIE GARDNER.

ON THE INFLUENCE OF HEAT ON COPPER POTASSIUM CHLORIDE AND ITS SATURATED SOLUTION.[1]

THE blue crystals of copper potassium chloride, $CuCl_2.2KCl.2H_2O$, when heated to upwards of 100°, change their colour, and a closer investigation proves such is due to the formation of a new brown salt, $CuCl_2.KCl$, according to the equation—

$$CuCl_2.2KCl.2H_2O = CuCl_2.KCl + KCl + 2H_2O.$$

This same new substance can be obtained at lower temperatures, on heating the blue double salt in presence of copper chloride; it then results according to the following symbols—

$$CuCl_2.2KCl.2H_2O + CuCl_2.2H_2O = 2CuCl_2.KCl + 4H_2O.$$

Both transformations are reversible—*i.e.* the primitive substances are obtained anew on cooling, and both take place at definite temperatures, 93° and 56° respectively, which temperatures can be accurately determined in studying the abrupt change of volume which accompanies that of chemical composition.

The temperatures of 56° and 93° are, moreover, characterized by an intersection of three curves of solubility in each case, viz.—

1. At 56° the following three will meet—

(*a*) That of the system $CuCl_2.2KCl.2H_2O$; $CuCl_2.2H_2O$.
(*b*) That of the system $CuCl_2.2KCl.2H_2O$; $CuCl_2.KCl$.
(*c*) That of the system $CuCl_2.KCl$; $CuCl_2.2H_2O$.

2. At 93°—

(*a*) That of the system $CuCl_2.2KCl.2H_2O$; ClK.
(*b*) That of the system $CuCl_2.2KCl.2H_2O$; $CuCl_2.KCl$.
(*c*) That of the system $CuCl_2KCl$; ClK.

Lastly, those same temperatures are characterized also by an intersection of four vapour pressure lines at each, viz.—

1. At 56° those of the above-mentioned three saturated solutions, and that of the dry blue salt, mixed with copper chloride, meet.

2. At 93° those of the other three mentioned above and that of the dry blue salt, mixed with potassium chloride.

J. H. VAN'T HOFF.

[1] Abstract of a paper read at the Leeds meeting of the British Association.

THOMAS CARNELLEY.

BY the death of Prof. Carnelley the science of chemistry in this country has suffered an irreparable loss. It appears that some little time ago Dr. Carnelley had been suffering from an attack of influenza, and it was whilst returning to Aberdeen after a journey to the south, made with the object of recruiting his health, that he was seized with sudden and severe illness, which was due, as his medical attendants discovered, to the formation of an internal abscess. Surgical aid proved unavailing, the patient's strength gradually gave way, and Dr. Carnelley passed away at mid-day of August 27, at the comparatively early age of thirty-eight.

Prof. Carnelley was a native of Manchester, the son of Mr. William Carnelley, Chairman of the directors of Messrs. Rylands, Limited, of that city. His early education was received at King's College School, London, and it was during this period, whilst attending the evening classes at King's College, that Carnelley began the study of that science with which he in after life identified himself. In 1868 he entered the Owens College, Manchester, gaining one of the Dalton Entrance Mathematical Exhibitions. During his career as a student, an exceptionally brilliant one, he busied himself not only with the study of the many subjects required of graduates in science of the London University, but found time to devote special attention to his favourite science, and carried out an original investigation on the vanadates of thallium, for which he received in 1872 the Dalton Chemical Scholarship. In this year also he obtained the degree of Bachelor of Science of the University of London, gaining at the final examination for this degree marks qualifying for the scholarship in chemistry, in consequence of which he held the Dalton Chemical Scholarship for an additional year. During the next two years he acted as private assistant to Prof. Roscoe, and commenced his career as a teacher by giving lectures in connection with the evening classes of the Owens College. During the year 1874-75 he continued his studies at the University of Bonn under Profs. Kekulé, Zincke, and Wallach; and on his return to England in 1875 was appointed Demonstrator and Assistant-Lecturer in Chemistry in the Owens College under Prof. Roscoe. During the time that he held this appointment he also acted as Principal of the North Staffordshire School of Science at Hanley, where his teaching proved eminently successful. In 1879 Carnelley, who had taken the London degree of D.Sc., was appointed to the newly-founded chair of chemistry in the Firth College, Sheffield, and, after three years' successful work in this institution in fitting up the chemical laboratory and inaugurating the teaching of chemistry in this College, he passed on to the then recently endowed University College of Dundee. Here ample means were placed at his disposal, and he had the satisfaction of superintending the erection of a block of buildings in which are located the chemical laboratories, lecture-rooms, &c., which he had designed and carefully planned. Under his guidance the Chemical Department of the Dundee College rapidly developed; his enthusiasm, his forgetfulness of self, his unstinted energy, and his ability and zeal as a teacher, all combined to make his department the most important one in the new College and to endear him to his students. Signally successful as was Carnelley's career in Dundee as a professor of chemistry, he also in many other ways conferred lasting benefits on the town and its inhabitants, amongst whom he spent six years, perhaps the most active of his life, and his acceptance of the appointment to the chair of chemistry at the University of Aberdeen in 1888 caused universal regret in Dundee.

Amidst his many duties, first at Owens College, then at Firth College, and afterwards at University College, Dundee, where he conducted both day and evening

classes and superintended the teaching in the laboratories, Dr. Carnelley did not forget that the first duty of a man of science is to advance his subject. That he did so with good effect is seen from the numerous communications of importance contributed to the various learned societies both in this country and in Germany, either alone or in conjunction with other investigators. Prominent amongst the researches with the results of which he has enriched science are those by which he sought to extend the application of Mendeléeff's discovery of the "periodic law," in accordance with which the chemical and physical properties of the elements and of their compounds are periodic functions of the atomic weights of the elements. Carnelley, when a student at the Owens College, appears to have been greatly impressed with Mendeléeff's conceptions, and it was to the study of the physical properties of the elements and their compounds, and to the devising of new methods of obtaining trustworthy determinations of the melting-points of metallic salts and the elements, that he early devoted his energies. The results of these experiments were subsequently utilized to show that the fusibility of the elements and of certain of their compounds is a periodic function of their atomic weights. From the relationships discovered by him to exist between the melting-points of the chlorides of the elements and the atomic weights of those elements Carnelley was led to draw conclusions respecting the atomic weight of the element beryllium and to fix its position in the classification of the elements.

Other physical properties have been shown by Carnelley to be related to the atomic weights of the elements, and in a paper read at the Aberdeen meeting of the British Association he developed a series of analogies between the elements and various series of hydrocarbons, from which he concluded that the chemical elements may be represented by a formula $A_nB_{2n+(2-x)}$, in which n is the series and x the group to which the element belongs; $A = 12$ and $B = -2$. In a paper published in the *Philosophical Magazine* in January last, he tells us that since 1872 he had attempted to give the periodic law a simple numerical expression, and states that early in the summer of 1889 he had obtained such an expression, in which the atomic weight is represented as equivalent to the product of a constant, c, into a factor made up of m, a member of an arithmetical progression, dependent on the series to which the element belongs, and v, the maximum valency, or the number of the group of which the element is a member. Thus—

$$A = c\left(m + v^{\frac{1}{x}}\right).$$

The best results are obtained when $x = 2$, and m is 0 for series II., $2\frac{1}{2}$ for III., 5 for IV., $8\frac{1}{2}$ for V., 12 for VI., $15\frac{1}{2}$ for VII., 19 for IX., $22\frac{1}{2}$ for X., 26 for XI., and $29\frac{1}{2}$ for XII.

The formula thus becomes $A = c(m + \sqrt{v})$, and m is a member of an arithmetical series in which the difference is $3\frac{1}{2}$, save in the first two series, when it is $2\frac{1}{2}$. By using this equation, the value for c in the case of 55 elements is found to lie between 6·0 and 7·2, with a mean value of 6·64. Accepting 6·6 as the value of c, the calculated atomic weight of sodium, for example, would be found as follows:—Sodium is the first member of series III., m is therefore 2·5 and $v = 1$, so that $A = 6{\cdot}6(2{\cdot}5 + \sqrt{1}) = 23{\cdot}1$. In the paper referred to the atomic weights of all the elements are given as calculated by this formula, and compared with those generally accepted. The results obtained exhibit very near approximation, the calculated values being, in fact, nearer the experimental numbers than those obtained by the aid of Dulong and Petit's law. The remarkable coincidence that the value 6·6 for the constant c in the above formula very nearly approximates to the value 6·4, accepted as the atomic heat of the elements, in acccordance with Dulong and Petit's law, is noted, and that the specific heats of the elements may consequently be represented as equivalent to $\frac{1}{m + \sqrt{v}}$. The specific heats calculated by the aid of this formula are compared with the experimental values, and in the case of the 55 elements, in which a comparison can be instituted in 45 instances the agreement is very satisfactory, while the other 10 are elements the specific heats of which, according to Dulong and Petit's law, are more or less abnormal.

Accepting Bettone's conclusion that the hardness of an element is inversely proportional to its specific volume, it is shown that hardness may be represented in terms of the specific gravity, and the expression $6{\cdot}6(m + \sqrt{v})$, thus—

$$\text{Hardness} = \frac{1}{\text{spec. vol.}} = \frac{\text{sp. gr.}}{\text{at. wt.}} = \frac{\text{sp. gr.}}{6{\cdot}6(m + \sqrt{v})}.$$

But Carnelley's energies were not alone given to the investigation of questions of a purely scientific interest, for, naturally, one situated as he was all his life in the midst of active industrial communities found many opportunities of applying his knowledge and training for the benefit of those around him. Notably was this the case in the valuable examinations, chemical and bacteriological, of the air of dwellings, schools, &c., in Dundee and district, in a report to the School Board, of which he was an active member. Much valuable information was brought to light by these investigations, and it would appear that one result attained was the realization by the authorities in Dundee, Aberdeen, and some other towns, of the necessity of making provision in schools for the supply of a pure aërial food for the scholars. This subject—the ventilation and heating of schools, &c.—was, we believe, one with which he was busily engaged at the time of his last illness, and it is to be hoped that the labour which he expended upon it will be continued by one of his competent collaborateurs.

Prof. Carnelley was also the author of an elaborate and most valuable compilation of certain physical constants of chemical compounds, published in two large volumes, a monument of industry and devotion to science; he was, moreover, an extensive contributor to the German-English dictionary of scientific and technical terms published by Messrs. Vieweg and Son, of Brunswick.

Of a retiring, modest, unselfish, and deeply religious nature, his earnest enthusiasm served not only to create in all a sincere regard for him, but to make him beloved by those who were privileged, whether as teachers or students, to become intimately acquainted with him. At all times an ardent student, an untiring investigator, a successful teacher, and a contributor in so great a variety of ways to the advancement of science, by his early death an already brilliant career has been deplorably cut short and a vacancy created in the ranks of scientific men in this country which must long remain unfilled.

H. E. R.
P. P. B.

NOTES.

THE well-known writer on vegetable palæontology, Prof. E. Weiss, of Berlin, died on July 5 last.

THE Swedish residents of Chicago have subscribed for a statue of Linnæus, which will shortly be erected in the Lincoln Park in that city.

DR. A. MÖLLER, of Berlin, has established, at Blumenau in the State of S. Catharina in South Brazil, with the assistance of the Prussian Academy of Sciences, a botanical laboratory, where, during the next two years, he will pursue Brefeld's method of the artificial culture of the higher and lower filamentous Fungi. He will be glad to receive suggestions from botanists interested in the subject

THE Congress of the United States has granted the sum of 40,000 dollars, to be employed, exclusively of salaries, in the prosecution of botanical work by the Division of Botany of the Department of Agriculture. The Section of Vegetable Pathology has now been made a distinct division, and is at present especially concerned in investigating the grape-vine disease which is spreading rapidly in California.

THERE has recently been added to the marine collection at the Brighton Aquarium a specimen of the manatee, or "sea cow." The specimen has been imported from Trinidad, and was brought from Liverpool by Mr. Wells, the marine superintendent, under whose careful supervision it was safely transferred to its new home in the Brighton Aquarium. The tank in which it has been placed has been specially fitted with heating apparatus, it being necessary to keep the temperature to between 70° and 80°. The manatee is 4 feet 6 inches in length, and feeds principally upon lettuce, of which it consumes large quantities.

THE Lords of the Committee of Council on Education have just sanctioned under Clause 8 of the Technical Instruction Act, 1889, a resolution passed on August 12, 1890, by the Council of the city of Worcester. This resolution consisted in making grants, under the powers conferred upon them by the Act, to certain institutions in Worcester for the promotion of technical instruction, and it was the opinion of the Council that such a form of instruction is required by the circumstances of the district. The instruction is to be given in the following subjects, which are not included in the branches already recognized by the Science and Art Department: French, German, type-writing, shorthand, bookkeeping, commercial geography, commercial arithmetic, and cookery.

THE *Photographic News* contains an account of the eleventh annual Convention of the Photographers' Association of America, which proved a great success. The chief part of the programme was the unveiling of the monument to Daguerre at the city of Washington. The work was after the design of Scott Hartley, and is stated to be of a beautiful unique design, and worthy of the admiration of every photographer in America. The sessions, owing to the kindness of the United States officials, were held in the National Museum, and under the very able management of the executive officers an excellent programme was provided and carried out successfully. There was an unusually large number of papers presented, and the discussions were entered into by the members in the most hearty and satisfactory manner. Among other articles are those by Dr. H. W. Vogel on photography in Germany, and by Colonel J. Waterhouse on the reversal of the negative photographic image by thio-carbamides.

THE last two numbers of *Cosmos* contain some very interesting information on various topics. Some new discoveries have been made at Pompeii, near the Stabiana Gate, and a description is given of them. Three bodies were found, two being those of men and the third that of a woman. Not far from the resting place of these bodies was found the trunk of a tree, 3 metres in height, and measuring 40 centimetres in diameter. This tree, together with its fruits that were found with it, have been examined by the Professor of Botany, M. Pasquale, who finds in it a variety of *Laurus nobilis*. By means of the fruits, since they come to maturity in the autumn, he concludes that the eruption did not take place in August but in November.

THE current number of *La Nature* contains an interesting account of the ceremony of unveiling the new statue of Gay-Lussac at Limoges. In the name of the Academy of Sciences, M. P. P. Dehérain gave a long discourse on the life and works of this great man, extracts from which are given. Prince Roland Bonaparte gives a description of the race of Somalis, some of which are at present in the Acclimatization Gardens of Paris, and which form a most curious ethnographical exhibition. The article is written from observations of the author and from other sources, and deals with the country, food, habits, dress, &c., of these people who inhabit that country, "si affreux et si désolé."

IT was observed a short time ago by Dr. Kremser, that the curve of mortality in North Germany lagged about two months behind that of the variability of temperature. An inquiry into this matter in the case of Budapesth has been lately made by Dr. Hegyfoky, taking the nine years 1873–81. Comparing the months, he failed to make out a certain connection. But taking into account other meteorological elements besides temperature, and reckoning by seasons, he found the variability of weather in the different seasons to give the following order from maximum to minimum: winter, spring, autumn, summer. As regards mortality, the order was: spring, summer, winter, autumn. Thus it appears there is a displacement of three months. If a connection of the kind referred to really exists between weather and mortality, the effect, mortality, must appear somewhat later than the cause, variation of weather.

THE *National Review* for September contains an article on the progress of weather study, by H. Harries. The subject dealt with refers chiefly to the wind, and the author traces the history of the development of the law of storms and of the practical application to weather prediction. He points to the useful work of the Meteorological Department under Admiral FitzRoy in collecting synchronous observations of the *Royal Charter* storm of October 1859. The charts then published, although too limited in their area, threw much light on the movements of the atmosphere and formed a most important step in the right direction. The later investigations of the Meteorological Council of the United States Office, and of the *Deutsche Seewarte*, &c., have contributed greatly to increase our knowledge, and to improve the accuracy of weather forecasts and it is by such synchronous discussions, and by taking advantage of the reports from rapid steam vessels, that we must hope for an extension of our knowledge in the future.

MR. E. NEVILL, the Government Chemist at Natal, in his last report to the Colonial Secretary, notes that valuable deposits of argentiferous galeno of copper and of bismuth exist in the colony, and of such rich nature that they could be profitably exported in bulk. In both Alexandra and Umvoti Counties deposits of silver-bearing lead ore have been found, containing from ten to fifteen pounds worth of silver per ton of lead ore. Saltpetre has been found so rich as to be worth more than three times as much as the best Peruvian deposits. Plumbago, asbestos, and the mineral phosphates appear to be of inferior quality. Several calcareous formations have been examined, which are likely, under proper treatment, to yield good hydraulic cement.

SOME chemical reactions can be started or accelerated by sunlight, and an increased effect is to be expected where the rays are concentrated by a lens or concave mirror. Herr Brühl has recently described (in the *Berichte*) experiments made in this way, in production of zinc ethyl from zinc and ethyl iodide (a reaction difficult to start). The retort, containing zinc-filings and several hundred grammes of ethyl iodide, was placed at the focus of a concave mirror, about a foot in diameter, receiving the sun's rays. The reaction soon began, and grew so vigorous that cooling was necessary. In a quarter of an hour all the ethyl iodide was consumed, and through the subsequent distillation in an oil-bath, a good yield of zinc ethyl was had. This radiation process, it is suggested, might be variously useful in actions on halogen-compounds, which tend to be disaggregated by sunlight. A lens, owing to the athermanous property of glass, would be less powerful.

MR. A. MCADIE, Fellow of Clark University, U.S., has forwarded us his prize essay on tornadoes, reprinted from the *American Meteorological Journal* for August. After a discussion of the state of our knowledge of storms of this character, the possibility and practicability of predicting them is considered. It is suggested that, since the barometric movement is too sluggish and the thermometric indications too much masked to be serviceable, the electrometer might be better adapted to give notice and warning of the proximity of violent whirlings in the air and detect those which would otherwise pass unheeded. A careful study of cloud movement is also suggested, as a method promising much in the way of obtaining knowledge bearing on the occurrence of tornadoes.

THE Report of Dr. Eitel, Inspector of Schools in Hong Kong, for the past year, contains some interesting details. The total number of educational institutions of all descriptions, known to have been at work in the colony of Hong Kong during the year 1889, amounts to 211 schools, with a grand total of 9681 scholars under instruction. More than three-fourths of the whole number of scholars, viz. 7659, attended schools (106 in number) subject to Government supervision, and either established or aided by the Government. The remainder, with 2022 scholars, are private institutions, entirely independent of Government supervision and receiving no aid from public funds. The total number of schools subject to direct supervision and annual examination by the Inspector of Schools amounted, in 1889, to 104, as compared with 50 in 1879, and 19 in 1869. The total number of scholars enrolled in this same class of schools during 1889 amounted to 7107, as compared with 3460 in 1879, and 942 in 1869. In other words, there has been an increase of 31 schools and 2518 scholars during the ten years from 1869 to 1879, and an increase of 54 schools and 3647 scholars during the ten years from 1879 to 1889. It would seem, therefore, that the decennial increase of schools and scholars during the last twenty years has shown a tendency to keep up with the progressive increase of population. Comparing the statistics of individual years, the number of schools under supervision and examination by the Inspector of Schools rose from 94 in 1887 and 97 in 1888 to 104 in 1889, whilst the number of scholars under instruction in these same schools rose from 5974 in 1887 and 6258 in 1888 to 7107 in 1889. There is, therefore, a steady annual increase during the last three years, progressing from an increase of 284 scholars in 1888 to an increase of 849 in 1889. The expenses incurred by the Government during the year 1889, on account of education in general, amounted, exclusive of the cost of new school buildings, to a total of $53,901.

IN *Science* reference is made to a question which may interest many of our readers, "Should beer be drunk out of a glass?" Dr. Schultze claims to have established, by a very extended series of experiments, that beer, by as little as five minutes' standing in any glass, even when cold and in the dark, will be materially affected both in taste and odour. By making trial tests on some one hundred persons he sustains his claims. The change is due, as he thinks, to the slight solubility of the glass substance in the beer. Lead is used in the manufacture of glass, making it more easy to manipulate, and from experiments with glass obtained from the leading sources of supply, he determined that one cubic centimetre of beer, by five minutes' standing in glass, dissolved 6–26 ten-millionths of a milligram of the glass substance containg 0–48 thousand-millionths of a milligram of lead oxide. It is this small quantity of glass substance that affects the taste of the beer, and if it contains lead, renders it objectionable for sanitary reasons. By further experiments with vessels of different substances, he comes to the conclusion that gold-lined silver mugs are the best, and he ranks covered salt-glazed stone mugs as good.

THE United States Consul at Hanover, in a recent report refers to afforestation in that State, where, he says, there were formerly rich tracts of forests. These, in consequence of wars, were reduced to desolate wastes, and remained so until the first decades of the present century—particularly those districts between Hamburg and Hanover which are known by the name of Luneburger Haide. Another reason for the devastation was mismanagement, such as division of common forests, by which they were dispersed and fell into the hands of people with small means, and thus were doomed to neglect and destruction. The celebrated Burkhardt was appointed Director of the Forest Department in 1850, and under him part of the Luneburger Haide, as well as other tracts growing more and more desert by the encroachments of sand, have been wooded with great pains and trouble. To prevent the increase of sandy deserts those tracts were at first planted with fir-trees. After a number of years these were cleared, and beech and other trees substituted. How much the forests have been enlarged in this manner will appear by the fact that the wooded surface amounted in the year 1850 to 1,217,625 acres, and in 1885 to 1,551,900 acres. The Government granted large sums for the purchase of land unfit for cultivation to be turned into forest tracts, and is now intent on uniting again those formerly scattered wooded parts into one single tract. In the same way the Klosterkammer (Administration of cloister funds) purchase extensive stretches of soil. Municipalities, communities, and even private individuals who are inclined to establish forests and manage them rationally, will receive loans at 2 per cent. from the Provincial Government, to be reimbursed yearly by small instalments; single subsidies also are granted for converting large wastes into forest grounds. The Government employed vagabonds, tramps, and prisoners not of a dangerous character, for forest culture. In this manner about 9000 acres were planted with trees by those troublesome classes within the years 1876 to 1888. Moreover, communities as well as private individuals have turned about 14,000 acres into forest grounds within the same period by means of subsidies afforded by the Provincial Government, and various towns have laboured to preserve and plant forests in their vicinity for purposes of health, recreation, and incidentally of profit also.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. A. S. Keys; a Brazilian Tree-Porcupine (*Sphingurus prehensilis*) from Trinidad, presented by Mr. J. N. Kilner; two Vulpine Phalangers (*Phalangista vulpina* ♂ ♀) from Australia, presented by Mr. J. G. Mackie; two Pucheran's Guinea Fowls (*Numida pucherani*) from East Africa, presented by Mr. Keith Anstruther; a Silver Pheasant (*Euplocamus nycthemerus* ♀) from China, presented by Mr. E. W. H. Blagg; two Wheatears (*Saxicola œnanthe*), a Stonechat (*Pratincola rubicola*), a Whitethroat (*Sylvia cinerea*), British, presented by Mr. J. Young, F.Z.S.; an Owen's Apteryx (*Apteryx oweni*) from New Zealand, presented by Capt. E. A. Findlay, R.N.R., R.M.S. *Ruapehu*; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented by Mr. Luxmore Marshall; a Blue-eyed Cockatoo (*Cacatua ophthalmica*) from New Britain, presented by Mrs. R. E. Anson; a Guillemot (*Lomvia troile*), British, presented by Mrs. Forbes; two Common Gulls (*Larus canus*), a Black-headed Gull (*Larus ridibundus*), British, presented by Mr. A. C. Howard; a Lion (*Felis leo* ♂) from Sokoto, West Africa, deposited; a Common Bee-eater (*Merops apiaster*), European, a Green-headed Tanager (*Calliste tricolor*) from Brazil, purchased; three Garden Dormice (*Myoxus quercinus*) from Vosges, France, received in exchange; two Viscachas (*Lagostomus trichodactylus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on September 25 = 22h. 18m. 40s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4815	—	—	22 32 1	+33 49
(2) 615 Birm.	6	Yellowish-red.	22 34 18	+56 13
(3) B.A.C. 7954	4	Yellowish-red.	22 43 46	−14 4
(4) ζ Aquarii...	3	White.	22 23 12	− 0 35
(5) η Aquarii	4	White.	22 29 42	− 0 41
(6) 251 Schj.	8	Very red.	21 38 43	+37 31
(7) T Herculis	Var.	Reddish.	18 4 56	+31 0

Remarks.

(1) According to the observations of Huggin and D'Arrest, this nebula has a continuous spectrum, but further observations for "irregularities" or bright flutings should be made. The nebula is described as "Bright; pretty large; pretty much elongated in the direction 160°; suddenly much brighter in the middle."

(2) Dunér compares the spectrum of this star of Group II. with that of α Herculis, and states that "it is one of the finest in the northern sky." The bands 2–9, including 6, are very wide and dark, and the spectrum is one which may be advantageously studied. Light-curves of spectra of this type are valuable, as they show the relative extent of carbon radiation, and therefore serve as a cross check in the classification which is made on other grounds.

(3) The spectrum of this star is one of Group II., in which bands 2, 3, and 7 are dark but not very wide, and bands 4, 5, and 8 are feeble and narrow. Dunér thinks it almost intermediate between Group II. and Group III., but in this he is probably mistaken, as the description agrees almost exactly with that of 75 Cygni (see p. 511), which turns out to belong to an early and not a late species of the group. In that case the bright carbon flutings are predominant, and it will probably be found that this applies also to the star in question. Here, again, a light-curve of the spectrum compared with that of a star like (2) should emphasize this point.

(4) This star has a spectrum which is almost Group IV., the hydrogen lines being considerably broad, but, at the same time, *b* and D are seen without much difficulty. Its proper place on the temperature curve is therefore the last stage of Group III. It may be remarked that with the same thickness of F in a star of Group V., the metallic lines would not be so prominent. One need only compare Aldebaran and Capella to see this difference in the intensities of the metallic lines in Groups III. and V.

(5) A star of Group IV.

(6) In the spectrum of this star of Group VI. no secondary bands were seen with certainty by Dunér, and although the green and yellow zones are very bright, the blue light is very feeble. It seems as if in some of these stars there is more continuous absorption than in others, and comparative light-curves of the spectra of stars of the group might throw light upon this point. This again would probably enable us to determine the relative temperatures of the different stars. The intensity of the blue zone certainly does not depend altogether upon magnitude.

(7) The approaching maximum of this variable (October 4), will offer another opportunity of determining the character of its spectrum. It is much to be regretted that so many variables have as yet unknown spectra, and the sooner they are observed the better. T Herculis has a period of about 165 days, and ranges from 6·9–8·3 at maximum to 11·4–12·7 at minimum (Gore).

A. FOWLER.

SOLAR ACTIVITY FROM JANUARY TO JUNE 1890.—Prof. Tacchini has just presented to the Paris Academy of Sciences a note on the distribution in latitude of solar phenomena observed by him during the first half of this year (*Comptes rendus*, September 15). Hydrogen prominences have been more frequent in the southern hemisphere than in the northern, and reached a maximum of frequency in the zone included between the latitudes 40°–50°. This was also the case in 1889 (*Comptes rendus*, May 5, 1890). During the second quarter of this year prominences have been observed very near to the poles, which indicates that solar activity is on the increase. Faculæ show maxima of occurrence at the same distance from the equator in both hemispheres, viz. 20°–30°. The maximum frequency in the northern hemisphere is more marked, however, than in the southern. The distribution of groups of spots is the same as that of faculæ, hence Prof. Tacchini concludes that we have reached a period of change in the distribution in latitude of solar phenomena; for whilst prominences have maintained a predominance in the southern hemisphere, faculæ and spots have been more frequent in the northern. The absolute number of groups of spots during the second quarter of this year was greater than that of the first quarter, thus indicating that the minimum period has definitely passed.

THE TELLURIC SPECTRUM.—In the current number of *L'Astronomie* M. Janssen gives a short account of his work in Algeria on the telluric spectrum. The object of the expedition was to photograph the solar spectrum on isochromatic plates when the sun was respectively on the meridian and horizon. By the use of such plates, having maxima of sensibility at the less refrangible end of the spectrum, the increase in intensity of the most important telluric lines, which accompanied a decrease in the sun's altitude, may be strikingly demonstrated. The observations were made from a small fort near Biskra, situated on the edge of the Sahara, and having an uninterrupted view towards the south. The solar spectrum was obtained by means of a Rowland's grating, and many photographs were taken during the three months of observation. Their discussion is not yet completed, but M. Janssen notes that without the purity of the sky at the place of observation and the continuance of fine weather it would have been impossible to obtain any useful results. An excursion was made to Tuggurt in order to study the solar spectrum from one of the driest places on our globe. Some photographic observations of mirages were also made at the same time, and are said to throw much light on the nature of the conditions necessary for the production of these singular phenomena.

THE PERSEID METEORS.—In *Comptes rendus* for September 15, Prof. Denza gives an account of the observations made in Italy from August 9 to 11 under the direction of the Italian Association for the Observation of Meteors. From the results obtained at the thirty stations it is concluded:—

(1) The number of luminous meteors, especially on August 11 and 12, was greater than in preceding years, and has relatively attained a maximum. This appears to prove that the earth has cut through a condensation in the meteoritic ring.

(2) The meteoritic shower, which formerly began on August 10, appears to have suffered a retardation, and now begins on August 11.

(3) The following are the numbers of meteors observed at some of the stations: Vatican Observatory, 1971; Florence, 1749; Aprica, 1740; Gaeta, 1305; San Martino in Pensili, 1276; Moncalieri, 1036.

(4) The radiant of the principal shower was found to have the same position between Perseus and Cassiopeia as has previously been noted.

(5) Other radiants were also observed, and notably in Ursa Major and Ursa Minor, Cygnus and Andromeda.

(6) Most of the meteors seen had the yellow colour characteristic of this swarm.

(7) The shower was a remarkable one this year, not only because of the great number of meteors, but also because of their large size.

NATAL OBSERVATORY.—From the annual report of the Government Astronomer of this Observatory for 1889 it appears that the principal work in progress is a comparison of the declinations deduced from observations made at Observatories in the northern and southern hemispheres by a comparison by Talco's method of the zenith distances of northern stars and circumpolars both above and below the pole. Some important results have also been obtained from an investigation into the present theory of lunar motion. The meteorological observations made during 1889 have been tabulated, and will be found useful.

THE NARRABURRA METEOR.

THE Narraburra meteor was found in the year 1854, by Mr. O'Brien, in lat. 34° 10′ S., long. 147° 43′ E., which is a point on the Narraburra Creek about 12 miles east of Temora. When

found, it was on a hard and stony surface, but I have been unable to obtain any other particulars, as the finder has long since passed away to the majority.

Mr. O'Brien gave the meteor to Mr. Patrick Harrold, of Mount Hope, near Cootamundra, and it has been in his keeping ever since; until, on March 30, 1890, he was induced by Mr. William R. Eury, Inspector under the State Children's Relief Branch, to send it to me. Mr. Eury, as soon as he saw the meteor, pointed out to Mr. Harrold the great scientific interest attaching to it, and that undoubtedly the proper place for it was in the Observatory, where a collection of these so-called shooting stars is being made, and upon this, Mr. Harrold sent it to me. I am very much indebted to both of these gentlemen for enriching the Observatory collection by this most interesting specimen of a metallic meteor. Our museum for meteors now contains six.

In appearance this meteor is like rusty iron, and it has a very irregular outline, which seems to have resulted from the oxidation or solution of rounded masses, which had solidified with the iron, and upon removal formed cavities. In size it measures 11 inches × 7½ inches. Two of these are so placed that they look like the orbits in an ox's skull, a suggestion borne out by

the general outline, which is not unlike the bone in question. In one place a hole nearly 1 inch in diameter and 1½ inch deep, has been made straight into the solid iron, and there seems to be little doubt that, when the iron originally cooled down from its gaseous state, it did so in the presence of these rounded and symmetrical masses, which impressed their form on the plastic iron as it solidified. These, as I have already suggested, have no doubt been removed since they reached the earth's atmosphere.

A meteor which fell in New England in November last was seen to have a spiral motion, emitting steam or smoke in jets. Looking at the holes in this meteor, one can see at once that if, when it reached the atmosphere, they were charged with some substance that would burn freely in the oxygen of the air, this solid mass of iron would have twisted about under the influence of the many gas-jets from the burning masses in its sides.

I find its specific gravity is 7·57 and its weight is 71 pounds (70 pounds 14 ounces). Meteoric iron is, I think, never quite pure, and masses of it vary very considerably in specific gravity. Taking five at random which fell in different parts of the earth, it varies in them from 7·38 to 7·82, and the mean happens to be 7·62, almost exactly the same as the one before us.

This meteor has not been analyzed yet.

July 26 H. C. RUSSELL.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE thirty-ninth annual meeting of this body was held on August 20-26 at Indianapolis, which is the capital and largest city of the State of Indiana, and is the largest inland city of the United States, being a railroad centre without navigable water of any kind, and having, with its suburbs, a population of about 140,000.

Near the city is located the greatest region of natural gas in the world. The manufacturing business of this whole region has received a wonderful stimulus from the discovery of natural gas, which has caused a rapid increase in population and manufacturing within the past three years. The gas is found in the Trenton limestone at a depth of nearly a thousand feet over a large area north and east of the city, and, besides being used *in situ*, is brought to the city in pipes for use there, where it has displaced other fuel in the large factories. One of the most instructive object-lessons the Association has ever had was the excursion on Saturday through this gas belt, stopping at Noblesville (whence the supply for Indianapolis is drawn), Kokomo, Marion, Muncie, and Anderson. At the latter place a remarkable exhibition was made of gas forced through the river and ignited upon the surface. President Goodale warned the citizens in an address of the necessity of economizing this resource, since it is not inexhaustible.

The sessions of the Association were held in the new Capitol, a fine building completed only two or three years ago at the cost of about 2,000,000 dollars, and decidedly the most sumptuous quarters ever offered to the Association, being also spacious enough to accommodate all the eight Sections under one roof.

A number of affiliated associations meet at or about the time of the principal one. Of these the Society for the Promotion of Agricultural Science, and the Society of American Geologists preceded the main meeting, while the Botanical, Entomological, and, this year for the first time, the Ornithological Club, met at intervals between meetings of the parent Association.

On Wednesday morning the retiring president, Prof. T. C. Mendenhall, called the Association to order, and resigned the chair to Prof. George L. Goodale, President-Elect. The morning was devoted to addresses of welcome and responses, and to the organization of the several sections. Among the speakers were Mayor Sullivan and Lieutenant-Governor Chase. An invitation was received from the Australasian Association for the Advancement of Science to attend the meeting at Christ Church, New Zealand, in January 1891, and President Goodale was deputed, and will attend as a delegate.

In the afternoon the eight Vice-Presidents read their several annual addresses before their respective Sections. These addresses were generally ably written.

Section A (Mathematics and Astronomy) was addressed by Prof. S. C. Chandler, of Harvard, on the variable stars. The number of these discernible with an ordinary field-glass is two thousand, while our largest telescopes reveal, perhaps, hundreds of thousands. The cycle of change, commonly called the period, ranges from less than eight hours in the wonderful variable recently found by Paul, up to two years. Between these limits is a highly significant deviation from uniformity of distribution. At least five-sixths of the variables are reddish, and the redness of the variable stars is, in general, a function of the length of their period of light variation. The redder the tint, the longer the period. An examination of fluctuations in brightness, or light curves, enables us to distinguish a number of types, of which the most remarkable is that of Algol. The cause of variability is still problematic, except for the ten stars of the Algol type, which seem to be explained by the theory of an occulting satellite, somewhat modified however. For the other types we may perhaps seek an explanation in certain consequences of rotation of the stars upon their axes, or by introducing modifying suppositions of unequally illuminated surfaces, irregular forms, tidal action upon light-absorbing atmospheres, spontaneous and intermittent explosions, meteor swarms, and the like.

Section B (Physics) was addressed by Prof. Cleveland, Abbé of Washington, colloquially designated throughout the United States "Old Probs," for the reason that he is in charge of the weather bureau, and makes up the daily weather report, with indications, formerly called probabilities. His theme was terrestrial physics. There are two kinds of physics—molecular and terrestrial. The latter he names, following the German nomenclature, geo-physics. It relates to the earth as a whole, including all phenomena relating to earthquakes, volcanoes, gravitation, and the variations in its intensity on land and sea, mountain, plain, and valley, magnetism of the earth, tidal motion and tidal stress of the earth's crust as well as of the ocean, and in general the study of the entire interior of the earth, of the earth's crust, both land and water, meteorology, auroras, &c. He deplores the lack of laboratories for such researches, and deems a good geo-physical laboratory a great desideratum. He urges that some such institution should be founded and endowed, rather than to continue the founding of laboratories for research in chemistry or molecular physics of which so many are already in existence, or the establishment and endowment of universities to teach only what is already known.

The address of Prof. R. B. Warder, of Washington, to Section C (Chemistry), on geometrical isomerism, was decidedly the most abstruse of the series, but to one able to follow him, it was of unusual interest, giving the latest results of study into the subject of the relative positions of atoms in a molecule, including a careful study of the right-handed and left-handed carbon molecules. Most of this material is very recent, the prominent workers, such as Wislicenus and Wunderlich, having made more progress within two or three years than in any previous period. Besides the speculative interest of these studies they have a very important practical application in the physiological and pathological action of isomers, many of which, while identical in chemical constitution, affect living organisms very differently, whether administered as food or as medicine.

Prof. James E. Denton, of Hoboken, addressed Section D (Mechanical Science and Engineering) on mechanical tests of lubricants. Experiments to determine the co-efficient of friction between lubricated rubbing surfaces have been prosecuted for two hundred years, and have resulted in the existence of many forms of satisfactory apparatus for such measurement, which are now known as oil-testing machines. The overheating of bearings is due, however, to accidental abrasion of rubbing surfaces, which generates an intense heat at some points, and tends to vaporize some oils more than others. Oil-testing machines are inadequate to reveal these differences, and moreover the supply of oil is artificially abundant, instead of feeding through practical forms of oil-cups. It is concluded, therefore, that each oil must be tested with a series of conditions of the rubbing surfaces, and practical feeding devices which involve opportunities for abrasion and overheating. Explanation was given of the paradoxical fact that overheating is often remedied by supplying sand or emery to bearings. The sand grains make grooves around the wearing parts, and as a result the oil is uniformly distributed, and the hot-box cools down to the limit of safety.

Prof. John C. Branner, State Geologist of Arkansas, addressed Section E (Geology and Geography) on relations of the state and the national geological surveys to each other. He thus recapitulates the benefits to be derived from voluntary cordial co-operation between all geologists and all geological organizations in this country.

"(1) Geologic research being under the nominal direction of the leading investigators would be so conducted as to be of the greatest utility to the greatest number.

"(2) When a piece of work was done by one it would be done for all, and duplication by state surveys and by individuals, and the consequent waste of energy, time, and money would cease.

"(3) The functions and fields of official organizations being better defined, state and national surveys and individuals could so direct their efforts as to serve the purposes of others without neglecting their own immediate aims, and without infringing upon each other's ground.

"(4) National and state surveys would be strengthened, and local organizations and individual effort encouraged.

"(5) It would give us a better geologic literature, better instruction, better geologists, and more thorough specialists.

"(6) And finally, we trust it would put a stop to those oracles of science who are so ready to prophesy in its name."

Dr. Charles S. Minot, of Boston, addressed Section F (Biology) on certain phenomena of growing old. The loss of vital power commences from birth; the older an organism is the more time it takes to produce a given change, and this indicates a progressive loss of vitality. Anatomical peculiarities can be found correlated with this progressive loss of vitality. Considering in detail the various tissues of the body in the order of

their development, in each of the principal tissues and organs of inner, middle and outer layer of the body, the cells composing them show the same peculiarity, namely, that in their young condition they contain only a small amount of protoplasm, and in the adult condition a very much larger amount, so that the proportion of protoplasm to nucleus increases with the age of the organism. The conclusion is that development of protoplasm is associated with loss of vitality. So that instead of speaking of protoplasm as the physical basis of life, we might term it the physical basis of advancing decrepitude, or in other words, the physical basis of death. The reverse development is seen in generation, where the first process which the fecundated ovum undergoes is segmentation into numerous nuclei, with attendant decrease in the proportion of protoplasm to nucleus, and precisely the same phenomenon is noted in animals which multiply by fissure, the tissues at the point of fissure becoming greatly segmented.

Dr. Frank Baker, of Washington, addressed Section H (Anthropology) on the ascent of man, in which he traced with much detail the modifications which the body has undergone in ages of development, the more striking modifications being those connected with the limbs, the change from quadrupedal to erect posture and the segmentation of the body, and indications of change being left as vestigial organs. The erect position is gradually acquired, and the difficulty that an infant experiences in learning to walk erect is strong evidence that it is an accomplishment acquired by the race late in its history. The human body gives evidence of a previous semi-erect position. The special changes of structure which secure the erect position are less marked in children and in the lower races. In the course of evolution of these changes, there is a period of struggle before the body becomes thoroughly adapted to them. Such struggle is still going on, the adaptation being far from complete. Hence the liability of man to certain deformities and diseases, to which quadrupeds are not so much disposed. It is in just this line that is to be found the explanation of the greater difficulty and dangers of parturition in the human family, and of the fact that woman in her entire organism has suffered more than man in the upward struggle. The increased influence of gravity also explains the greater tendency to certain disturbances of the circulatory organs. Study of the bony skeleton gives, in man, evidence of his relationship, in origin to the lower animals, as in the persistence of relics of ribs, and in unmistakable signs that the skull is composed of segmented pieces like the vertebræ. The evidence of such relationship has come, and is coming from all sides, from the study of comparative brain weight and structure, of the facial angle, the face bones and teeth, with their resulting changes in expression from brute or brutal man to the highest types, in which the brain shows its rulership in the countenance

Prof. J. R. Dodge, of Washington, addressed Section I (Economic Science and Statistics) on the standard of living in America. Prof. Dodge is chief of the agricultural bureau of statistics of the United States, and his report of the condition of growing crops on the 10th of every month is always eagerly awaited, and has a great effect on market prices of agricultural produce of all kinds. The American standard of living is the highest known. To maintain it, wages are and should remain high. Production is not thereby diminished because of the brain power of the American people and our utilization of labour-saving machinery, so that in many articles exportation increases enormously despite high wages. Our woods are tougher than those of Europe, and we would not accept European tools if given to us.

His most important conclusions are: The question arises, Shall the present standard of living be maintained? It is a point upon which hangs "the future education, enterprise, independence, and prosperity of the people" of the United States. It depends on the industry of the producing classes, and wisdom in the distribution of their labour for a production that shall meet their wants. If idleness shall be encouraged, production limited, importation enlarged, and dependence on foreign countries fostered, wages will be reduced, and the ability to purchase as well as the volume of production will decline. If the advice of public and private teachers of repressive economy to buy everything abroad, and sit down in the enjoyment of the luxury of idleness at home, shall become the law of the land, short rations will follow, and high prices will only be abated by the inability of our people to purchase for consumption.

Unless the largest variety of production shall be encouraged, and the highest skill shall be stimulated in the endeavour to meet all the wants of our people by the results of our own labour, it will be impossible for us to have a surplus for export. It is a matter of time, of determined effort, of high endeavour to render high wages consistent with large exportations of surplus, but the future will accomplish it, if the present scale of living and rate of wages of the American people shall be maintained.

Wednesday evening was taken up with the address of the retiring president, Prof. T. C. Mendenhall, chief of the United States Geodetic and Coast Survey, who spoke on the relation between men of science and the community. He began by calling attention to the fact that this association is the outgrowth of the Association of American Geologists and Naturalists organized just fifty years ago. He spoke of the duty assigned the retiring president to present an address as giving an opportunity to dismiss the relationship between members of the Association and the general public whose interest is often born of curiosity rather than intelligent appreciation. The meetings of this Association have been the means of disseminating proper methods of investigation and study throughout the land. He considered various elements of weakness in scientific men such as assumption of superior knowledge in lines of investigation outside of their own specialties, lack of a proper amount of utilitarianism, as well as lack of interest in political affairs, contrasting this spirit with the distinguished service rendered to mankind by such scientific men as Newton, Watt, and Franklin. The ideal of duty which ought to be present in the mind of every man of science may well be higher than that growing out of mere selfish pleasure in the acquisition and possession of knowledge.

The remaining days of the session—Thursday, Friday, Monday and Tuesday—were devoted to general business and the reading of papers in the sections. On Friday evening Dr. H. C. Hovey lectured on Mammoth, Marengo, and Wyandotte caves, and on Monday evening Prof. C. Leo Mees lectured on electricity.

The general business included an appropriation of 250 dollars to Prof. E. W. Morley for the further prosecution of his researches in the velocity of light in a magnetic field; resolution of thanks to two Brazilian gentlemen for removing to the museum at Rio the largest meteor ever found, weighing five tons; resolution requesting Congress to provide fire-proof quarters for the botanical collection at Washington, and another urging protection of the forests; resolution favouring the use of the metric system at Custom houses in the United States.

It was decided to hold the next annual meeting at Washington, and invitations were sent to other governments on the American continent to send delegates, thus giving to this meeting, which is the only one held at Washington in recent times, an international character.

The Association adopted the report of the committee of anatomical nomenclature, which recommends the following changes, with special reference to the brain: "That the adjectives dorsal and ventral be employed in place of posterior and anterior, as commonly used in human anatomy; and, in place of upper and lower as sometimes used in comparative anatomy; that the cornua of the spinal cord and spinal nerve roots be designated dorsal and ventral rather than posterior and anterior; that the costiferous vertebræ be called thoracic rather than dorsal; that the hippocampus minor be called calcar; that the hippocampus major be called hippocampus; the pons variolii, pons; the insula Reilii, insula; pia mater, pia; dura mater, dura."

Two hundred and fifty-nine papers were read, of which the largest number, fifty-one, were in the section of physics, and the next largest, forty-eight, in biology. It is difficult to attempt a selection without doing injustice, but a few of the papers deserve mention, while perhaps others, equally meritorious, may be overlooked. Prof. Cleveland Abbé read papers by himself on kinematic methods of determining the altitudes and motion of the clouds, and, by Frank N. Bigelow, on further study of the solar corona, and on terrestrial magnetism. The corona is deemed to consist of matter streaming out from the sun in zones about 32° distant from the poles, and falling back into the region of sun-spots, which are, probably, thus caused. It is regarded as similar to the earth's aurora, though of denser matter.

Prof. T. C. Mendenhall, in his paper on the use of the magnetograph as a seismoscope, showed that earthquakes are caused by the tidal stress of sun and moon upon the earth's

crust, and are accompanied by magnetic currents which serve as indices of their approach.

Prof. E. W. Morley's report on the velocity of light in a magnetic field shows an increase in velocity in such a field amounting to seven parts in one thousand million. These investigations are to be continued.

Prof. Morley also read a paper on the determination of the volumetric composition of water, and one on the ratio of the density of oxygen and hydrogen. In twenty determinations the minimum value of combination in water was 2·0005, the maximum was 2·00047, mean 2·00023, with a probable error of one part in 30,000. The value two to one, which every schoolboy learned is the ratio of hydrogen and oxygen in water, must be increased about one nine-thousandth. In two determinations of density, Morley reaches the same result as Rayleigh, viz. 15,884, giving 15,882 as the atomic weight of oxygen. Prof. W. A. Noyes read a paper on the atomic weight of oxygen, giving the results of four series of six determinations with apparatus devised by himself. The value found is 15,896, or about seven one-hundredths less than the usually accepted one.

The series of papers on distribution of North American plants, prepared on topics assigned last year, was pronounced by the presiding officer the most remarkable ever presented to the biological section. They were on the distribution of the North American umbelliferæ, by John M. Coulter; the distribution of hepaticæ of North America, by Lucien M. Underwood; geographical distribution of North American grasses, by W. J. Beal; geographical distribution of North American cornaceæ, by John M. Coulter; and the general distribution of North American plants, by N. L. Bulton. The following assignments were made for next year:—The absorption of gases, J. C. Arthur; the aëration of aquatic plants, W. P. Wilson; the absorption of fluids, L. H. Pammel; the movement of fluids in plants, W. J. Beal; transpiration, C. E. Bessey.

The exhibition of apparatus included some delicate seismoscopes and seismometers. Prof. Mendenhall exhibited some of the metric standards recently distributed by the International Congress, in the manufacture of which to distribute to all nations, two-thirds of all the iridium in the world was used. Prof. W. A. Rogers exhibited a precision screw 8 feet long, with a variation of only 1/8000 of an inch in its entire length.

Officers elected for the Washington meeting were: President, Albert B. Prescott, of Ann Arbor, Mich.; Vice-presidents, Section A, E. W. Hyde, of Cincinnati, O.; Section B, F. E. Nipher, St. Louis, Mo.; Section C, R. C. Kedzie, Agricultural College, Mich.; Section D, Thomas Gray, Oene Haute, Ind.; Section E, J. J. Stevenson, New York; Section F, J. M. Coulter, Crawfordeville, Ind.; Section H, Joseph Jastrow, Madison, Wis.; Section I, Edmund J. James, Philadelphia, Pa.; Permanent Secretary, F. W. Putnam, Cambridge, Mass. (holds over); General Secretary, Harvey W. Wiley, Washington, D.C.; Secretary of the Council, Amos W. Butler, Brookville, Ind.; Treasurer, William Lilly, Manch Chunk. Secretaries of the sections: Section A, E. D. Preston, Washington, D.C.; Section B, A. McFarlane, Austin, Texas; Section C, T. H. Norton, Cincinnati, O.; Section D, William Kent, New York; Section E, W. J. McGee, Washington, D.C.; Section F, A. J. Cook, Agricultural College, Mich.; Section H, W. H. Holmes, Washington, D.C.; Section I, B. E. Vernon, Washington, D.C.

This ticket was elected as reported from the nominating committee, except that a substitution was made in the Vice-President for Section I, which is notable as the first instance in the history of the Association in which any change was ever made in the list of nominees reported.

WM. H. HALE.

CHEMISTRY AT THE BRITISH ASSOCIATION

MANY of the papers read in Section B this year were of considerable theoretical importance. Additional interest was also given to the proceedings by the presence of several distinguished foreign guests.

After the President's Address, Prof. Dunstan read the third Report of the Committee on the present methods of teaching chemistry. During the past year the Committee has been principally engaged in collecting and comparing the regulations issued by the more important of the examining bodies in the kingdom, in order to discover how far their requirements were in harmony with such a course of instruction as that suggested by the Committee in their second Report, presented at the Newcastle-on-Tyne meeting. The Committee direct special attention to the following points:—

It is of great importance that natural science should be sufficiently represented on the board which issues the regulations and is responsible for the proper conduct of the examination.

In addition to examinations, periodical inspection of the teaching seems desirable, the reports of the inspectors as well as the students' own record of work testified to by the teacher being taken into account in awarding prizes, certificates and grants, in addition to the results of an examination.

With respect to the schedules and examination papers, for the most part they do not aim at an educational training of the kind suggested in the Committee's last report, being on the other hand more suitable for those who wish to make a special and detailed study of chemistry as a science. The obvious conclusion is that the necessary reforms can only be brought about by the active co-operation of examiners and teachers.

Sir Henry Roscoe introduced a discussion on recent legislation for facilitating the teaching of science. He drew attention to the powers given by the Technical Instruction Act of 1889, to County Councils and other local authorities, and assured his hearers that the Education Department and the Science and Art Department were extremely anxious to give local authorities a free scope, and free choice of subjects. Referring to the action of the Chancellor of the Exchequer, which placed in the hands of the County Councils this year the sum of £743,000 to be devoted, whole or in part, to the purpose of technical education, he urged upon these bodies the importance of taking full advantage of this grant. In the discussion which followed hopes were expressed that the money would not go simply towards the relief of the rates. It was also remarked that for the success of these provisions it is necessary that more attention should be given to primary education.

Dr. J. H. Gladstone and G. Gladstone read a paper on the refraction and dispersion of fluorbenzene and allied compounds. Fluorine behaves quite differently to chlorine, bromine, and iodine, as it exerts scarcely any refractive action upon the light rays, and it has the property of reversing the dispersion produced by other substances.

Dr. G. H. Bailey and J. C. Cain gave a paper on a method of quantitative analysis by weighing precipitates suspended in liquids. The object of the method is to do away with the operations of filtering and washing. The specific gravity of the precipitate having been determined once for all, it is weighed together with the supernatent liquid in a specially constructed measuring flask. The specific gravity of the supernatent liquid can be readily determined, and hence the weight of the precipitate calculated. The method is found to be rapid, and to give results of sufficient accuracy for many technical purposes.

Dr. G. H. Bailey and A. A. Read gave a paper on the behaviour of different metallic oxides when exposed to high temperatures. This is a continuation of work previously published in the Journ. Chem. Soc. on oxide of copper. The following oxides were subjected to high temperatures in an oxidizing atmosphere:—SnO_2, Bi_2O_3, V_2O_5, PbO WO_3, MoO_3. The following results were obtained:—V_2O_5 was converted into V_2O_3, SnO_2 lost weight slightly, and MoO_3 lost oxygen, and was transformed into the blue oxide of molybdenum, the others were unchanged. It was suggested that some light might be thrown by the experiments on the formation of minerals in nature.

A paper was then read by Dr. G. H. Bailey on the spectrum of the haloid salts of didymium. The influence of dilution and of various reagents on the intensity of the different bands was studied. It was found that the addition of nitric acid to the solution of didymium chloride influenced some bands quite differently to others. Again the variation of the halogen element, in combination with the didymium, brought about differences in the relative positions of the bands. In addition to these, observations were also made on the effect of polarised light. Each of these different conditions influenced the bands sometimes in intensity, sometimes in position, and this in a selective manner. The connection was pointed out between these results and the experiments of Welsbach on the fractionation of didymium.

Prof. Armstrong read the fifth Report of the Committee on isomeric naphthalene derivatives. A complete set of reference compounds has now been prepared in the disubstituted series. It is found that although 13 dichlor naphthalenes have been

described only 10 exist. Of the 14 possible triderivatives 13 are known. Light has been thrown by these researches on the mode of action of reagents upon naphthalene and other hydrocarbons, and it appears that in all cases the initial action is the same, the ultimate product depending on secondary causes, *e.g.*, in the case of benzene an ortho compound is always first obtained, meta and para compounds being produced by secondary causes. The influence of structure on the colouring properties of naphthalene derivatives has also been studied in connection with these researches.

Prof. J. H. Van't Hoff read a paper on the behaviour of copper potassium chloride and its aqueous solutions at different temperatures.

This compound, which is a blue salt, splits up on heating into potassium chloride, water, and a brown double salt, according to the following equation: $CuCl_2\ 2KCl_2H_2O = CuCl_2KCl + KCl + 2H_2O$. On cooling the reverse change takes place. The brown salt can also be formed by the action of cupric chloride on the blue salt thus: $CuCl_2\ 2KCl\ 2H_2O + CuCl_2\ 2H_2O = 2CuCl_2KCl + 4H_2O$. The changes of volume attending these transformations have been studied, also the solubility of the various constituents of the system at different temperatures, and the vapour pressure of their solutions, and interesting relations are shown to exist between the values obtained in each case.

Dr. Richardson read the Report of the Committee for the investigation of the action of light on the hydracids of the halogens in presence of oxygen.

It has been found that the presence of 10 per cent. hydrochloric acid prevents all decomposition of chlorine water, even after long exposure to sunshine.

Aqueous solutions of pure bromine and iodine have been exposed to sunlight for a period of fourteen months. It was found that, in a dilute solution of bromine water (0·16 per cent. Br.), as much as 57 per cent. of the total bromine is converted into hydrogen bromide; in a saturated solution the minimum amount of decomposition occurs, again increasing with further additions of bromine. With iodine water under an atmosphere of carbon dioxide, 8·3 per cent. of the total iodine in the solution was converted into hydrogen iodide. Under an atmosphere of air 14·2 per cent. of the total iodine was converted. Further experiments have been made on the oxidation of gaseous hydrogen bromide in sunlight. The presence of free bromine exercises a retarding influence on the decomposition.

The influence of temperature on the oxidation of hydrogen chloride and bromide has been studied. Rise of temperature appears to retard oxidation in the first case and accelerate it in the second.

Profs. Liveing and Dewar gave a paper on some experiments on the explosion of gases under high pressure. It was found that with increase of pressure the luminosity of the flame steadily increased. When hydrogen was exploded with excess of oxygen, it was found that large quantities of nitrogen peroxide were formed from the nitrogen present as impurity in the oxygen. The water formed contained 3 per cent. of nitric acid. With excess of hydrogen small quantities of ammonia were formed. It was found that, in an atmosphere of carbon dioxide, it was very difficult to maintain the oxy-hydrogen flame if the pressure exceeded two atmospheres. Experiments were also made with ethylene and cyanogen exploded with oxygen.

Prof. H. B. Dixon and J. A. Harker gave a paper on the rates of explosion of hydrogen and chlorine in the dry and wet states. They showed that there was no great difference in the rate, such as they had previously found with carbonic oxide and oxygen mixtures, thus showing that, in the case of hydrogen and chlorine, the aqueous vapour simply acts like any other inert gas, making the rate a little slower.

Dr. G. S. Turpin read a paper on the ignition of explosive gaseous mixtures. The author has commenced a thorough investigation of the conditions affecting the ignition of explosive mixtures of gases, and the present paper gives an account of the results obtained in a series of experiments on the temperatures of ignition of various mixtures of CS_2 vapour with oxygen and other gases. The method used is a modification of Mallard and Chatelier's second method, in which the gases are introduced into a heated and exhausted bulb. The existence of a discontinuity between gradual combustion and ignition proper is found to exist in some cases, while in others there is a perfect gradation from slow combination, attended by a faint glow, to instantaneous combination, attended by a bright flame. The effect of change of pressure on the ignition was examined and found to be somewhat complex.

The Report of the Committee on the properties of solutions was read by Dr. Nicol. The experiments have now been completed on the solubility of a salt in a solution of another salt, of known strength. In general a salt is less soluble in a salt solution than in pure water. An exception is the case of the solubility of KNO_3 in solution of $NaNO_3$.

A joint discussion with Section A on the nature of solution and its connection with osmotic pressure was opened by Prof. Pickering, in a paper on the present position of the hydrate theory of solution. The supporters of the hydrate theory claim that the curved figures, representing the properties of solutions of various strengths, show sudden changes of curvature at certain points, which are the same whatever be the property examined, which correspond to the composition of definite hydrates, and which, therefore, can only be explained by the presence of these hydrates in the solutions; while the supporters of the physical theory, now identified with the supporters of the osmotic pressure theory, claim to have shown that, with weak solutions at any rate, the dissolved substance obeys all the laws which are applicable to gases, and that, therefore, its molecules must be uninfluenced by, and uncombined with, those of the solvent.

With regard to the lowering of the freezing-point of a solvent, the following questions were proposed:—

(1) Is the molecular depression (*i.e.* that produced as calculated for one molecule dissolved in 100 molecules) constant, independent of the nature of the solvent?

(2) Is it independent of the strength of the solution, so long as this strength does not exceed the limits (gas strength) above mentioned? (Boyle's law).

(3) Is it independent of the nature of the dissolved substance? (Avogadro's law).

Evidence was adduced involving a negative answer to each of these questions.

Objection was taken to the theory of dissociation into ions, on the grounds of its irreconcilability with our ideas of the relative stability of various bodies, and with the principle of conservation of energy.

A letter was afterwards read from Prof. Arrhenius in which it was shown that both the osmotic pressure and the electrical dissociation theories must be taken into account in drawing conclusions from observed numbers.

Prof. Armstrong remarked that, according to the electrical dissociation theory, hydrochloric acid and water must be regarded as entirely different substances, whereas in their chemical relations they are very nearly allied.

Prof. Fitzgerald, Prof. Ostwald, and Prof. Lodge all spoke to the effect that Ostwald's experiment, on the decomposing effect of a charged body on a salt solution, does not involve a contradiction of the principle of the conservation of energy.

P. J. Hartog and J. A. Harker described a convenient form of apparatus for determining freezing-points, and for performing reactions in the cold. Adopting a proposal of Raoult, the evaporation of a volatile liquid is used to produce low temperatures.

A paper was given by A. G. Green, C. F. Cross, and E. J. Bevan, on a method of photographic dyeing and printing. It was observed that the diazo-compound of primuline was decomposed by light, thereby losing its property of combining with phenols and amines. If a material, dyed with diazotized primuline, be exposed to light under a design, those parts which are acted upon by light will be decomposed, whilst the parts protected from the light will remain unaltered, and consequently on subsequent development with a phenol or amine, will produce colours, whilst the decomposed portions will not.

Prof. Thorpe gave a demonstration of some of the most striking properties of phosphorous oxide. He believes that the physiological effects usually ascribed to phosphorus are due in reality to this oxide.

Prof. R. Meldola read a paper on diazo-amido-compounds, a study in chemical isomerism. The paper dealt largely with heterogeneous diazo-amides, which the author believes have the general constitutional formula—

$$\begin{array}{l} X\text{—}N\text{—}N\text{—}NR'\text{—}Y \\ \quad\;\; | \quad\;\; | \\ Y\text{—}N\text{—}N\text{—}NR'\text{—}X \end{array}$$

thus being derivatives of a hypothetical tetraimine—

$$\begin{array}{l} HN\text{—}NH \\ \;\; | \qquad | \\ HN\text{—}NH \end{array}.$$

Compounds have been prepared of all degrees of stability, from well-defined individuals to molecular compounds. The above general formula has been given for chemical reasons. A molecular weight corresponding to half that represented by the above formula is given by Raoult's method, but it is believed that dissociation takes place in solution.

C. H. Bothamley read a paper on the action of phosphorus trichloride on organic acids and on water. The equation given in the text-books, representing the action of phosphorus trichloride on organic acids, is shown to be incorrect. An equation given previously by Thorpe is confirmed, viz.:—

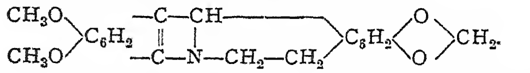
$$3CH_3COOH + 2PCl_3 = 3CH_3COCl + P_2O_3 + 3HCl.$$

The reaction, however, only takes place according to this equation in the case of acids of low molecular weight, and when the reacting substances are present in the required proportions. As the molecular weight of the acid increases the reaction tends to become more complex.

A paper was read by Prof. W. H. Perkin, Jun., on the constitution of the alkaloid berberin. On treatment with permanganate the alkaloid yields three principal oxidation products of the following empirical formulæ:—

$$C_{20}H_{17}NO_8, C_{20}H_{17}NO_7, C_{20}H_{17}NO_6.$$

From the results of the careful investigation of these, the following formula has been proposed for berberin:—



In the course of the meeting interesting discourses were given by Dr. W. H. Perkin on the development of the coal-tar colour industry since 1880, and by Prof. Hummel on fast and fugitive coal-tar colours.

GEOLOGY AT THE BRITISH ASSOCIATION.

THE short but extremely useful address of the President, A. H. Green, Professor of Geology at Oxford, formerly of Mathematics and Geology at the Yorkshire College, dealt with the educational aspect of Geology. Although he dwelt on the risk of becoming loose reasoners, which geologists continually ran, the President pointed out how by a proper training in minute and delicate experimental work, the student might be taught the necessity of exactness, and could then proceed to practical work, which would lead him into the open air, and compel him to acquire the eye and enthusiasm necessary for geological research.

Amongst the reports presented to the section was one by Prof. T. R. Jones, describing a *Saccocaris* from the Arenig, *Aristozoe* and *Estheria* from the Devonian and Carboniferous; one by Mr. G. R. Vine, giving lists of Cretaceous Polyzoa from the Neocomian, Gault, Upper Greensand, Cambridge Greensand, and Red Chalk; one from Mr. A. Bell giving the lists of fossils obtained from the "manure gravels" of Wexford, by which he is able to indicate the date of the final separation of Ireland from England; and one from Mr. Marr, giving the proposed circular letter and record sheet to be issued to the curators of public and private museums, in order to obtain a reliable register of the location of all type specimens of fossils.

Mr. Jeffs presented the report of the Photograph Committee containing a list of about 300 photographs of geological interest, and suggestions for their collection and registration; he also exhibited photographs collected during the present year, amongst which some from Yorkshire, Antrim, and Scotland were of especial value; Dr. H. Johnston-Lavis gave an elaborate report on the volcanic phenomena of Vesuvius, including a plan of the cone in April 1890, an estimate of the lava extruded between May and December, 1889, and a general record of the doings of the volcano in the year; while Mr. De Rance's report on underground water included an immense number of well sections in different parts of England.

The chief papers contributed to the section were perhaps those on local geology, some of which gave the results of many years' research. Mr. Holgate described the coals and clays of Leeds, and showed that the colour and texture of the containing rock was influenced by the nature of its fossils; thus, the more delicate plants like ferns give blue, larger plants black, and animal remains hard, black rocks. He followed with a paper on the physical properties of coals, in which he showed that coals with a dull black colour and a wide cleat were chiefly made of spores, with but little fusible ash, and were the best for use where the temperature is high; while the bright, soft coals, with close cleavage, made largely of mineral charcoal and probably of plant stems, contained much fusible ash, broke up in burning and formed slow burning, caking coals. Mr. J. R. Dakyns described the setting in of the Yoredale beds in Yorkshire, and the gradual changes which occur in them and in the lower and upper Millstone grit as the beds are traced northwards; Messrs. Cash and Lomax accentuated the identity of *Lepidophloios* and *Lepidodendron*, of which plants a magnificent series of slices was displayed in the temporary museum; Mr. J. W. Davis stated that fish remains had been found at nine horizons in the West Riding coal-field, from two of which, one above the Better bed and the other in the Adwalton Cannel of Tingley, no less than sixty species of fish and some of Labyrinthodonts had been described. Mr. Tate identified the so-called "Ingleton Granite" as a quartzose volcanic tuff, and Phillips's dyke at Ingleton as a mica-trap belonging to the minette group; Dr. Hatch also described mica-trap dykes from nine localities in West Yorkshire. Prof. Silvanus Thompson gave the results of experiments on the sources of the river Aire made by means of the fluorescent properties of uranin, and Mr. Maule Cole described a lacustrine deposit of post-glacial age near Filey.

Mr. Lamplugh dealt with the famous cliff section at Speeton, which was in capital condition for the inspection of a geological excursion on Saturday. He divided it into five zones by its belemnites, and by means of this classification was able to correlate its divisions with those of Lincolnshire. The same author gave a table of the Yorkshire boulders, from which he concluded that the North Sea ice stream drove that from the valleys of the Tees and other northern rivers southward and pressed it against the high eastern coast of Yorkshire. In a second paper he argued that the North Sea ice which formed the Basement Clay with its shelly inclusions, overtopped the Speeton cliffs and overrode Flamborough Head, passing into Bridlington Bay; the purple clays of Holderness were the equivalent of gravels of the interior and to the north; whilst the Upper Boulder Clay (and Hessle Clay) was formed by the retirement of extra-British ice and the increase of that from the Pennine high land. Mr. Lamplugh also presented a final list of mammals and shells from the ancient sea beach of Bridlington, which is earlier in date than the first glaciation of the Yorkshire coast. In connection with local glacial work may be mentioned Dr. Crosskey's report on erratic blocks; he exhibited a fine map of the distribution of the principal boulders in the Midlands, gave lists of boulders from Warwickshire, Lancashire, Cheshire, Isle of Man, and Yorkshire, and attributed their deposit to at least two distinct periods; Mr. Kendall's note on the occurrence of Eskdale and Scotch granites and local rocks in the glacial drifts of the Isle of Man; the account of the boulders of Scotch and Cumbrian granites and other rocks from the Cheshire area by Messrs. Antrobus and Hatch; and a paper by Mr. E. Jones describing the find of neolithic burials in the Elbolton cave near Skipton.

Taking the more general papers in order, we come to two papers by Dr. Hicks, one on earth-movements and their effects on Archæan and Lower Palæozoic rocks in Wales and Shropshire, and a second on the contents of Cambrian conglomerates, which provoked some discussion. In the latter he identifies twenty types of fragments, many of which must have been derived from Archæan rocks, while in the former he attributes many phenomena to earth-movement, which have often been put down to intrusion. Mr. Morgan noted the occurrence of Llandovery rocks in Montgomeryshire, and Mr. Watts correlated so far as possible the Silurian rocks of the Long Mountain with those of the typical Silurian areas of Wenlock and Ludlow. An important paper, establishing correlations in the Devonian rocks of South Devon and Cornwall, was read by Mr. Ussher, and one on an unconformity involving the absence of two zones in the Upper Lias of Bridport, by Mr. J. F. Walker. Mr. Whitaker suggested that trials for coal in the south-east of England might well be made in such localities as St. Margaret's, Chartham, Chatham, Bushey, and Coombs, where borings had already given some idea of the thickness and character of the secondary rocks. Mr. G. H. Morton showed that the Liverpool Bunter was 1950 feet in thickness and the Keuper, of which only the lower part is exposed, 800 feet; two important pebble beds

occur in the succession. Mr. A. Irving dealt with the chemical and geological characters of the Bagshot sands, their bedding, and fossils, and argued that they must have been deposited in an estuary opening on the sea.

Chief amongst the palæontological papers must be placed Prof. Marsh's restorations and descriptions of the Ceratopsidæ, of the skulls of which he exhibited life-size diagrams, some more than six feet in length. Prof. Seeley gave a description of the mural arch in the Ichthyosauria from Liassic and Oolitic specimens. Mr. Smith Woodward exhibited five examples and plates of fishes from the Hawkesbury series, and, on behalf of Prof. Anton Fritsch, plates and descriptions of Palæozoic Elasmobranchs, while Dr. P. H. Carpenter, dealing with the morphology of the Cystidea, compared them with the Crinoids and Blastoids, and suggested that in forms without a genital pore the anal pyramid may have subserved generative functions, while in two forms a fourth opening may possibly have been nephridial in function.

With the exception of local papers, petrology was thinly represented. Mr. Hunt read a paper on the saline inclusions of the Dartmoor granite, and favoured the idea of their derivation from the sea; and Mr. Brindley gave a useful account of the principal marbles of the Mediterranean—a pendant to Sir Lambert Playfair's address to the Geographical Section. Other foreign papers were, one on the geology of Nicaragua, and a second on human footprints in recent volcanic mud in the same area, by Dr. J. Crawford, an account of the minerals of New South Wales, including coal, gold, silver, tin, copper, antimony, iron, diamonds, and ornamental stones, by Mr. C. S. Wilkinson, and a paper on the seismic origin of the "Barisál Guns" of the Gangetic delta, by Mr. T. La Touche.

There only remain to be mentioned, Dr. Tempest Anderson's photographs and descriptions of landslips and volcanoes in Iceland; Mr. Logan Lobley's paper containing an estimate of the gold scattered through the pyrites in the clays and chalk of south-east England; Mr. Hart on volcanic paroxysms; and a paper by Mr. Browne on historical evidences for changes of sea and land levels in the south-east of England.

MECHANICS AT THE BRITISH ASSOCIATION.

THERE was a full programme in Section G at the recent meeting of the British Association at Leeds. It is questionable, however, whether quantity was not obtained somewhat at the expense of quality. We are aware of the great difficulty there is in regulating the supply of papers in the Mechanical Section, and so long as the present mode of procedure remains in force the difficulty will also remain. There should be a limit to the number of papers to be read, and there should be a fixed day on which contributions might be sent in. The day being fixed, it should be adhered to with absolute severity—not the names of all the professors and all the science-knights should suffice to break the law. The papers that were deemed most worthy would be accepted, whilst those with less merit would be returned with thanks. This would create a competition amongst contributors, and would-be contributors, which would, we are sure, have a most healthy influence on the proceedings of the Section. We do not make these remarks simply by the way; the fact is, the proceedings in Section G are becoming of a scrambling and hap-hazard character. It is not long since that one gentleman in this Section read a paper he had previously read before the Institution of Naval Architects. He did not take the matter and re-dress it, but calmly read from the proceedings of the latter society, word for word. This year we have had a great deal of matter that has already appeared in some of the technical journals. The discussions on the papers were, as a natural consequence, generally of a poor description. There was so much to get through that the president was obliged to be constantly hurrying, and any one who was not of the elect was treated with somewhat scant ceremony. As no one knew what the papers were to be about, the most that could be said as a rule was of a superficial and commonplace character, some of the most noted exponents of this school of discussion being especially to the fore. It is very certain that, unless Section G sets its house in order, the mechanical science of the British Association will become a byeword amongst engineers. When one contrasts the scant and listless audience at Leeds last week with that at a meeting of the Institution of Civil Engineers, or of the Institution of Naval Architects—the meetings of the latter are more akin to those of the British Association—one cannot but feel that there is need for very radical reform. The two chief reforms we would suggest would be that a limit should be put to the number of contributions, and that abstracts should be printed in good time and copies be previously sent to members and associates on application. The former would raise the quality of the papers—because that which every one can get no one values—whilst the latter would raise the quality of the discussions.

There were thirty items on the five days' programme in Section G, namely twenty-seven papers, two reports of Committees, and the Presidential address of Captain Noble. The proceedings commenced at noon of Thursday the 4th inst., a later hour than usual being selected in order that the members of the mechanical section might hear the Presidential address of Dr. Glaisher in the Mathematical and Physical Section. Captain Noble's address we have already printed in full.

The first paper on the list was by Mr. J. F. Green, of Blackwall, and was entitled "Steam Life-Boats." The historic firm at Blackwall Yard have at last succeeded in solving a problem, oft attempted but never before with success, and have produced a steam life-boat which has given satisfaction to the Royal National Life-boat Institution. The vessel is driven by the reaction of a stream from a turbine, a mode of propulsion which certainly finds a useful position for life-boat work, whatever may be its shortcomings in the matter of mechanical efficiency. The boat has been placed on the Harwich station, and gives, we think, every promise of success. The great question is undoubtedly that of expense, first cost of boat and cost of upkeep. That however is a matter to be settled by Messrs. Green and the Life-boat Institution. We would suggest that this boat might be improved by the use of liquid fuel on the principle adopted by Messrs. Doxford, of Sunderland, and applied by them to the big torpedo boat they have recently constructed. We know the danger of including too many experiments in one vessel, but now that Messrs. Green have proved their design so far they might venture a step further; and we can speak as to the practicability of the liquid fuel system in question.

"The Victoria Torpedo" was the title of the next paper, which was contributed by Mr. G. R. Murphy. This weapon, which, like all other torpedoes, is to beat everything that has gone before in murderous potentiality, has not yet assumed tangible shape, but the form it is to take when completed was fully illustrated and described in the columns of one of our technical contemporaries a few weeks ago. A paper on aluminium bronze, which calls for no special comment, came next, and was followed by one of the most interesting contributions to the section, in the shape of a paper by Prof. Barr and Dr. Stroud, on new telemeters and range finders. Without illustrations we could not give a fair description of the ingenious instruments, in which the authors of the paper have applied certain mathematical laws to judging of distance, and we will therefore leave the subject for a future occasion.

On the following day, Friday, the 5th inst., the proceedings commenced with the reading of two reports of Committees, namely the Estuaries Committee, and the Graphic Methods Committee. Both these were very brief, and consisted in substance in saying that the work was still progressing. A paper on the manufacture of netting from sheet metal dealt with a process already described in a technical journal. A number of short slits are made in a sheet of metal by a special shearing press, and the slits are opened out so as to form a number of diamond-shaped holes. The invention is ingenious, and the "netting" possesses the great merit of rigidity. Cable tramways next occupied the attention of the Section; Mr. W. N. Colam reading a paper in which he described certain devices which he has devised in connection with this means of dealing with passenger traffic. The "Serve" tube and the simplex brake were the subjects of two papers by Mr. W. B. Marshall. The former is for boiler tubes, and has ribs of metal running the whole length of the interior of the tube. These ribs extend down into the stream of hot gases, and so absorb much of the heat that would otherwise go to the uptake. Of course the heating surface of the tube is much increased, and this is effective heating surface, as the resistance to absorption is greatest at the surface. The Thorne Type Composing Machine, which next came before the Section, appeared to us as an old friend which we think made its *début* in Europe at the American Exhibition, if not before, and was duly illustrated and described in the technical journals of the day. The Bénier hot air motor had

also previously made its appearance in an engineering publication, but the contribution of Mr. Vernon on this subject was taken as read.

On Saturday, the 8th inst., only three papers were taken. Prof. A. Lupton read a contribution on the pneumatic distribution of power; in the course of which he gave some interesting details of the important system which is now working at Birmingham. This paper gave rise to a good discussion, in the course of which the author was sharply taken to task for the efficiency he claimed for the system. It should be pointed out, however, that Mr. Lupton did not speak of "efficiency" as looked at from a scientific standpoint, but from a commercial point of view, which enabled him to take credit for certain waste heat, not obtained from the power installation, which would otherwise be thrown away. This was plainly stated in the paper. Mr. F. G. M. Stoney's paper on the construction of sluices for rivers, &c., was next read. The subject was of course well treated by the author, and the paper was acceptable; but there was little novel in it, except the reference to the new tilting sluices which are to be put up in connection with the new lock at Richmond. Mr. Cope Whitehouse's paper on the Raiyan reservoir was listened to by a thin audience, the preparation for the afternoon's excursions calling the majority away.

Monday in Section G is now given over to applied electricity, and there is invariably a large influx of the more abstract A's into the section. The Leeds meeting was no exception to this rule, and when Sir William Thomson opened the proceedings by reading his paper the People's Hall, which the section occupied, had quite a crowded appearance. The subject which first occupied Sir William's attention was the new electric meter which he has recently brought out. This apparatus is yet in the experimental stage. Perhaps Sir William will be able to do something towards cheapening the design. An example of the meter was shown in operation on the platform. In the discussion which followed, Prof. Fleming made some pertinent remarks on the effect of rough and smooth surfaces. The multicellular voltmeter and the engine-room voltmeter described by the author had previously been brought before the public through the medium of technical literature. A new form of voltapile, also described, was an instrument which was intended for standardizing operations. Mr. Gisbert Kapp described the Lineff system of electric traction, by means of which a partially buried conductor can be used with safety to man and beast. Messrs. Lawrence and Harries next read a paper on alternate *v.* continuous currents in relation to the human body. No doubt at times the effect of electrical currents on the human body possesses a very intense interest for engineers, nevertheless the paper was hardly suitable for the Mechanical Section. It is well, however, that engineers should remember, as was stated in the paper, that not voltage only, but current strength is the important factor in estimating the danger from accidental contact. In the discussion which followed, the late American execution naturally occupied a prominent place. Mr. Wilson Hartnell brought the meeting back to a more mechanical complexion by reading a paper on electric lighting and fire insurance rules, illustrating his remarks by practical examples. He succeeded pretty conclusively in showing that the fire insurance companies want instruction in electrical matters, and, we think, at the same time, he surprised some of those present, who certainly have had considerable experience in electrical matters, by the result of his experiments. The paper was eminently practical and worthy of study by engineers. The last paper on the list for the day was by Mr. W. J. S. Barber Starkey on secondary batteries, in which the author described his system of adding carbonate of soda to secondary batteries. The subject is not new.

Tuesday, the 9th inst., was the last day on which Section G met. Mr. Preece first occupied about five minutes in reading a short contribution on submarine cables for long distance telephony. Mr. F. Higgins next exhibited the "Column Printing Machine," after which Mr. Arthur Greenwood read his paper on heavy lathes. Mr. W. Bayley Marshall followed with a suggestive paper on factors of safety, in which he gave the results of a large number of tests of iron and steel extending over a period of five or six years. The conclusion he had come to was that in roof and bridge work elastic limit, and not ultimate tensile strength, should be the important factor, but in the discussion that followed, which was the best discussion during the meeting, the pertinent question was raised as to what "elastic limit" is. A paper by Mr. J. H. Wicksteed on the measurement of elongation in test samples was also well discussed. A paper by Mr. A. Mallock, on the measurement of strains, in which the author described an instrument he had devised for the purpose, and an exhibition by Prof. Barr of a mechanism for giving vertical motion to a camera, brought the business of the Section to a close.

SCIENTIFIC SERIALS.

American Journal of Science, September.—Rocky mountain protaxis and the post-cretaceous mountain-making along its course, by J. D. Dana.—The magneto-optical generation of electricity, by Dr. Sheldon. It is well known that, by using proper conditions, a beam of plane polarized light may be rotated by an electromagnet, and that a reversal of the current causes the plane to be rotated in the opposite direction. A rapidly alternating current thus produces a rapid swinging to and fro of the plane of light. The author has conducted the converse experiment, and by oscillating the plane of polarization through 90° about 300 times per second, has produced an alternating current.—Contributions to mineralogy, No. 49, by F. A. Genth, with crystallographic notes by S. L. Penfield. The results are given of the examination of some specimens of ferric sulphate from Mina de la Compania, Chili.—Chalcopyrite crystals from the French Creek Iron Mines, St. Peter, Chester County, Pa., by S. L. Penfield.—Koninckina and related genera, by Dr. Charles E. Beecher.—The effect of pressure on the electrical conductivity of liquids, by C. Barus. It is shown that, both in the case of mercury and a concentrated solution of zinc sulphate, the effect of isothermal compression is a decrement of resistance nearly proportional to pressure, and from this fact the deduction is made that the immediate effect of rise of temperature is a decrement of specific resistance.—Notice of two new iron meteorites from Hamilton County, Texas, and Puquois, Chili, by Edwin E. Howell. Analyses of the two meteorites are given.—The Cretaceous of Manitoba, by J. B. Tyrrell.—On mordenite, by Louis V. Pirsson.—Geology of Mon Louis Island, Mobile Bay, by Daniel W. Langdon, Jun.—On Leptænisca, a new genus of Brachiopod from the Lower Helderberg group, by Dr. Charles E. Beecher.—North American species of Strophalosia, by the same author.—Notes on the microscopic structure of oolite, with analyses, by Erwin H. Barbour and Joseph Torrey, Jun.

L'Anthropologie, sous la direction de MM. Cartailhac, Hamy, et Topinard, tome i., Nos. 3 and 4 (Paris, 1890).—The exotic races at the Exhibition in Paris, 1889, by MM. Deniker and Laloy. In this report the authors give the general results of the anthropometric determinations they obtained from their examination of 145 individuals belonging to the most different races, some of which had not previously been made the subject of scientific inquiry. The value of their remarks on the various Senegalese and other South African negroes is enhanced by an admirable series of portraits, copied from spirited photographs by Prince Roland Bonaparte. From the observations of the authors, it appears that the negroes of West Africa may be divided into three or four groups, differing in physical characters. In fact, crispness of the hair, and a more or less dark coloration of the skin, seem to be the only characteristics common to all. The negro races generally are tall, have flat noses, and are of a dolichocephalous type, each group presenting, however, certain features which distinguish them from the remainder. The two leading varieties are separated by tribes which are small in stature, with a very hairy skin, and are of a marked brachycephalic type. This intermediate group is spread across Africa from the extreme east to the west, in about 2° S. and 3° N. of the equator, and it is among these peoples that the true pygmy tribes are found, which under the name of Akkas or Tiki-Tiki of the Nile, Batus of the Congo, Akoas of the Ogowe, have become known to us through Stanley and other recent explorers. According to Emin Pasha, to whom we are indebted for the few particulars that we know regarding their physical character, the mean height of these so-called negrilloes is 1·36 m., and their mean cephalic index 79; brachycephalism being a marked character in all the pygmy tribes. Very complete tables are given by the authors.—New explorations at Solutré, by M. A. Arcelin. Palæontologists will welcome the report here given of the various explorations that have been in progress at Solutré since these important deposits were first made the subject of scientific inquiry in 1866. The extent of the beds, which at some points are fully

ten metres in depth, has retarded the work, which is of a complicated nature in consequence of the different groups of materials that have been brought to light, and which include two distinct *foyers*, belonging the one to the reindeer age, and the other to a probably earlier period, besides numerous sepulchral remains and several accumulations of the bones of horses. The latter are perhaps the most curious of the Solutré finds, since within an area of about 4000 metres there is a circular embankment constructed of horse-bones so densely packed that it is estimated to contain the remains of no less than 10,000 animals. According to the author, these bone-mounds may be regarded as the *kökken möddings* of the early men of Solutré, whose principal food must therefore have been horse-flesh.—A note on two Phœnician skulls found in Tunis, by Dr. Bertholon.—Art among the barbarian races at the fall of the Roman Empire, by Baron J. de Baye. The author shows how greatly archæology has gained in recent times by the researches of French and other men of science in regard to art among the barbarian nations. In France the Abbé Cochet, by his clear definitions of the distinctive features of industrial art among races of Burgundian and Frankish origin, has given a new and firm basis to mediæval archæology, and to him we are indebted for several very important works on the forms and symbolical character of barbaric ornamentation, which is now shown to be common to peoples of the most widely separated countries. The present article is copiously illustrated with drawings of buckles and other ornaments presenting symbolical designs, which have been found not only in Central Europe, but in Russia, the Crimea, and Northern Caucasia. From a careful study of these objects, which have ordinarily been referred to as specimens of Gothic art, it would appear that so-called Gothic forms of ornamentation have an eastern origin, and were gradually vulgarized by barbarian tribes in their passage westward.—A history of the so-called Oppidum de Castel-Meur en Cléden (Finistère), by Paul du Chatellier.—The muscles of the face in a negro of Ashantee, by Dr. Popovsky. This case, according to the author, supplies an instance of the interlacing of the facial muscles, which is not unfrequent among the inferior races, and belongs to a class of anomalies presenting a strongly-marked character of atavism.

Bulletin de l'Académie des Sciences de St. Pétersbourg, nouvelle série, vol. i., Nos. 2 and 3.—The chief papers (in French or German) are:—On the normal variations and the perturbations of magnetical declination, by H. Wild.—On some (seven) species of Russian and Siberian earthworms, by N. Kulaguin.—New contributions relative to the *Olenellus mickwitzi* from the Lower Cambrian deposits of Esthonia, by Fr. Schmidt.—On the quantitative determination of antimonium and sodium, by F. Beilstein and O. Blaese.—A formula for the computation of the length of the arcs of longitude upon the earth ellipsoid, by A. Bonsdorff.—The bases of a mathematical theory of the interior diffusion of light, by Dr. O. Chwolson. The general solution of the problem is not possible; but, on the hypothesis that the interior diffusion of light in a transparent body is due to particles of matter which reflect the light, and can be considered as independent sources of light, the author, after having established the general theory, discusses several special cases in which the problem appears simplified to some extent.—Sahidic fragments of the Bible, by O. Lemm.—Fishes from the Lower Silurian deposits, by J. Rohon. The little hooks, described by Pander as "Conodonts," which formerly were taken for teeth of fishes, but are now considered to have belonged to Annelids and *Gephyreæ*, are accompanied by real teeth of Vertebrata which wholly differ from them, and prove that fishes were living at the earliest times of the Silurian epoch as well.—Report of the Russian delegates to the Paris Conference upon Metrical Measures, by H. Wild and O. Backlund.—On the ancient Turkish dialects: (1) Seldschuk verses in the Rebab-Nâmeh, by W. Radloff.—Ad Plutarchi quæ feruntur Moralia, by P. Nikitin.—Devonian fishes from the Yenisei, by J. Rohon, followed by remarks upon the spinal cord of Devonian fishes generally.—De scholiis in Sophoclis tragædias a P. N. Papageorgio editis, by A. Nauck.—Preliminary results of his observations made upon the satellites of Saturn by means of the 30-inch refractor, by Herm. Struve. The observations were made for the purpose of determining the orbits of the interior satellites, Rhea, Dione, Tethys, Enceladus, and Mimas, and later on, the dimensions of the planet and its rings.

Memoirs of the Odessa Society of Naturalists, vol. xiv.—On the diffusion of a solution of common salt, by N. Umoff. The experiments were made on the system recommended by Sir William Thomson, by means of glass balls, and the results are given day by day for a period of six months. The result is that the law proposed by Dr. Fick for cylindrical vessels is not yet proved.—On the influence of HCl and metallic chlorides upon the photochemical decomposition of water, by E. Klimenko and G. Pekatoros.—On the excretory organs of the Invertebrates, note by A. Kovalevsky.—On isomery in the thiophene series, by N. Zelinsky. Preliminary report.—On M. Timtchenko's anemograph, which combines an anemometer with a weather-cock, by A. Klossovsky.—On some snow-storms, by the same author.—Catalogue of plants found in the neighbourhood of Kishineff (420 dicotyledons and 84 monocotyledons).—On the peritracheal cells of insects, by J. Pekarsky (with a plate).—On the action of the phosphor-pentachloride upon citric acid, by E. Klimenko and Buchstab.—On the snow-covering of South-West Russia, by P. Pantchenko.—On the Nemertinæ of Sebastopol Bay, by J. Lebedinsky. Description of a dozen species of Nemertinæ, formerly unknown at Sebastopol.—Geological exploration in the peninsula of Kertch, by N. Andrussoff. The Mediterranean Miocene deposits of Kertch belong to a basin of the Miocene sea, which extended from Varna, in the Balkan peninsula, to the Ust-Urt, and was connected in the west with the Miocene sea of Roumania and Galicia by means of one or several straits. A good deal of information supplementing the former explorations of the same author is also given.—On the history of the development of the crab *Eryphia spinifrons*, by J. Lebedinsky; an elaborate paper, illustrated by several plates.—On the excretory organs of some insects, spiders, and myriapods, note by A. Kovalevsky.

Bulletin de la Société des Naturalistes de Moscou, 1889, No. 4.—On the chief properties of meteoric showers, by Th. Bredichin (in French). After having developed in his former articles the idea that the "anomalous" tails of comets give rise to meteoric showers, which, as a rule, may appear annually with varying intensities, the author now examines into those meteoric streams which appear in great multitudes at intervals of several years.—Studies on the palæontology of Ungulata, by Marie Pavloff (in French).—The cosmical origin of naphtha, by W. Sokoloff.—Zoological researches in the Trans-Caspian region, by N. Zaroudnoi (in French). The list of mammals mentioned is now increased to 42 species, and that of birds to 309 species; the short notes about their habitats and modes of life are of the same high character as in the preceding work of the same author.

Geological Annals of the Balkan Peninsula, vol. ii., fasc. 1.—Note on the meteorite of Jelica, by J. M. Žujović. Twelve fragments of this meteorite, which fell on November 19, 1889, were collected; the largest of them weighed 3175 grammes. Its composition resembles that of a trachytic breccia. In an earthy, ash-coloured mass, porphyric elements and angular stony pieces of a dark colour, sometimes 4 centimetres long, are disseminated. The latter seem to be aggregates of crystals, probably of pyroxene. Closer microscopical examination is promised.

SOCIETIES AND ACADEMIES.

SYDNEY.

Royal Society of New South Wales, May 7.—Annual Meeting.—Prof. Liversidge, F.R.S., President, in the chair.—The Report stated that twelve new members had been elected during the year. One honorary member, the Rev. J. E. Tenison-Woods, and one corresponding member, Major-General Sir Edward Ward, R.E., had died, and the total number on the roll on April 30 was 461. During the year the Society held eight meetings, at which the following papers were read:—Annual address, by Sir Alfred Roberts. (1) Note on the composition of two sugar plantation soils; (2) well and river waters of New South Wales, by W. A. Dixon. The aborigines of Australia, by W. T. Wyndham. (1) Note on the recent rainstorm; (2) the source of the underground water in the Western Districts, by H. C. Russell, F.R.S. On the high tides of June 15–17, 1889, by John Tebbutt. List of the marine and fresh-water invertebrate fauna of Port Jackson and the neighbourhood, by T. Whitelegge. The eruptive rocks of New Zealand, by Prof. F. W. Hutton. On the application of prismatic lenses for making normal-sight magnifying spectacles, by P. J. Edmunds. Flying machine memoranda, by L. Hargrave. Irrigation in its relation to the pastoral industry of New South Wales, by H. G. McKinney. (1) The analysis of

prickly pear; (2) on the occurrence of arabin in prickly pear (*Opuntia braziliensis*), by W. M. Hamlet. Personal recollections of the aboriginal tribes once inhabiting the Adelaide Plains of South Australia, by E. Stephens. Aids to the sanitation of unsewered districts (poudrette factories), by J. Ashburton Thompson, M.D. Brux. Notes on Goulburn lime, by E. C. Manfred. Notes on some New South Wales minerals, by C. H. Mingaye. The Australian aborigines, by Rev. J. Mathew. The Medical Section held seven meetings, twelve papers were read, and numerous exhibits shown; the Microscopical Section held six meetings. The Clarke Medal for the year 1890 had been awarded to George Bennett, M.D. Univ. Glas. The Society's Bronze Medal and money prize of £25 had been awarded to J. Whitelegge, Sydney, for list of the marine and fresh-water invertebrate fauna of Port Jackson and neighbourhood; also to Rev. J. Mathew, Coburg, Victoria, for paper on the Australian aborigines; and the Council has since issued the following list of subjects with the offer of the medal and £25 for each of the best researches if of sufficient merit:—To be sent in not later than May 1, 1891: The meteorology of Australia, New Zealand, and Tasmania. Anatomy and life-history of the Echidna and Platypus. The microscopic structure of Australian rocks. To be sent in not later than May 1, 1892: On the iron ore deposits of New South Wales. On the effect which settlement in Australia has produced upon indigenous vegetation, especially the depasturing of sheep and cattle. On the coals and coal-measures of Australia.—The Chairman read the Presidential address, and the officers and Council were elected for the ensuing year, Dr. A. Leibius being President.

Paris.

Academy of Sciences, September 15.—M. Duchartre in the chair.—On the atomic weight of gadoline, by M. Lecoq de Boisbaudran. The author finds that the atomic weight of gadoline is 155·95, which agrees fairly well with the value 156·75, found previously by M. de Marignac.—Observations of the new minor planet discovered by M. Charlois, made at Paris Observatory, by M. G. Bigourdan. The nights of observation of position were September 11 and 12.—Observations of Denning's comet (1890, July 23), made with the great equatorial of Bordeaux Observatory, by MM. G. Rayet, Picart, and Courty. Some observations of position are given which extend from August 5 to September 12.—Solar phenomena observed during the first half of 1890, by M. Tacchini. (See Our Astronomical Column).—The shooting-stars of August 9 and 11, 1890, observed in Italy, by M. P. Denza. (See Our Astronomical Column.)—The tornado-cyclone of August 19, 1890, by M. L. Gauthier. The author thinks that the storm of August 19 should be called a tornado-cyclone, because of its complex character. He gives an account of secondary phenomena that accompanied it, viz. electrical manifestations, divisions of the principal branch, the conical form of the cloud, the aspiration produced by the rapid whirling of the air, and the formation of a lateral wind.—The storms of the month of August 1890, and the solar period, by M. Ch. V. Zenger. The author traces a connection between August storms, the Perseid meteors, and the sun-spot period.—On the acetic ester of acetal, by M. A. Combes.—On the *Isonandra Percha* or *Isonandra Gutta*, by M. Serullas. The author gives an account of the *Isonandra Gutta*, both as to its discovery and as to the growth of certain specimens. Some interesting information with respect to the use of gutta-percha for commercial purposes is also given.—Researches on the propagation of the vine by cuttings, by M. L. Ravaz.—Notes were also submitted by MM. Dumoulin-Froment and Doignon on the electrical gyroscope designed by M. Trouvé for the rectification of the compass; and by M. Mathieu Plessy, stating that he had discovered potassium in the supposed new base that he obtained by heating ammonium nitrate (*Comptes rendus*, August 25, 1890).

Brussels.

Academy of Sciences, August 2.—M. Stas in the chair.—On the preservation of oxyhæmoglobin when sheltered from the action of atmospheric germs, by M. Léon Fredericq. In a note published in the *Bulletin de l'Académie*, No. 2, 1890, the author recorded that oxyhæmoglobin may be preserved intact for more than a month without losing its oxygen, and without being transformed into methæmoglobin, by isolating it from the action of atmospheric germs. He has since found that the oxyhæmoglobin cannot be preserved for an unlimited period, but after a time begins to pass into methæmoglobin, and the transformation is complete at the end of a few months. It appears, in fact, that oxyhæmoglobin preserved in a sealed tube and containing atmospheric germs is transformed entirely into reduced hæmoglobin in a few days. If, however, such germs are rigorously excluded, the oxyhæmoglobin is preserved intact for a much longer period, but at length is transformed into methæmoglobin.—On the characteristic property of the common surface of two liquids in contact, by M. G. Van der Mensbrugghe.—On the reduction of invariant functions, by M. Jacques Deruyts.—On conjugate cubical involutions, by M. Cl. Servais.—Some facts with respect to aldehyde, by M. Maurice Delacre. The author brings some facts relating to the dissociation of chloral hydrate to explain why it should be a well-defined and stable compound, whilst aldehyde hydrate is unknown in an isolated state.—On the deformations produced at the surface of a hollow metallic hemisphere by the impact and by the pressure of a hard body, by M. H. Schoentjes.—Reduction of nitrates by sunlight, by M. Emile Laurent. The author has found that a solution of potassium nitrate exposed to the sun behaves as if it contained a nitrite. It has therefore been concluded that the nitrate is reduced to nitrite by the action of sunlight. Griess's reaction was employed for the identification of the nitrites.—On the reduction of nitrates by brewers' yeast and by some Mucorini, by the same author. From a series of researches it has been found that grains of barley and maize sterilized and placed in sterilized water until the shoot was one centimetre long, contain no bacteria in their tissues, and therefore have not the power to reduce nitrates. Hence the author considers the reduction of nitrates as a property common to certain microbes, and to the cells of superior plants which are developed in a medium containing no oxygen. The researches have reference to some observations made previously by M. Jorissen.

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THURSDAY, OCTOBER 2, 1890.

THE METAL OF THE FUTURE.

Aluminium: its History, Occurrence, Properties, Metallurgy, and Applications, including its Alloys. By Jos. W. Richards. (London: Sampson Low and Co., Ltd., 1890.)

AS the recent improvements in the manufacture of aluminium have been so great as to enable it to be bought now at one-tenth the price it was only three years ago, and as its uses, especially in its alloys, are becoming constantly more extended and varied, a somewhat detailed review may be of service in directing attention to this, the latest book on the subject. It is intended by the author, who is instructor in metallurgy in Lehigh University, to lay before the general public as well as before metallurgists a full and accurate account of the aluminium industry as it exists at the present time. To do this, the author has found it necessary to make such numerous and extensive additions to the first edition, that the present volume may almost be regarded as a new book.

Passing in review the various parts of the book, we come first to an admirable *résume*, of 27 pages, of the history of the progress made in reducing aluminium metal; it contains much interesting information, not easily obtainable elsewhere, describing the founding of the various works for this manufacture from the time of Deville to the present electrical processes of Cowles, of Lockport, New York; of Hall, of Pittsburg; and of Heroult, of Neuhausen, Switzerland. The affairs of Frishmuth, of Philadelphia, here mentioned, may serve as a warning to those too ready to believe reports of success from enthusiasts or from the inventors of secret processes.

Chapter ii., of 7 pages, deals with the occurrence of the compounds of aluminium in Nature. It may be interesting to remind the reader of the existence of several precious stones that contain aluminium, but a list of "some other compounds occurring frequently" is surely very misleading when it contains the minerals turquoise, lazulite, wavellite, topaz, and even cryolite; these ought to have been replaced by such minerals as the sodium- and potassium-felspars, hornblende, augite, mica, kaolin, &c. These common aluminium minerals are described, curiously enough, however, in the chapter dealing with the artificial preparation of aluminium compounds. The statement "that aluminium has never been found in animals or plants" requires correction. The description of beauxite is accompanied by many analyses; but that of cryolite, which is directly used in the manufacture of aluminium, is accompanied by an incorrectly calculated percentage composition, and the one statement that "the so-called pure article was found by Prof. Rogers, of Milwaukee, to contain 2 per cent. of silica and 1 per cent. of iron," although further details are to be found on several other pages of the book.

Chapters iii. and iv., of 31 and 13 pages, deal with the physical and chemical properties of aluminium. A list is given of analyses, and an account of various specimens of commercial metal, showing the amount of the impurities, iron and silicon, that may be contained, and the effect on the physical properties is mentioned. The chapter on the chemical properties concludes with a paragraph headed "General Observations on the Properties of Aluminium"; this, being a quotation from Deville's general theoretical considerations, is very much behind the time indeed, and should be replaced by observations made with respect to Mendeleeff's classification of the elements, and coupled to the general considerations on the "structure of aluminium compounds" that introduces the next chapter.

Chapter v. describes generally the properties and preparation of aluminium compounds, but requires some alterations; thus, on p. 86 we read, "Alumina forms no carbonate," and p. 103 is a paragraph headed "Aluminium Carbonate," describing the preparation of the compound $Al_2O_3.CO_2$; and again, p. 88, we find diaspore is $Al_2O_3.H_2O$, beauxite is $Al_2O_3.2H_2O$, and gibbsite is $Al_2O_3.3H_2O$, whereas on p. 47 it is said that "beauxite is a combination between diaspor, $Al_2O_3.3H_2O$, and brown hematite, $Fe_2O_3.3H_2O$," and on the same page is also found "Diaspore, $H_2Al_2O_4$." We must certainly disagree with the names "aluminium-ammonium chloride" for the substance $Al_2Cl_6.3NH_3$, and "aluminium fluorhydrate" for $Al_2F_6.7H_2O$, and also think that the description of some dozen substances as the double chlorides of aluminium and sulphur, phosphorus and selenium, the above ammonia compound and the selenide and selenite of aluminium, might have been entirely omitted, and the space devoted with advantage to a more detailed description of the really important compounds.

Chapter vi., of 29 pages, is a well-written account of the "Preparation of Aluminium Compounds for Reduction," and describes the preparation of alumina from crude sulphates, from beauxite and from cryolite, the preparation of aluminium chloride, and aluminium sodium chloride, and the preparation of artificial cryolite and of aluminium fluoride, and also that of the sulphide of aluminium. The numerous processes here described give one much food for thought and comparison, and though no author is really responsible for the statements of others which he may introduce, yet where the statements of two authorities do not agree, the disagreement should be mentioned and suggestions made to explain the cause of it. Two cases may be cited. On pp. 113 and 118, analyses are given of alumina precipitated by carbonic acid from sodium aluminate solutions: the one shows 2·5 and the other 20 per cent. of sodium carbonate. Again, on p. 121 we find the method of Sauerwein for preparing alumina from cryolite, viz.,

$$Al_2F_6.6NaF + Al_2O_3.3Na_2O + 6H_2O = 2(Al_2O_3.3H_2O) + 12NaF;$$

while on p. 137 is described the very reverse reaction, a method of Berzelius for preparing cryolite from alumina, viz.,

$$2Al_2O_3.3H_2O + 12NaF = Al_2F_6.6NaF + Al_2O_3.3Na_2O + 3H_2O$$

(this last item is a mistake for $6H_2O$).

Chapter vii., of 39 pages, describes "The Manufacture of Sodium," giving a full account of the older processes, and also of the recent ones of Castner and Netto, in which not sodium carbonate, but hydrate, is reduced by carbon at a red heat. There is also reference

made to the experimental preparation of sodium by electrolysis of fused salt.

The eighth chapter, 13 pages, considers "The Reduction of Aluminium Compounds from the Standpoint of Thermal Chemistry." After a short introduction, a list is given showing the heat developed in the oxidation of various metals. At the head stand magnesium and aluminium, and the author predicts the possibility of reducing alumina by magnesium under certain unknown conditions; and it is interesting here to note that C. Winkler, only a few months ago, in the course of a series of logical researches, has found that alumina, heated with magnesium, gives (according to the proportions) either finely-divided aluminium or a hitherto unknown oxide, viz. AlO, a perfectly black substance; and this was the substance for which Deville was searching in 1854, when for the first time he accidentally obtained pure aluminium in globules, an accident which led to his well-known labours in connection with this metal. The chapter concludes with an account of the thermal aspects of the formation and reduction of the chloride and sulphide of aluminium.

The next two chapters, ix. and x., of 26 and 24 pages, are headed, "The Reduction of Aluminium Compounds by Potassium or Sodium," although potassium has probably not been used for the preparation of aluminium since the experiments of Wöhler, in 1845. The first chapter is devoted to the double sodium chloride as source of the metal, and contains very full accounts of the process as practiced by Deville, and of the various improvements made up to the time of Paul Morin in 1882. Describing the patented process of Frishmuth, the author says: "In what the originality of the process consists . . . we cannot see, and we simply acquiesce blindly to the mysterious penetration of our Patent Office Board"; and the remark might fitly be applied to other patents than this particular one, and to other Patent Office Boards than that of the United States. The second chapter describes the reduction of the fluorine compounds. From the experiments and experience of H. Rose, Percy, Dick, Deville, Tissier Brothers, all fully described, the conclusion is drawn that the best use of cryolite is as a flux when reducing aluminium sodium chloride; but as a contrast to this is the account of the Alliance Aluminium Company's processes, by which 77 per cent. of the metal contained in the cryolite was extracted. The account of Grabau's processes, the reduction of aluminium fluoride by sodium, is very interesting, and especially so as he has been the first to produce on a commercial scale aluminium with less than half of 1 per cent. of impurities.

Chapter xi., of 70 pages, is one of the most important chapters in the book. It is an account of the "Reduction of Aluminium Compounds by the use of Electricity," and is introduced by an all too brief review of "the principles of electro-metallurgy as they apply to the decomposition of aluminium compounds." The method of calculating, from the heats of formation, the electromotive forces required to decompose aluminium chloride and alumina having been described, the number of volts thus found are explained to be

"the absolute minimum of intensity which would produce decomposition, and the actual intensity practically required would be greater than this, varying with the distance of the poles apart and the temperature of the bath as far as it affects the conducting power of the electrolyte. From this it would immediately follow that, if the substance to be decomposed is an absolute non-conductor of electricity, no intensity of current will be able to decompose it. If, on the other hand, the substance is a conductor, and the poles are within reasonable distance, a current of a certain intensity will always produce decomposition."

We are sure that such an explanation of phenomena that can only be successfully treated mathematically will not greatly enlighten the uninitiated, and hope that in the next edition the author will find it possible to give a more exact and fuller account of electric phenomena in so far as they apply to the subject in hand; as, for instance, an account of Ohm's law applied to electrolytes, of the chemical and thermal effects of electric currents upon electrolytes, of the chemical, electrical, and thermal effects of secondary reactions to which the products of the electrolysis may give rise, &c., and also even a brief description of the instruments and machines used to measure and generate the powerful currents used in the manufacture of aluminium. With the expression of this hope we will pass over many inaccurate and dubious expressions relating to electrical terms and descriptions.

Exceedingly curious is the account of some twenty patented processes for depositing aluminium or its alloys from aqueous solutions, and the following remarks of the author summarize the results obtained by all these enthusiastic labourers in Nature's unwilling fields :—

"We have inventors affirming in the strongest manner the successful working of their methods, while other experimenters have followed these recipes and tried almost every conceivable arrangement, yet report negative results. . . . No good authority testifies to the success of any process so far advanced, neither have I seen any so-called aluminium plating (from aqueous solution) which really was aluminium."

"The Electric Decomposition of Fused Aluminium Compounds" is treated, with the exception of a few cases, chronologically; in reviewing the chapter we shall, however, group them according to the electrolyte used, and we cannot but think that this very important chapter could have been presented more concisely in such a way.

First, then, there are accounts of the electrolysis of fused aluminium sodium chloride by Deville, Bunsen, Le Chatellier, Berthaut, and Grätzel, whose process was actually tried on a large scale, but abandoned. The processes of Omholt and Faure are amusing, inasmuch as the one melts aluminium chloride in a reverberatory furnace! and the other electrolyzes a bath of the same substance at 300°!

The remaining processes may be classified as follows: (1) electrolysis of cryolite without addition of alumina, but with or without addition of salt, &c.; (2) the same as No. 1, except that alumina is also added; (3) electrolysis of alumina dissolved in cryolite salt, &c.; (4) electrolysis of fused alumina; (5) electrically heating mixtures of alumina and carbon to such a temperature that they react upon each other chemically; (6) methods using crude clay, beauxite, or kaolin as the source of alumina, and not worthy of further consideration. To the first class belong the processes of Gaudin, Grabau, Feldman,

and perhaps also that of Rogers; the products of the action are aluminium and chlorine or fluorine. To the second class may be ascribed the process of Kleiner, perhaps that of Rogers, and that of Bernard Brothers; the products of the action are the same as the first class, but the aluminium fluoride destroyed by electrolysis is in part restored to the bath "by causing the fluorine vapours evolved to act on alumina or beauxite placed somewhere about the anode." To the third class may be ascribed the processes of Henderson, Hall, and part of Heroult's patented process; here the products are said to be aluminium and oxygen, which by contact with the carbon anode is converted into carbon monoxide or carbon dioxide, and the cryolite, &c., used as solvent for the alumina are said to remain unchanged. The fourth class contains only Heroult's process, and of that only the latter half of his claims. The fifth class contains the process of Monckton, Cowles, Menges, and Farmer.

As regards the details of these various processes reference must be made to the book. The processes of Cowles, Hall, and Heroult are reported as being in active and very extensive use by the several companies, and if one is to believe the glowing reports that are published they are very successful indeed; thus Hall claims to extract 50 per cent. of aluminium from alumina, instead of the theoretical 52·94 per cent., while the fluorides used waste only very slightly, and require replenishing to the extent of a small fraction of the weight of the metal made; and with his latest improvements aluminium is not to cost more than half a dollar a pound!

The scientific investigation of these processes is either kept secret, or, alas, has scarcely been attempted; and yet the surest and quickest way to establish a process on a sound commercial footing is to thoroughly investigate the conditions regulating every reaction, and not merely those conditions relating to the principal reaction, for those relating to the ubiquitous "impurity" are at least of equal, if not of greater, importance. In describing the above processes, the author introduces scientific and numerical discussions on several points; but the work would have been more valuable to the increasing number of metallurgists interested in the subject if the book had bristled more with hard facts expressed in figures, and with references to volume and page where the original might be found.

Chapter xii., of 31 pages, is a summary of the very many processes that have been proposed for the "Reduction of Aluminium Compounds by other means than Sodium or Electricity." Many of the accounts record the partial success of actual trials, and deserve consideration; but many are but little more than written hopes and imaginations.

As far as the end of this last chapter, the subject-matter, with the exception of chapters iii. and iv., is purely chemical, and relates, indirectly or directly, to the primary production of aluminium or of certain of its alloys. From this point the book deals with the manner of working aluminium, the preparation of its alloys, and the properties which characterize them. This metallurgical part of the book may be considered as being introduced, as far as aluminium itself is concerned, by chapters iii. and iv. and some four pages of chapter xiii., which describe the "Purification of Aluminium," and refer very briefly to Mallet's preparation of the pure metal. The special methods found suitable for the analysis of aluminium and its alloys are described in the last chapter of the book.

Chapter xiii., of 29 pages, describes fully the methods of working in aluminium—casting, rolling, annealing, soldering, &c., &c. In speaking of the uses of aluminium, the author says, when referring to its lightness,

"but I would say a word or two about the popular fallacy of aluminium replacing steel as a constructive material, . . . or in any position where its strength is of importance, . . . it is forgotten that it is only one-third as strong."

The aluminium alloys are considered in the next three chapters. Chapter xiv., of 30 pages, describes many alloys, of which the following two classes are especially important, as they promise to enter largely into commerce. The alloys with copper and nickel mostly contain but a very small proportion of aluminium, but nevertheless are superior to ordinary German silvers for strength and fineness of grain. Those containing copper and zinc, and known as aluminium brass, possess exceedingly valuable working qualities, are three and four times as strong as ordinary brass, and containing mostly only 2 or 3 per cent. of aluminium are further recommended by their low cost. Chapter xv., of 32 pages, describes the alloys with copper; of these the most important contain 5 or at most 10 per cent. of aluminium, and are known as aluminium bronzes; and full accounts of the methods of working and tests of the strengths of the metals are given. Chapter xvi., of 31 pages, describes the "Aluminium-Iron Alloys," and is a very interesting account of a difficult but exceedingly important subject. The chapter is divided into three parts, dealing with the effects produced by adding trifling quantities of aluminium to steel, to wrought-iron, and to cast-iron; in almost all cases the castings are quite free from blow-holes; and in certain cases the metal becomes more fluid, allowing of castings being more readily made. Cast wrought-iron sounds like a paradox, but it is not one, for, by adding a small amount of aluminium to wrought-iron that has been heated until it has become pasty, the latter immediately liquefies, and can then be poured into moulds, making castings as sound as if they were of grey cast-iron. The author discusses at some length the probable explanations of the effect of adding aluminium to the various kinds of iron, and his conclusions may be very briefly stated as being: (1) addition of very small quantities of aluminium, *i.e.* 0·01 to 0·1 per cent., causes the destruction of carbonic oxide or dioxide, or of the oxygen compounds, as oxide of iron, disseminated mechanically, and which at the moment of setting give rise to the formation of these gases; hence the cast metal is free from blow-holes, and, owing to the removal of suspended oxides, the metals cease to be pasty and become quite fluid; (2) addition of aluminium in larger quantity, *i.e.* 0·2 to 0·5, or even several per cents., converts the combined carbon—that is, if there be any appreciable amount—into graphitic carbon, and, according to the quality of the iron operated on and the amount of aluminium added, has the effect of rendering the castings free from a chilled surface, of making the metal very uniform in texture and hardness, or

of separating the graphite to such an extent that the metal becomes pasty and unfit for making castings. Wonderful, indeed, are the effects of traces of foreign substances on the physical properties of the metals, and, though much has been done towards studying the effect of foreign substances on the properties of iron—the metal of the past, the present, and the future also, notwithstanding all that has been said about aluminium—yet the effects of this new "impurity," aluminium, are so great that evidently not only the modern man of science, but also the time-honoured iron-master, has still much to learn.

H. BAKER.

ELECTRIC DARKNESS.

Electric Light: its Production and Use. By John W. Urquhart. Third Edition. (London: Crosby Lockwood and Son, 1890)

THIS book has the characteristic defect of many scientific works that go through several editions—the old matter is fondly retained, while edition by edition, new bits of information are inserted here and there, until finally the paragraphs must feel as awkward in one another's company as ancient Britons and gentlemen in top hats. And unfortunately Mr. Urquhart gives no hint to the readers of "Electric Light" as to which are his aboriginal paragraphs painted in woad, and which of them wear the modern frock coat.

That section of his book which is devoted to arc-lamps almost starts with a description of the *latest* form of the Brockie-Pell lamp, followed by an account of the Siemens and Hefner Alteneck pendulum and differential lamps, the Thomson-Houston, and the Brush lamps, types which may all be met with in constant use at the present day; then the author, without a word of warning that he is becoming historical, dilates on the Wallace-Farmer and the Rapieff forms. Next comes the Crompton lamp, with only a page given to it, and not thought worthy of an illustration. The reader would hardly gather from this that the Crompton lamp is extensively used in railway stations and elsewhere at home and on the Continent, and that the streets of one of the few towns in England electrically lighted—viz., Chelmsford—obtain their light wholly from Crompton lamps. We have then the description of a very excellent lamp, the Pilsen, especially in view of the improvements introduced into it by Mr. Joel; these, however, are not even referred to, Mr. Joel's contribution to electric lighting being confined, according to Mr. Urquhart, solely to his semi-incandescent lamp of 1881. And the description of the Pilsen lamp only occupies a fraction of the space devoted to the rotatory disc, the Regnier, the Werdeman, the Wilde, the Jamin blowpipe lamp, and other obsolete specimens which close this section, wherein may be found some of the most important arc-lamps of the present day indiscriminately jumbled up with types that figure only in museums and text-books.

Although the book is dated 1890, the description of Sir William Thomson's meters, to which only half a page is given, must have been written several years ago, before Sir William abandoned the use of iron, since, according to "Electric Light," all the assistance Sir William has contributed to the electric light industry is the invention of a voltmeter in which a stumpy bit of iron is attracted by a coil. The co-inventor of the Ferranti dynamo is we learn, another man, a Sir William Thompson, with a "p."

With reference to the Deptford mains we are told, "The main is composed, first, of a copper tube of small diameter surrounded by a considerable thickness of insulating material, the whole being enclosed in a copper or other metallic tube about three inches in diameter. It is to be particularly observed that the 'return' is intended to be put in connection with the earth." We should like to hear what the Postmaster-General would say to this bit of intelligence after the opposition that he offered in the spring of 1889 to the original plan being carried out, and which led to the return of the Deptford mains being insulated.

Details are given of the electric lighting of the Albert Hall by 5 arc-lamps, the author not mentioning that the words, "At the Albert Hall a saving of gas is effected, &c.," and those that follow were written in the very early days of electric lighting. And yet, so anxious to be up to date does the author profess himself to be that he states, when dealing with high candle-power lamps, "We need not enter more deeply into the question how many, because . . . calculations made in 1889 would probably not apply in 1890."

This happy indifference that he displays to the distinction between the past and present tense may very likely lead people to unfairly condemn as useless, and out of date, a good deal of solid and valuable information which the book contains. The chapter on electric distribution is distinctly good, and the chapters on dynamos may be read with profit if we set down to the author's love of living in the past the accounts he gives of the Wallace-Farmer, of the Bürgin, and of other dynamos now practically abandoned; and if we attribute to a like cause such information as the following with reference to direct-current dynamos:—"The idea of making the armature a fixture, and of causing the field magnet to revolve within it, has, . . . in several lately-constructed machines, proved a most advantageous form of construction." The section on the management of the dynamo is particularly useful, and contrasts most favourably with the large amount of historical matter the book contains. We hope, however, that the author's statement regarding a shunt dynamo, as to its probably being impossible to burn up such a machine by short-circuiting, will not be brought forward as an excuse by some beginner for short-circuiting a shunt dynamo which has been running on open circuit; because the bill that will probably have to be paid for rewinding a burnt-up armature will forcibly illustrate the importance of taking into account what the author has neglected, viz. the residual magnetism of the field-magnet cores.

The detailed instructions which are given for making simple apparatus like batteries, a laboratory magneto-Gramme machine, simple arc lamps, &c., will recommend the book to amateurs, but the author's views that the vertical slit down the cylindrical zinc of a cell is for the purpose of preventing local action, that "both sides of the zinc evolve electricity," that "electricity of

opposite name is believed to flow off in contrary directions in equal quantities from the surface of generation, viz. the junction of the liquid with the positive plate," are very crude even for amateurs.

Mr. Urquhart's account of accumulators is a trifle mixed. On p. 47, "the negative grids are filled with litharge;" but on p. 48 we have "the litharge (positive) plate"; the capacity of an accumulator with 32 lbs. of plate is stated to be 50 ampere hours, whereas, as a matter of fact, it is about two and a half times that amount. The specific gravity of the solution, which Mr. Urquhart says should be 1·220 when the cell is fully charged, falls, he says, about 0·1 for every 5 ampere hours, no reference whatever being made to the size of the cell. The specific gravity, then, of the liquid of an accumulator from which 61 amperes could be taken, would fall to nought at the end of the first hour of discharge, though what that might mean we do not know. On p. 51 we are told in connection with the miner's lamp, that an accumulator weighing only 3 lbs. can "be made to light a small incandescent lamp for ten or twelve hours, yielding a light of two or three candles." Now 3 candles for 10 hours means about 120 watt hours, so that, if we assume that the box and liquid weigh together only 8 ounces, this marvellous accumulator stores something like 140,000 foot-pounds of energy per 1 pound of plate. On p. 294 the weight of the miner's accumulator and incandescent has gone up to 7 lbs., and the light has gone down to 1 or $1\frac{1}{2}$ candle.

This sort of looseness runs through the book, "The legal ohm is the resistance presented by a column of pure mercury, 106 centimetres in length and 1 millimetre in section," the word square before millimetre, and all reference to temperature being omitted. After the definition of the watt it is stated that "An incandescent lamp is said to need 4 watts per candle power, or 60 watts in all to run it;" "said to need" looks as if it were a definition instead of being an experimental fact, and since the candle power of the 60 watt lamp is not mentioned, it might appear that all incandescent lamps from 2 to 2000 candles power required 60 watts. In speaking of the number of lamps a dynamo can maintain glowing, the author says, "More lamps could be maintained at 5 watts per candle than at 4;" we should very much like to know why. The phase of an alternate current is defined as its life. Under "Cost of Electric Light" we are told, "The Board of Trade unit, consisting of a kilowatt (a thousand watts for one hour) is the recognized standard of calculation," and that this is not a printer's error is shown by the author going on to say, "that a kilowatt can be sold at a fair profit at from 7*d.* to 9*d.*" Perhaps the author will favour us with the market value of one mile an hour.

A large amount of useful information has been collected together, the illustrations are abundant and well executed, and probably much time has been spent in the compiling of this book. Is it not a pity then that its value, both for the technical reader and for the electrically-lighted householder, should be much diminished by the unscientific vagueness that runs through it, and by the indiscriminate mixture of the antique with the modern in its pages?

COUES'S "HAND-BOOK OF ORNITHOLOGY."

Hand-book of Field and General Ornithology: a Manual of the Structure and Classification of Birds. With Instructions for Collecting and Preserving Specimens. By Prof. Elliott Coues, M.A., M.D., &c. Pp. 344. (London: Macmillan and Co., 1890.)

NATURALISTS are not unfrequently regarded as belonging to two categories—those of "the field" and those of "the cabinet." The "field naturalist" is too often little acquainted with scientific method, and apt to undervalue scientific research. On the other hand, the "cabinet naturalist" in many cases despises the labour of his brother of "the field," and thinks that he can solve all the problems of life without studying the living organisms. The best naturalists—it is not necessary to quote names in support of such a truism—have always been those who combine much experience in the field with great study in the cabinet. The author of the present work is well known to possess both these qualifications, without which, indeed, he could hardly have ventured on the task of writing it. His experience in the field, as he tells us in his prefatory remarks, reaches in time over thirty years, and extends in area over large portions of North America. Having made personal acquaintance with most of the species of North American birds, and having shot and skinned with his own hands several thousand specimens, he may reasonably claim to speak with authority on field ornithology. On the other hand, Dr. Coues is the author of the "Key to North American Birds," which has passed through many editions, and is generally recognized as the standard text-book of the American ornithologist. On this branch of his subject, therefore, Dr. Coues is likewise entitled to claim our full attention.

Dr. Coues commences his hand-book with "Field Ornithology," which, as he truly says, should lead the way to systematism and description, and devotes nearly ninety pages to this part of his work. The necessary implements for collecting, the various instruments and materials required for making skins, the proper modes of registration and labelling, and the right way to keep a collection when made, are all discussed in turn, and admirably explained and illustrated. "Labelling," we are glad to see, Dr. Coues expatiates upon at full length, and it is impossible to exaggerate its importance. How often are the best prepared and rarest specimens of natural objects rendered comparatively useless by the neglect of this requirement! We do not presume to say that all the twelve particulars insisted upon by our author should be given in every case, but the locality, the date, and the collector's name should at least never be omitted from the label of a scientific specimen.

A still more important part of Dr. Coues's hand-book is that of "General Ornithology," which occupies the remainder of the present volume. It is divided into four sections. In the first of these the author endeavours to define exactly what a bird is, and discusses the position of the class "Aves" in the series of Vertebrata. In the second section the principles of classification are reviewed, and it is shown that morphology or bodily structure is the only safe guide to a natural system. The third section is devoted to a description of the external characters of

birds, and the fourth to the internal characters, or, as they are generally called, the anatomy of birds. These two essays form in fact the most important part of the volume, and occupy more than half its pages. Both of them are well drawn up, the various characters are described in plain and simple language, and the structures are illustrated by a large number of woodcuts introduced into the text. That Dr. Coues's statements are absolutely free from error we by no means affirm. Zoological science is progressing rapidly nowadays, and since these essays were written, five or six years ago, discoveries have been made that should have caused a modification of some of them as they now stand. But Dr. Coues is generally well up to the level of modern science, and seems to be acquainted with most recent views of experts on most points. On the whole, we know of no volume likely to be more useful to the student who wishes to become acquainted with birds, alike in the field and in the cabinet, than Dr. Coues's "Hand-book," and we are of opinion that the publishers have done a good deed in reprinting it for the use of British ornithologists. No other manual that we are acquainted with exactly takes its place, or contains such a well-arranged mass of useful and generally correct information on this subject.

OUR BOOK SHELF.

Swanage: its History, Resources, &c. (London: William Henry Everett and Son, 1890.)

SEASIDE guide-books are generally the production of some local tradesman, but the rising town of Swanage has issued one by no less than eight authors and an editor. Nothing but exceptional care could knit such a work into harmony, yet of editing there is no trace but the name. Though a full chapter by such an authority as Horace B. Woodward is devoted to geology, its interest is allowed to be forestalled earlier in the book by writers who are in apparent ignorance of the coming chapter, and who make no reference to it.

The book contains no itineraries, no suggestions as to how and in what time places of interest can best be reached; no guide as to hotels and lodgings, or tariffs for carriages and boats; no hints as to sea and river fishing; nothing of the birds; not a word on marine zoology. In place of these there are an introduction and conclusion, presumably editorial, worthy of a tenth-rate society paper, the latter containing a table of distances by a literary scaramouch. In this extraordinarily facetious table, Cowes is given as distant $27\frac{1}{2}$ miles by water and $6\frac{1}{2}$ by *land*; the Needles are $19\frac{1}{2}$ miles nearer by *land* than Bournemouth; Parkstone and Poole, though well-nigh suburbs of Bournemouth, are no less than 24 miles nearer by land to Swanage; Bournemouth itself is said to be 34 miles distant, while everyone knows it is only 25 miles by rail; Southampton is actually less distant than Christchurch, and so on. With such editing we are not surprised to find the same place figuring as Branksea in the letterpress and Brownsea on the map.

It is impossible seriously to criticize the anonymous portions of such a book, except to say that the archæological information is evidently by an accomplished antiquary. It is a pity that his solid contributions are interwoven with adulatory remarks, perhaps by the editorial gentleman, which must be distasteful to Mr. George Burt, who, owning some 150 to 200 acres of the best building land "already laid out and ready for erecting residences," no doubt finds his account in what he does.

Of the specialist chapters, that relating to hygiene, by Dr. L. Forbes Winslow, is the longest, and we should have thought 27 pages more than ample to tell us that, being almost on a promontory on the south coast, and well sheltered from the north-east by a high range of downs, the climate of Swanage is mild, equable, and bracing, and with good water and drainage should be particularly healthy. Visitors should be warned, on the other hand, that the air is strong, and that the Purbeck Hill at the back is bleak and bare of trees, and being riddled with stone quarries presents a forbidding aspect.

The chapter on geology is of course excellent, and, had it been illustrated with a few sections and figures of fossils, would be sure to induce visitors with time on their hands to take the subject up. We cannot think, however, that the Wealden has the enormous thickness of over a third of a mile so close to its western limit, and rather believe that the same beds occur over and over again in a series of truncated folds. The author, like others who know the section, does not endorse the views of Prof. Judd on the so-called Punfield Beds. The section deserves notice as the only British locality for a gigantic Paludina, and all the beds up to the chalk are fossiliferous, and deserve more careful investigation than they have received. On the other side of the massive chalk barrier, the Lower Bagshot beds, though only 70 feet thick at Alum Bay, occupy about half a mile of the shore at high angles, and are as obviously plicated as the Wealden. They are so entirely grassed over, except at Redend Point, that nothing can be known of them, but inland masses of Middle Bagshot are present in the folds. The beds are very fossiliferous in places, but the pipe-clays have had such a squeezing, that the leaves are miniature geological models of faults and slickensides, and readily fall to pieces. The vegetation is much more characteristic of swamp life than at Alum Bay, the prevailing fossils being a large fan palm, reeds, and a tropical Chrysodium massed together, and more rarely leaves of *Aralia primigenia*, *Quercus lonchitis*, Acer and Salix, and occasional shells of Unio and elytra of insects. The poverty of the flora is in contrast with the enormous wealth of that of the Bournemouth beds just across Poole Harbour.

Of the admirable and careful lists of plants by Mr. J. C. Mansel-Pleydell, and of insects by Messrs. Herbert Goss and Eustace Banks, we have nothing to say except to lament that the book is so unworthy of them. Of course there is no index, and the illustrations are commonplace process plates, in which Mr. Burt's house and his big refreshment-room on the hill, perhaps the future Casino, figure prominently. Really interesting bits like the tower of the old church, or romantic scenes like the Pinnacles or Old Harry, are omitted. J. S. G.

Graphic Lessons in Physical and Astronomical Geography. By Joseph H. Cowham, F.G.S. (London: Westminster School Book Depot, 1890.)

THE method of teaching adopted in this work justifies itself for the subjects with which it deals. The lessons have been prepared to cover certain courses of instruction, among them being the Standard Code, pupil teachers' course of geography during the four years of their training, scholarship examinations preparatory to entrance into a training college, and for the entire course of physical geography laid down in the certificate syllabus for first and second year's students in training colleges.

The main features of the work lie in its arrangement, the note-like style adopted in the great variety of simple sketches and blackboard illustrations which demonstrate well the innumerable points for which they are required. Each item of matter is surrounded by abundant information, and teaching hints in the form of notes are given here and there for the benefit of those using this book.

The end of each section contains a short summary of the preceding subject matter, and concludes with questions

for examination. The book will be sure to be well used, and we recommend it, and with the authors we hope "that it may stimulate others to make the teaching of physical geography a pleasant exercise for themselves and a valuable mental training for those whom they teach." W.

The Evolution of Photography. (Illustrated.) By John Werge. (London: Piper and Carter; John Werge, 1890.)

IN this work we have a most interesting account, arranged in chronological order, of the origin, progress, and development of the science and art of photography. The author has divided this time into four periods. The first deals broadly with facts bearing on the accidental discovery of photography, and on the early researches and ultimate success of the pioneers. The second embraces a fuller description of their successes and results, while the third is devoted to the consideration of patents and impediments, and the fourth to the final development of both photographic literature and art.

Although the author has not entered minutely into elaborate details of each process, yet he has given enough to form an interesting summary. Excellent illustrations of some of the chief photographic investigators, taken from paintings, daguerreotypes, &c., and reproduced by the callotype process, add greatly to the value of the book.

Following this there is a chronological record of inventions, discoveries, publications, and appliances connected with the development of photography, and the author concludes with the personal reminiscences, extending over a period of forty years.

This book will be an acceptable addition to our photographic literature, and will be found interesting not only by the practical photographer, but by many amateurs. W.

Geometrical Drawing for Art Students. By I. H. Morris. (London: Longmans, Green, and Co., 1890.)

ART students will be glad to find in this work a compendium of those parts of geometry which cover the necessary range for their course. Plane geometry and its applications, the use of scales, and the plans and elevations of solids, are treated concisely, and the method adopted throughout of placing the text on the left-hand pages, leaving the right-hand pages solely for figures, will be found most convenient. The figures are all neat and well drawn, those illustrating the problems on solid geometry being especially so.

The chapter on the construction and use of plain and diagonal scales and scales of chords, subjects which are generally stumbling-blocks to a great many students, is made very clear, and in chapter xv. good ideas are imparted in the applications of geometry to the construction of patterns and simple tracing.

Nearly six hundred figures are inserted in the book, together with a complete and exhaustive collection of exercises. Students interested in this subject other than those for whom the work is intended will find the arrangement adopted more convenient than in many other books on the subject. W.

An Elementary Text-book of Dynamics and Hydrostatics. By R. H. Pinkerton, B.A. Oxon. Second Edition. (London: Blackie and Son, Limited, 1890.)

WE are glad to see the appearance of a second edition of this serviceable little text-book, the first edition of which we reviewed some time ago. No material alteration has been made in any part of the work. The appendix has been extended by the introduction of the method of co-ordinates, the discussion of simple harmonic motion and its application to the pendulum, and the method of finding moments of inertia.

Several new examples have been fully worked out for the purposes of illustrating the methods of solving problems graphically, and some additional examination papers have been given.

At the beginning of the book tables of relative density and of the English and French measures will be found, and the work concludes with a newly added index. We can only repeat what we said formerly, that this is a book to be thoroughly recommended. W.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Pilcomayo Expedition.

IN view of the notice in the *Times* this week of the collapse of the Pilcomayo Expedition, the inclosed extract from a home letter of Mr. J. Graham Kerr, Naturalist with the Expedition, may be of sufficient interest for publication in NATURE. The letter was received in Scotland on September 2. It contains no mention of Captain Page, and must have been written before his death as recorded in the *Times*. The letter bears out the *Times* account of the difficulties encountered by the Expedition, and that the Pilcomayo is not likely to become a trade water-way; but it does not foreshadow disaster such as the *Times* account suggests, and it gives hope that the other members of the Expedition may not have shared the fate of Captain Page. ISAAC BAYLEY BALFOUR.

September 27.

"*s.s. 'Bolivia,' Rio Pilcomayo, lat.* 24° 25′ *S., long.* 58° 40′ *W.*,
"*Tuesday, June* 3, 1890.

"We entered the Pilcomayo on March 12, therefore we have been 3 months on the river. We have managed to penetrate about 300 miles by river in that time, but owing to the extraordinary tortuosity of the Pilcomayo, our distance in a straight line from Asuncion I don't suppose is more than, if it reaches, 100 miles. The river is very disappointing from the points of view of æsthetics, botany, zoology, geology, and anthropology. As regards the first, the scenery in the lower reaches is certainly beautiful, but of a type of beauty which soon palls upon one, and becomes intensely monotonous. The scenery is very much that of a sluggish flowing river at home. When we first entered the river, I was amazed at its small size—only about 50 yards in width. Up here it seldom reaches 20 yards, and is frequently not more than 10, and there is scarcely any water in it at all. For the last two months we have got forward not more than 10 leagues, at the very outside, and what little we have done has been by building dams, letting the water accumulate, and so getting forward for a short distance, when another dam was built, and so on. The larger steamer, the *General Paz*, we had to leave far down the river. The military detachment whom we had left a few miles down was discovered the other day to have flown, their provisions, no doubt, having run short. We brought a corporal and two men on with us. The other day, however, one of these deserted, and has, no doubt, either gone over to the Indians or been killed by them. To return, however, to the scenery. Here, and for a long distance down, we have had a type of scenery which is to be found in very few parts of the world—that of an immense palm forest, covering thousands of square miles. It consists typically of a perfectly level plain clothed with breast-high grass, over which are closely studded palm-trees with large fan-shaped leaves; all around, far as the eye can reach, an interminable vista of palm-trees, varied only by an occasional clump of brushwood, or near the river by a small patch of forest. In no way is the aspect of nature suggestive of the tropics here, *i.e.* when one has got over the first impression induced by the palm-trees. The Gran Chaco is in fact an immense wilderness. Large game occurs only in small numbers. I have managed to get only a couple of peccaries, and no one else has shot any large game. I have not even got a tiger yet, and have only once had anything approaching an adventure with one. Other adventures we have had absolutely none. Intense monotony and uninterestingness are the chief characteristics of the river. Botanically speaking, it is

an absolute desert. In an ordinary summer's afternoon walk at home one sees more species of plants in flower than I have seen since we entered the river. . . . However, this may improve, as it is now the dead of winter here, and with the advent of spring I hope to see many new and interesting flowers appear. Zoologically, too, it is disappointing, except in the case of birds. In the lower parts of the river not a bird was to be seen, but now they are rather more frequent, and I have already observed 116 species, of which I believe about 30 have not before been collected in Argentina. Owing to the desert nature of this part of the Chaco, its human inhabitants are very few, scattered, and nomadic. We have not seen a single Indian or canoe on the Pilcomayo. But we know they are about, for nearly every day we see their great fires for hunting all around us, and we occasionally come across a chipped palm, or the remains of an old *tolda*, the rude shelter which serves them as a tent; now and again, too, we see a human footprint, sometimes of immense size, impressed upon the muddy margin of a lagoon. So we are always on the alert, the four Britons of the Expedition keeping watch at night, fully armed and wide awake. The four said Britons are Poole, Kenyon (English), Henderson the chief engineer, and myself. When I go away collecting also, as I do every day, I always go with loaded revolver and knife—ready for emergencies. For in addition to Indians there are abundance of tigers about, which one has to be prepared for. Yesterday we got an alligator close to the boat, 8 feet long. The alligators here are all small, 8 feet being the largest we have seen. . . .

"As regards food we are on very short rations, being within a month or so of the end of our provisions. The canoe is to be sent down soon, I believe, to hurry up the fresh supplies of provisions, and by it I shall send this letter, although it is very doubtful whether you will ever get it. The health of the men is not good; we have always two or three of the 17 on board ill. I have, however, had excellent health. The only thing disagreeable is the fearful cold. In the mornings the thermometer is often nearly at freezing-point, and I feel quite benumbed. Fortunately, it generally gets a little warmer during the day, the temperature rising in the afternoon to between 70° and 90° F. The river-water is regular brine here, quite as salt as sea-water, and when occasionally we run out of fresh water for a few days, it is very disagreeable having to take coffee, &c., made with the salt water. Of fruits here, there are none worth eating. The young parts of the palm-trees are eatable, and we use a good deal of it in order to economize the rice, &c. I don't expect at all that we can possibly reach Bolivia, and I don't think the river could ever be made navigable."

Protective Colours.

Mr. Poulton, in his book entitled "The Colours of Animals," seeking a reason for the glistening metallic colours of many chrysalides, after showing that the colour is probably protective in its origin, states "that it has arisen from the protective resemblance to rough dark surfaces of rocks."

He comes to this conclusion after failing to find other more probable examples of glistening bodies in nature.

Are such not, however, very common (i.) in the slime or mucous covering many of the Invertebrata, and which snails and slugs leave on all surfaces over which they have passed; (ii.) the webs of spiders and their allies, especially if moist; (iii.) the exudation or excretion of many plants; (iv.) decomposing bodies; (v.) the bark of many trees?

Perhaps the commonest places to find glistening chrysalides are on palings, tree trunks, and various plants; all of which structures are usually resplendent with one or more of the above metallic hues, and among which the chrysalides are very hard to find.

May not these more common objects be those of attempted resemblance, rather than the less frequent pieces of broken rock?

Grosvenor Club, Bond Street, W. Walter K. Sibley.

Mr. Sibley's letter appears to me to contain valuable suggestions as to the meaning of the metallic appearance of certain chrysalides. It is probable that a resemblance to the objects he suggests does aid in concealing the pupæ. Mr. Roland Trimen has similarly concluded that certain brilliant beetles (*Cassididæ*) are protected by resembling drops of dew. At the same time I think that there is some evidence that the metallic appearance of the pupæ of *Vanessidæ* may have been originally acquired in order to favour concealment against glittering mineral surfaces. The evidence is as follows:—(1) In shape and character of the surface these pupæ strongly resemble a rough and broken piece of rock. (2) They appear in two forms, resembling grey and weathered as well as freshly exposed and glittering rock surfaces. (3) When they seek green leaves for pupation they either conceal themselves with the greatest care (*V. atalanta*), or a glittering variety of other species is represented by a green variety which is inconspicuous against the leaves (*V. Io*). (4) Another species (*V. urticæ*), which lacks the habit of *V. atalanta* and the green variety of *V. Io*, is, as far as my experience goes, very rarely found on the leaves of its food-plant, and when so found, is, as a rule, diseased.

I mention the chief lines of evidence upon which I have relied in order to show that it was not merely the failure "to find other more probable examples of glittering bodies in nature" which led me to adopt the view alluded to by Mr. Sibley. Although I still consider that my hypothesis is probable, at any rate for the *Vanessidæ*, I am convinced that the resemblance to other glittering objects, such as those mentioned by Mr. Sibley, has favoured the development and especially the persistence of the metallic appearance. E. B. Poulton.

September 19.

The Aryan Cradle-land.

"It will be for the benefit of our science," said the President of the Anthropological Section of the British Association, "that speculations as to the origin and home of the Aryan family should be rife; but it will still more conduce to our eventual knowledge of this most interesting question if it be consistently borne in mind that they are but speculations." With the latter, no less than with the former opinion, I cordially agree. And as, in my address on the Aryan cradle-land, in the Anthropological Section, I stated a greater variety of grounds in support of the hypothesis of origin in the Russian steppes than has been elsewhere set forth, I trust that I may be allowed briefly to formulate these reasons, and submit them to discussion.

(1) The Aryans, on our first historical knowledge of them, are in two widely separated centres, Transoxiana and Thrace. To Transoxiana as a secondary centre of dispersion the Eastern Aryans, and to Thrace as a secondary centre of dispersion the Western Aryans, can, with more or less clear evidence, or probable inference, be traced, from about the fourteenth or perhaps fifteenth century B.C.; and the mid-region north-west of Transoxiana and north-east of Thrace—and which may be more definitely described as lying between the Caspian and the Euxine, the Ural and the Dnieper, and extending from the forty-fifth to the fiftieth parallel of latitude—suggests itself as a probable primary centre of origin and dispersion.

(2) For the second set of facts to be considered reveal earlier white races from which, if the Aryans originated in this region, they might naturally have descended as a hybrid variety. Such are the facts which connect the Finns of the north, the Khirgiz and Turkomans of the east, and the Alarodians of the south, with that non-Semitic and non-Aryan white stock which have been called by some Allophyllian, but which, borrowing a term recently introduced into geology, may, I think, be preferably termed Archaian; and the facts which make it probable that these white races have from time immemorial met and mingled in the South Russian steppes. Nor, in this connection, must the facts be neglected which make great environmental changes probable in this region at a period possibly synchronous with that of Aryan origins.

(3) In the physical conditions of the steppes characterizing the region above defined, there were, and indeed are to this day, as has been especially shown by Dr. Schrader, the conditions necessary for such pastoral tribes as their language shows that the Aryans primitively were; while, in the regions between the Dnieper and the Carpathians, and between the Oxus and the Himalayas, the Aryans would, both in their south-western and south-eastern migrations, be at once compelled and invited, by the physical conditions encountered, to pass at least partially from the pastoral into the agricultural stage.

(4) The Aryan languages present such indications of hybridity as would correspond with such racial intermixture as that supposed; and in the contemporary language of the Finnic groups Prof. de Lacouperie thinks that we may detect survivals of a former language presenting affinities with the general characteristics of Aryan speech.

(5) A fifth set of verifying facts are such links of relationship between the various Aryan languages as geographically spoken in historical times, such links of relationship as appear to postulate a common speech in that very area above indicated, and where an ancient Aryan language still survives along with primitive Aryan customs. For such a common speech would have one class of differentiations on the Asiatic, and another on the European side, caused by the diverse linguistic reactions of conquered non-Aryan tribes on primitive Aryan speech, or the dialects of it already developed in those great river-partitioned plains.

(6) A further set of verifying facts are to be found in those which lead us more and more to a theory of the derivative origin of the classic civilizations, both of the Western and of the Eastern Aryans. Just as, between the Dnieper and the Carpathians, and between the Oxus and the Himalayas, there were such conditions as must have both compelled and invited to pass from the pastoral into a partially agricultural stage; so, in passing southward from each of these regions, the Aryans would come into contact with conditions at once compelling and inviting to pass into a yet higher stage of civilization. And in support of this all the facts may be adduced which are more and more compelling scholars to acknowledge that in pre-existing Oriental civilizations the sources are to be found, not only of the Hellenic and the Italic, but of the Iranian and the Indian civilizations.

(7) Finally, if the Hellenic civilization and mythology is thus to be mainly derived from a pre-existing Oriental or "Pelasgian" civilization, it is either from such pre-existing civilizations, or from Aryans such as the Kelto-Italiots, migrating northward and southward from Pelasgian Thrace, that the civilization of Western and Northern Europe would, on this hypothesis, be traced: and a vast number of facts appear to make it more probable that the earlier civilization of Northern Europe was derived from the south than that the earlier civilization of Southern Europe was derived from the north.

The three conditions of a true solution of the problem either of Semitic or of Aryan origins appear to be these. First, the locality must be one in which such a new race could have ethnologically, and secondly philologically, arisen as a variety of the Archaian stock of white races; and thirdly, it must be such as to make easily possible the historical facts of dispersion and early civilization. And I venture to submit the above sets of facts as not inadequately, perhaps, supporting the South Russian "speculation as to the origin and home of the Aryan family."

J. S. STUART GLENNIE.

The Shealing, Wimbledon Common, September 22.

Mr. Dixon's Mode of Observing the Phenomena of Earthquakes.

MR. HAROLD DIXON'S letter in NATURE of Sept. 18 (p. 491) is exceedingly interesting to seismologists. On two occasions he was able to make the only kind of observation which is of any value unless seismographs are actually employed; he has been able to make these in England, where earthquakes are rare, and I know of no record of such definite information being given by any of the trained observers in Japan, where earthquakes are so numerous. It requires great coolness to make such observations at such a time.

Seismographic records show that even in destructive Japanese earthquakes the vertical displacement of the ground is usually less than one millimetre, so that the mere *difference* in vertical displacement observed by Mr. Dixon between two points in the same room must have approached five hundred times the greatest *absolute* vertical displacement in Japan. Mr. Dixon truly says that, if the displacement observed by him had been due to the movement of the hill itself, it must have meant a good deal, for it would have meant some hundreds of thousands of times the greatest vertical earth movement recorded by any seismograph.

When I say that Mr. Dixon's letter is interesting, I make the assumption that what he observed was not merely what anybody observes who raises his head when looking at a distant hill through a window.

JOHN PERRY.

31 Brunswick Square, W.C., September 24.

Butterflies Bathing.

IN NATURE of August 21 (p. 402) is a note taken from the *Victorian Naturalist* describing an observation made by Mr. G. Lyell, Jun., of Melbourne. He states that several butterflies (*Papilio macleayanus*) were seen to enter the water backwards, remain partly submerged for about half a minute, and then fly off to the hill-sides refreshed with their bath. The heat of the weather is given as the cause of their action.

I should like to suggest that the insects were probably engaged in *depositing their eggs*. Perhaps some one who has the opportunity will ascertain if the larva of this butterfly is aquatic, or feeds on plants growing at the water's edge.

G. A. FREEMAN.

St. Olave's Grammar School, Southwark.

Surface-tension and Surface-viscosity.

IF an oiled needle be placed on the surface of pure water, it will be supported, but if it be washed in a solution of potash, it will sink. In the first case the effect cannot be due to the surface-tension, which is much diminished by the oil. Has the viscosity of the oil anything to do with it? Also in the case of a soap-bubble, is the effect due to viscosity, and not to surface-tension; and what is the difference between surface-tension and surface-viscosity? They are both, no doubt, due to cohesion, but it is difficult to form definite conceptions of the two properties. Would any of your readers kindly answer the above questions, and give references to any works bearing thereon? Maxwell's "Theory of Heat," on capillarity and viscosity, does not seem to throw any light on the matter.

W. P. O.

Leicester, September 25.

ON STELLAR VARIABILITY.[1]

II.

I HAVE before stated that the variability phenomena observed in stars of the Groups I. and II. and VI. are produced by the same cause; all differences in the details of the effects being due to the different physical nature of the central body. In Groups I. and II. it is a swarm of meteorites with which we have to deal; in Group VI. it is a condensed star of low radiation surrounded by a dense atmosphere containing carbon in some combination.

In both cases the bodies are normally dim; in Groups I. and II. they are so because the meteorites when undisturbed are relatively free from collisions; in Group VI. they are so for the reason stated above, the star being on the verge of extinction.

I insist upon this dimness, because the dimmer the central body the more important becomes the luminosity caused by, or set up in, secondary swarms. Further, such variability as we are now considering is unknown in the case of the hotter stars.

It is clear that phenomena produced in either group *by the action of two swarms* should strongly resemble each other, and that if it be found that this explanation holds good in one case it should be found to hold equally good in the other. It is to be expected then that phenomena observed in each may throw light upon the other, and that the view advanced may be tested by the differences observed.

Let us consider two hypothetical cases, to start with, in Groups I. and VI.

In Group I. we have a condensing nebula the light of which when undisturbed is say 6 mag. Round this there revolves a cometary swarm say in six time units. At periastron collisions occur which raise the light of the combined swarms to 3 mag. There is also another similar swarm revolving in say twelve time units. The conditions are such that this second swarm produces a smaller disturbance which only raises the light to $4\frac{1}{2}$ mag. We will assume the periods to be exactly commensurable, and the apastra to occur together. It is obvious that alternate minima will be raised by this second revolving swarm, *but the maxima will be constant.*

In order to put results of this nature into diagrammatic form we must consider that we are dealing with certain

[1] Continued from p. 419.

additions of light to the constant light of the star. These additions must be shown as such.

It is very important that I should point out that for this method of direct integration to be adopted a scale of light units must be employed, *for the reason that the amount of light which is sufficient to produce a change of a magnitude in a faint star would only produce a change of a fraction of a magnitude in a brighter star.*

Taking the light of a star of magnitude m as a unit, and using the formula

$$L_{m-n} = (2{\cdot}512)^n L_m,$$

in which L_m represents the light of a star of magnitude m and L_{m-n} the light of a star n magnitudes brighter, we get—

$$\begin{aligned} L_{m-1} &= 2{\cdot}51\ L_m \\ L_{m-2} &= 6{\cdot}31\ L_m \\ L_{m-3} &= 15{\cdot}85\ L_m \\ L_{m-4} &= 39{\cdot}78\ L_m \\ L_{m-5} &= 100{\cdot}02\ L_m \end{aligned}$$

The amount of light to be added for the different magnitudes will therefore be as follows :—

$$\begin{aligned} &\text{Additions for one magnitude} = (\ 2{\cdot}51 - 1\ )L_m = 1{\cdot}51\ L_m \\ &\quad ,,\quad ,,\ \text{next}\quad ,, \quad = (\ 6{\cdot}31 - 2{\cdot}51)L_m = 3{\cdot}80\ L_m \\ &\quad ,,\quad ,,\quad ,,\quad ,, \quad = (\ 15{\cdot}85 - 6{\cdot}31)L_m = 9{\cdot}54\ L_m \\ &\quad ,,\quad ,,\quad ,,\quad ,, \quad = (\ 39{\cdot}78 - 15{\cdot}85)L_m = 23{\cdot}93\ L_m \\ &\quad ,,\quad ,,\quad ,,\quad ,, \quad = (100{\cdot}02 - 39{\cdot}78)L_m = 60{\cdot}24\ L_m \end{aligned}$$

It is obvious that these differences are in exactly the same proportion to each other as the numbers representing the light of the stars of different magnitudes, and if in our diagrams we take a certain length of line to represent the added light equivalent to one magnitude, about $2\frac{1}{2}$ times this will represent the added light for the next magnitude, and each succeeding magnitude will be represented by a line $2\frac{1}{2}$ times as long as the preceding one. A scale of this kind must be adopted in integrating the effects of two sources of added light, for the reason already stated.

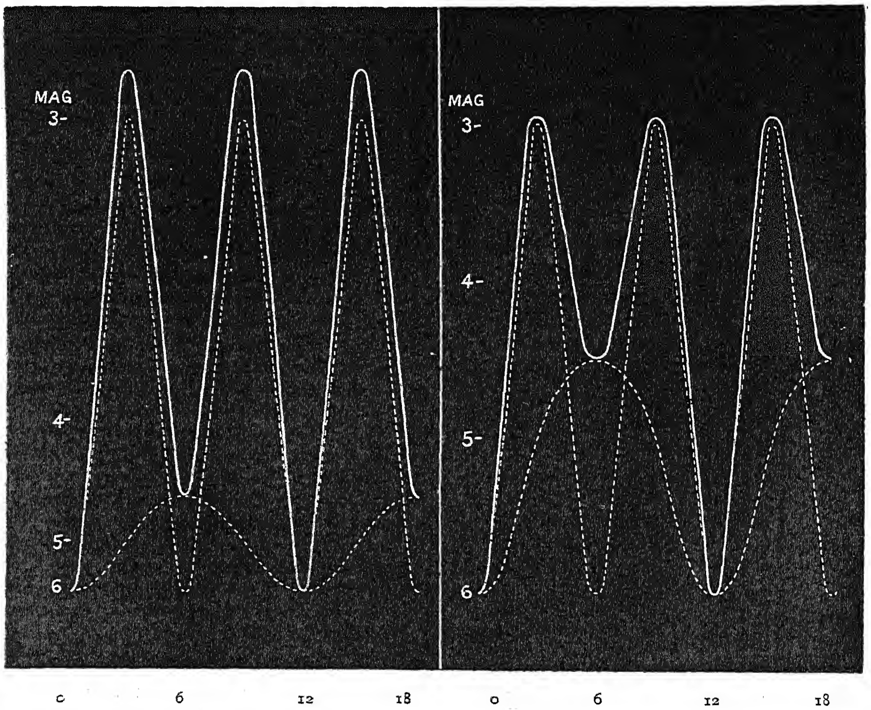


FIG. 1.—Hypothetical curve in light-units.

FIG. 2.—Hypothetical curve in magnitudes.

Thus while the amount of light to be added to a sixth magnitude star, to take an instance, to increase it to the fifth is $1\frac{1}{2}$ units, the number of the same units to be added to a fourth magnitude star to make it a third is $9\frac{1}{2}$. Hence the $1\frac{1}{2}$ units which raise a star of the 6th to the 5th magnitude—that is, one whole magnitude—would only increase a fourth magnitude star by about one-sixth of a magnitude.

To graphically represent what happens when by cometary action a star is raised three magnitudes above magnitude m, we get, in the above light-units—

1·512 additions for one magnitude
3·80 ,, ,, the next magnitude
9·54 ,, ,, ,, ,,

The sum of these numbers = 14·85, represents the *added* light.

The plan on which the following curves have been drawn will be gathered from the table given below, which shows how on the above basis the light-units and magnitudes correspond :—

Magnitude step.	Light addition for: Integral part of step.	Light addition for: Fractional part of step.	Total light addition.
½	0	0·58	0·58
1	1·51	0	1·51
1½	1·51	1·46	2·97
2	5·31	0	5·31
2½	5·31	3·66	8·97
3	14·85	0	14·85
3½	14·85	8·61	23·46
4	38·78	0	38·78
4½	38·78	23·08	61·86
5	100·02	0	100·02
&c.	&c.	&c.	&c.

In the hypothetical case represented in Fig. 1 the constant light of the central swarm may be taken as 6 mag., and the added light of the two secondary swarms as varying from nil to 3 mag. and from nil to $4\frac{1}{2}$ mag. respectively. It is then obvious that the integrated effects of the light added produce constant maxima of 14·85 units, and minima alternately 0 and 2·97. We can in this way represent the light-curve of a star which changes its magnitude from 3 to $4\frac{1}{2}$ and 3 to 6 alternately.

The relative scales of light-units to brightnesses shown

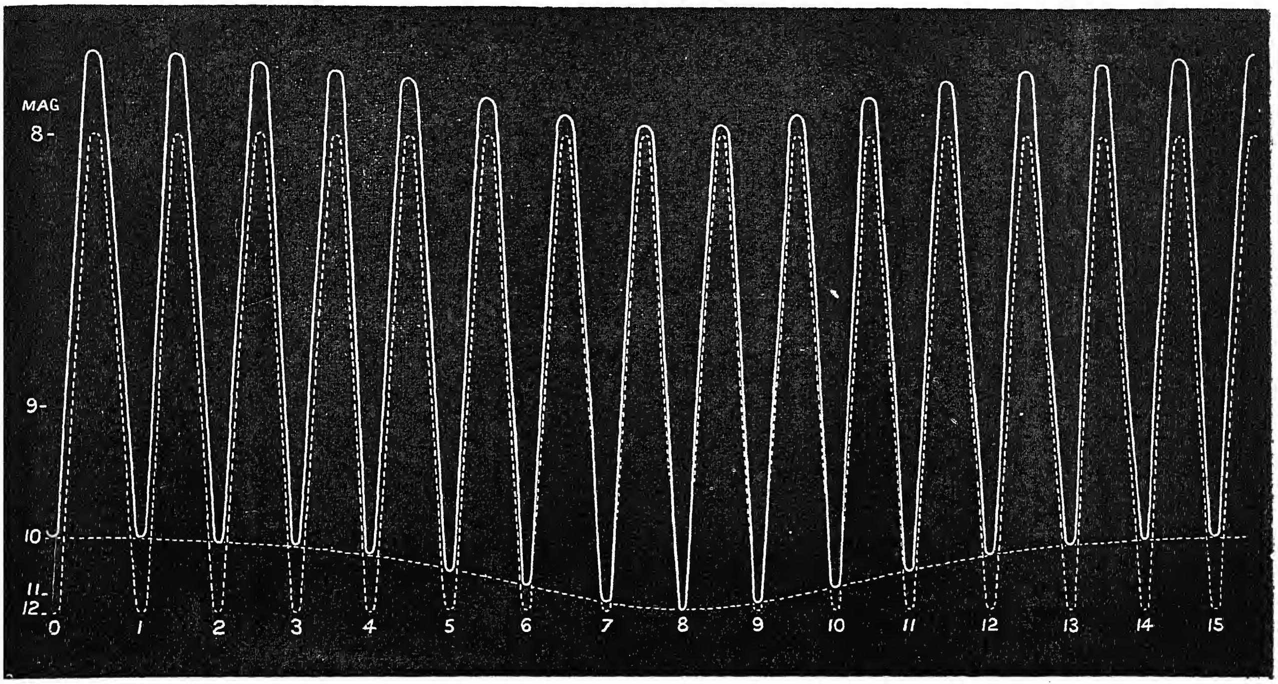


FIG. 3.—Hypothetical curve in light-units.

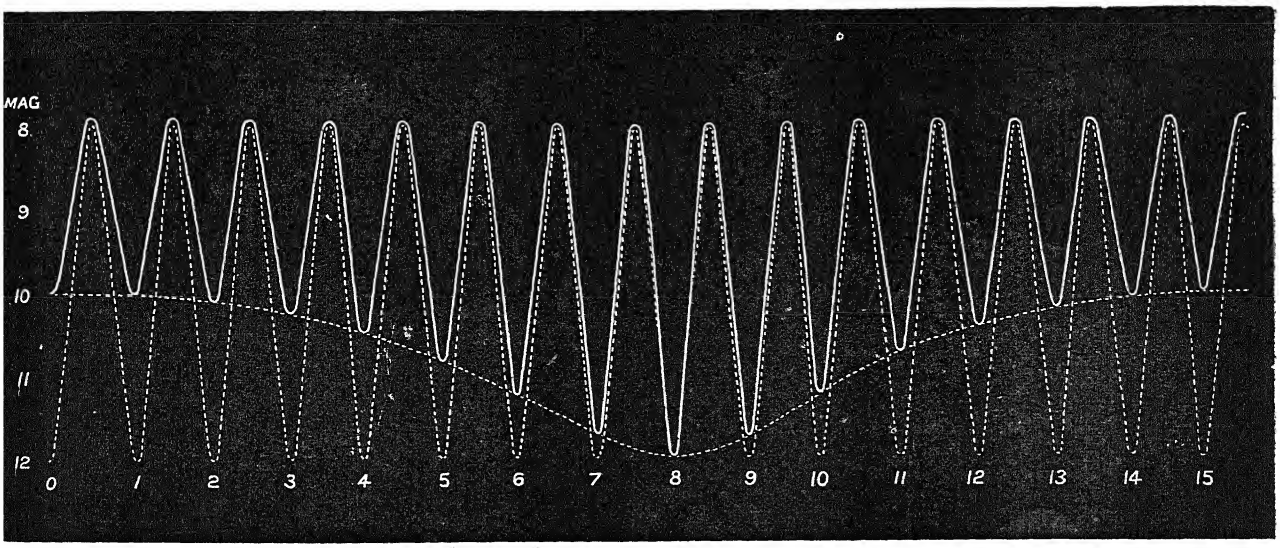


FIG. 4.—Hypothetical curve in magnitudes.

by the foregoing figures, however, enable us to transpose the diagram to one in which equal spaces represent equal differences of magnitudes. This is shown in Fig. 2.

In the diagrams, the light-curves of the two subsidiary swarms are represented by dotted lines, and the integrated result by the continuous line. One of the revolving swarms has a period of 6 units of time, and the other a period twice as long. The eccentricity of the primary swarm is such that it adds, at maximum, 14·85 light-units, while the secondary swarm adds 2·97 light-units.

A comparison of the two diagrams will make clear what

has already been said about the relative value of the light of one magnitude at the top and bottom of the curve.

We next take a hypothetical case from Group VI.

Here, instead of a nebula, dim owing to absence of collisions brought about by disturbances, we have to deal with a condensed body of small luminosity, the light of which is strongly absorbed by a carbon atmosphere.

We first consider the action of two subsidiary swarms, one producing more light with a short period, the other less light with a period say fifteen times longer. In fact we have one comet with an orbit of great eccentricity and short period, and another of small eccentricity and long period. We will assume the periastra to be coincident.

As the light is generally feeble, we may take the constant luminosity of the star as of the twelfth magnitude, and that it is raised to the eighth magnitude by the added light of the swarms at perihelion. We have then a difference of four magnitudes.

Proceeding as before we have :—

1·51 addition for one magnitude
3·80 „ the next
9·54 „ „
23·96 „ „

The sum of the added light gives us 38·81 of the light-units adopted = $(2·512)^4 - 1$.

The continuous curve represents in Fig. 3 the integrated effects expressed in light-units of the two added light sources, and it will be seen that the result is a variable with both maxima and minima also periodically variable. But although both maxima and minima are variable by an equal number of light-units, the effect on magnitude is totally different. Whereas the minimum varies by two magnitudes, the maximum only varies by about one-tenth of a magnitude.

In the hypothetical case represented, the maximum varies between 7·8 and 7·9, whilst the minimum varies between 10·0 and 12.

Like the curve for the variable of Group II., this may also be transferred to one in which equal differences of magnitudes are represented by equal spaces.

This is shown in Fig. 4, and here again it will be seen that, as in the former case, in adding a change of magnitude at the bottom of the curve to the top of the curve the magnitude-change is diminished according to the ratio of light-units.

The question now arises, Are there any stars in the heavens the phenomena of which can be represented by the hypothetical curves which we have just given? If so, we shall be justified in tracing a *vera causa* in the hypothesis under consideration. It may be here stated that one of the received explanations of such a variability as is represented on our first hypothetical diagram is that due to Prof. Pickering, who conceived that the observed effect might be produced by a surface of revolution; the ratio of the axes being 5 : 3, with a dark portion at one of the ends and symmetrically situated as regards the longer axis.[1]

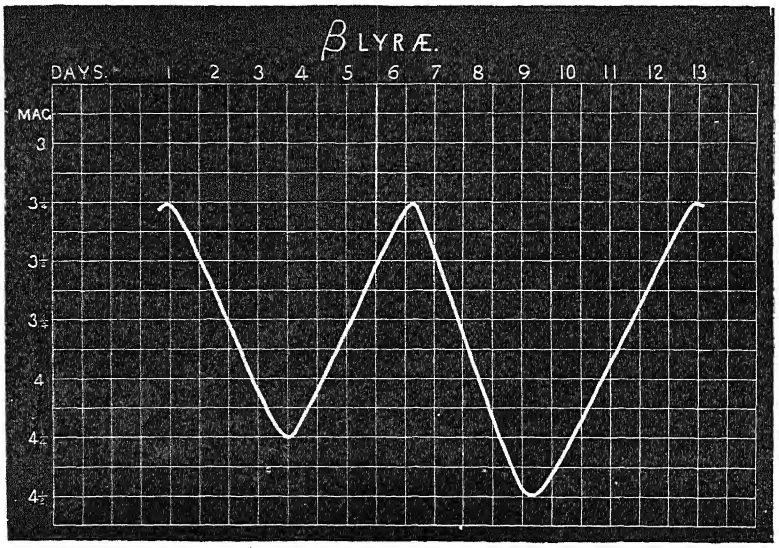


Fig. 5.—Light-curve of β Lyræ.

A reference to Fig. 5 will show that the hypothetical curve shown in Fig. 2 strikingly represents the actual light-curve of β Lyræ (actual magnitudes are not in question), and I submit therefore that the well-known phenomena of that star are produced by the causes I have suggested rather than by the complicated apparatus suggested by Prof. Pickering, to say nothing of the earlier suggestions of Maupertius and others.

I append another diagram (Fig. 6) to show that the second hypothetical curve is a close approximation to the light-curve of U Cygni, one of the best observed variables in Group VI.; and here I must express my obligations to Mr. Knott, who has freely communicated all his observations of this star to me, and has permitted me to publish them in this form.

Unfortunately, though the observations are of such a high order of exactness, they are not continuous. The parts of the curve in which the line is continuous represent the actual observations. The dotted lines added are for the purpose of enabling a comparison to be made with Figs. 3 and 4, in which the probable relations of the periods and intensities of the two hypothetic swarms are shown in light-units and magnitudes respectively.

The similarity between the hypothetic case represented and Mr. Knott's actual observations greatly strengthens my view.

It follows very clearly from the above considerations that on my hypothesis there should be frequently found rhythmical variations at the minimum, while the change at maximum is so slight that our best observers fail to notice it.

[1] Gore in "Astronomy for Amateurs," p. 238.

The smaller the range, the more will both maxima and minima be affected by the subsidiary swarms. W Cygni is a case in point.

It has been before remarked that the hypothesis demands that in sparse swarms of meteorites (Groups I. and II.) the ascent to maximum, due to the sudden action of the colliding swarms, should be much more rapid than the descent to minimum, for the reason that the descent must represent a *gradual* cooling down of the disturbance. This more rapid ascent has been noted in

R Piscium	Known Group II. stars.
S Vulpeculæ ...	
R Leonis Minoris	
R Ursæ Majoris	
R Corvi	
W Cygni	
S Cassiopeiæ ...	Group not yet determined.
R Arietis	
R Orionis	
T Delphini ...	
T Vulpeculæ ...	

I have also suggested that the short minimum is a measure of the indirect disturbance, but it is easy to imagine that this short minimum will not be invariable under all conditions, and accordingly we find in R Persei with a period of 212 days, a long minimum.

In stars of Group VI., on the other hand, where we have simply to deal with the added light of comets passing perihelion, there is no reason why this should happen, indeed, it ought to be rather the other way, since comets put on their greatest brilliancy after perihelion. As a matter of fact, so far as my inquiries have gone, I have not yet come across a case of a Group VI. star showing any great difference in the times spent in rising and falling.

On the hypothesis a *perfectly* constant period can only occur in the case of those double swarms in which the central one has a regular figure and density. The moment this condition is departed from, seeing that the central swarm is certain to be in rotation, variation of period as well as of maximum must be expected.

Nor is this the only variation which depends upon the central body. In the absence of knowledge in each case, we must assume that the structure of the central swarm resembles that of those which have been examined in Andromeda, Ursa Major, and Canes Venatici—that is, *the meteoritic density will vary locally* (S Aquilæ), and

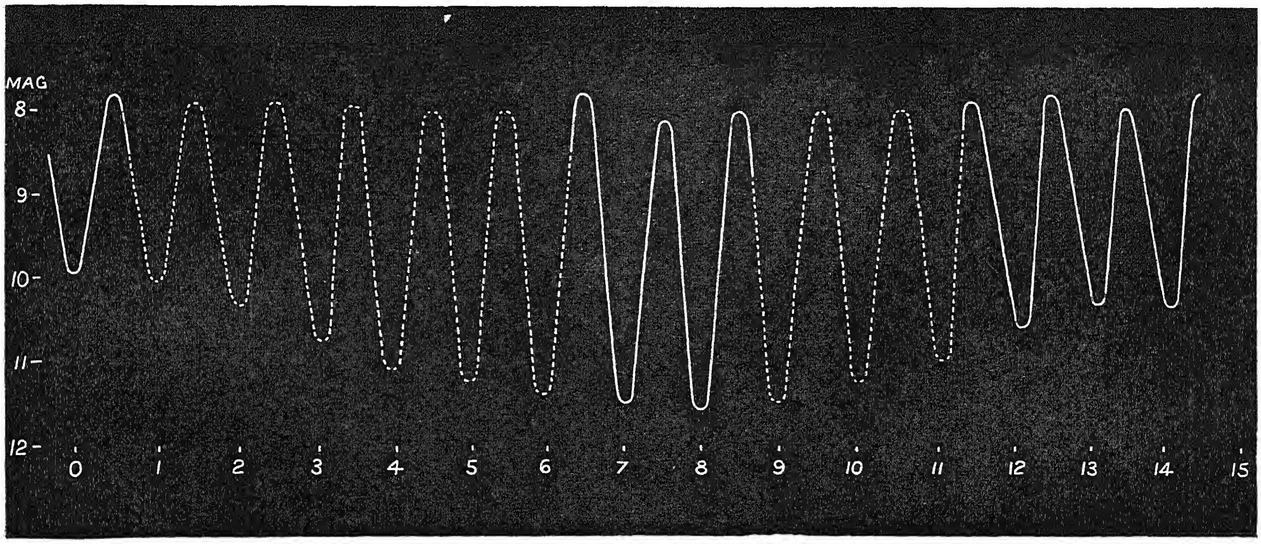


FIG. 6.—Light-curve of U Cygni, showing Mr. Knott's actual observations.

some of the observations made may be explained on the supposition that the subsidiary swarm breaks into regions in which the density is suddenly increased, as if we were noting the result of a ring being pierced (R Aurigæ).

We have only to look at Mr. Roberts's photograph of the nebula in Andromeda, and consider under what different conditions a secondary swarm might reach the same periastric distance if there were any rotation in the nebula or any movement of the nodes, to recognize the importance of taking the above points into consideration.

If there be a condition of the central body anything like that of the nebulæ named, it must be borne in mind that in the struggle for existence those swarms moving in the plane of the intakes and in the same direction, will be those that will longest survive; hence we ought to be able to explain the light-curves on the supposition that the conditions of the secondary swarms are as stated above, and it is seen that we can so explain them.

When we have more than one subsidiary swarm it is easy to see that certain relations of the regular periods of their orbital motions will produce an irregularity in the compound period; so that a rhythmic change of period will enable us to learn somewhat of the relation of the relative intensity and period of each of the swarms. We are really in presence of a *light-tide*, the elements of which can be found by analysis, as occurs with other tides nearer home.

The explanation suggested by the hypothesis of the variability of stars of Group VI. seems also to throw light upon the strange colours of some of them. R Leporis, for instance, one of the most marked variables in the group, is the famous crimson star observed by Mr. Hind. Now crimson = red + blue. All these stars are red, and in many of them the absence of the blue is one of the most emphasized characteristics of the spectrum.

But suppose that the secondary swarm which adds its light at maximum is a comet with the usual carbon bands, we shall get this condition of things:—

	Blue.	Green.	Citron.
Bands in star	masked by continuous absorption	absorbed ...	absorbed
Bands in subsidiary swarm ...	bright ...	bright ...	bright

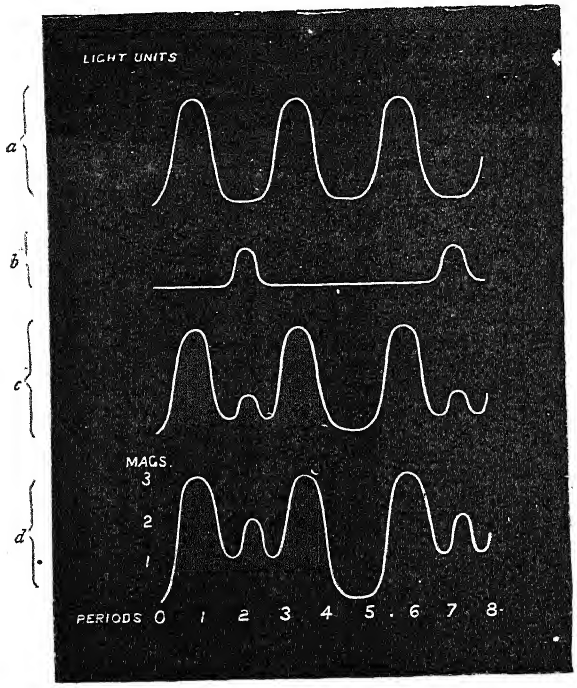


Fig. 7.—Periods 2 to 1. Apastra coincident.

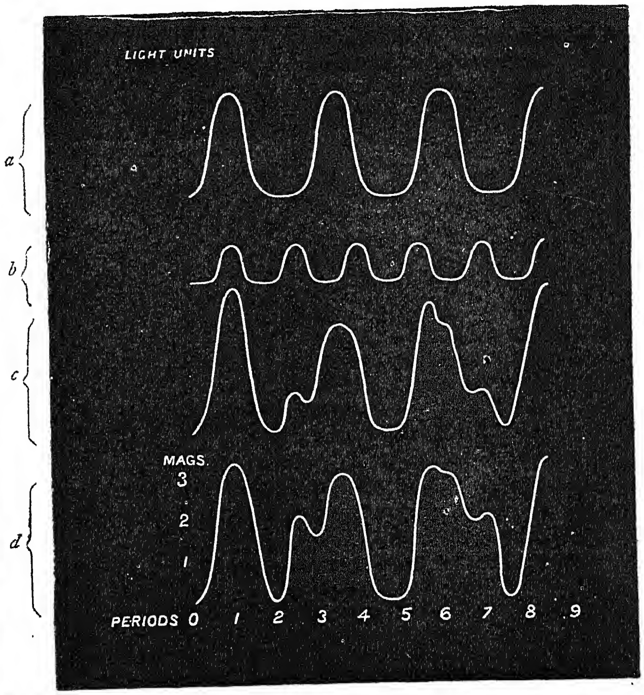


Fig. 9.—Periods 5 to 3. Periastra coincident.

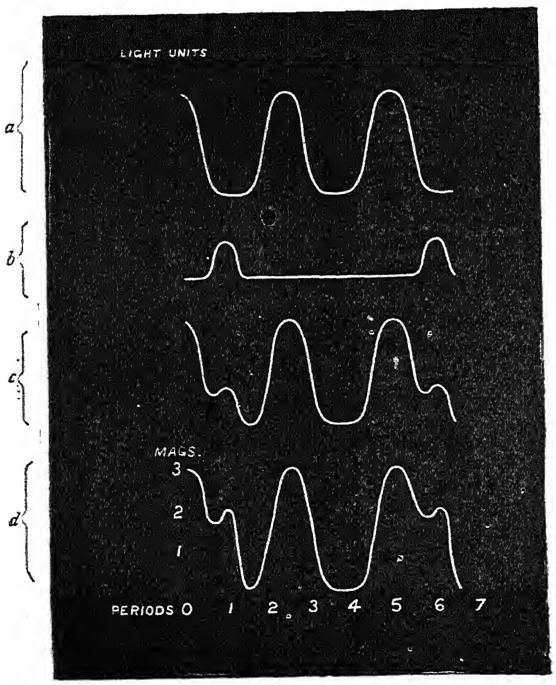


Fig. 8.—Periods 2 to 1. Apastra not coincident.

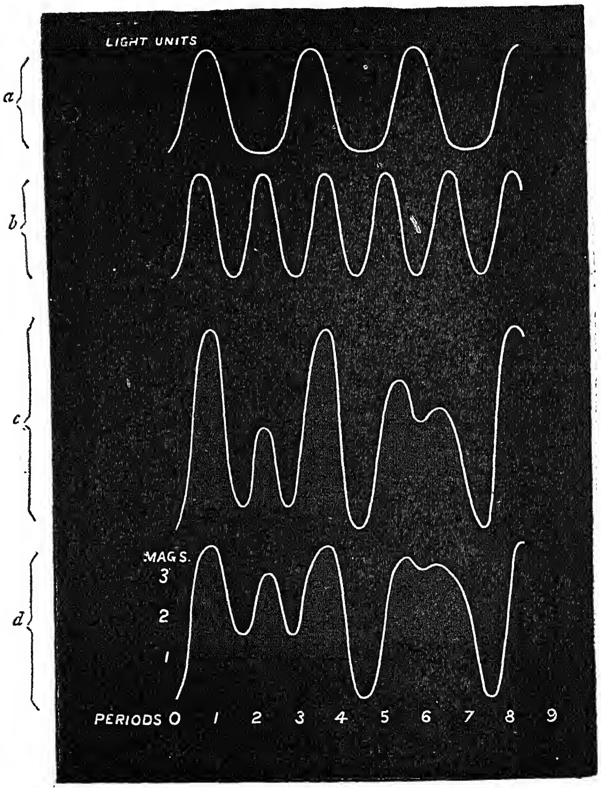


Fig. 10.—Periods 5 to 3. Periastra not coincident.

a Principal swarm. *b.* Subsidiary swarm. *c* Result in added light-units. *d.* Result in added magnitudes.

in other words, the bright fluting of carbon in the green and blue of the subsidiary swarm will just *mask* the absorption bands. They will *pale*, and the colour of the star (red) will be but slightly affected from this cause; but the blue flutings will be clear gain to the blue end of the spectrum, and crimson will result.

If this explanation be conceded, it is clear that comets travelling round such stars are conditioned very much like comets travelling round our own sun.

The general colour of the stars in Group VI. indicates that they are near the point of invisibility, the conditions being no doubt a red or white hot crust with a strongly absorbing atmosphere. It is worth while to point out that the cessation of all radiation of light from the central body need not prevent its passing on as a variable star to Group VII. As we must assume comets to be shreds of nebulæ, *i.e.* meteoritic fields or streams, filched by masses which pass near them; and as the mass remains after the light has gone, there will be the same attraction at work, and we have no right to assume that it will not act in the same way as heretofore.

We can gather from this that practically there can be no permanently dark bodies in space; they *must* at one time or another be accompanied by comets, and they must therefore be variables.

Here a most interesting point comes in: if the phenomena of the repulsion of comets' tails, or, in other words, the repulsion of carbon in some form or other from cometary swarms, depends upon the thermal energy of the central body, this result can no longer happen when the central body has cooled down. The effect of this upon the spectrum of such a compound system is well worth inquiring into.

In the hypothetical curves I have already given, I have dealt with simple cases. But in the stars there will be certain to be complex ones brought about by the successive periastra or apastra not being coincident in the two swarms (to deal only with two), and by different relationships in the periods.

I append (Figs. 7–10) some hypothetical curves worked out both in light-units and magnitudes, the conditions being stated for each. The paucity of actual light-curves available prevents any inquiry as to the stars in which the conditions here imagined actually exist, but in the absence of such knowledge it is still easy to gather that different periods separating maxima, secondary minima of unequal periods, and great variations in the rise to and fall from maximum, instead of necessarily being the result of "irregularity," are all demanded by the most perfect regularity, provided we have more than one swarm to deal with under conditions anything like those employed in the hypothetical curves above given.

If there is anything of value in what I have advanced, it is quite clear that the observations of variable stars and variable star catalogues require considerable revision. First, arrangements should be made with the observatories of America and India so that the observations of a certain number of stars in the northern hemisphere should be observed as continuously as possible. The relative brightening of the bright carbon flutings in stars of Groups I. and II., and the paling of the dark carbon flutings in Group VI., should be spectroscopically watched in each case.

It is highly important also that the precise group to which each variable belongs should be determined at once, and that this datum should take the first place in the working catalogues employed.

The observations should also be recorded when made on light-curves, the time ordinate being contracted as much as possible in order that the genesis of the compound curve may be suggested as soon as possible, so that future observations can be controlled, and the greatest attention be given at the critical periods.

The colour-observations have done their work and have had their day: less attention need now be directed to them, and much time will be liberated thereby.[1]

J. NORMAN LOCKYER.

THE LABYRINTHODONTS OF SWABIA.[2]

SWABIA, it need scarcely be said, lies to the south-east of Stuttgart—the classic ground of the Triassic Labyrinthodonts of Germany—and since it contains the same Triassic deposits, we should naturally expect to find therein the same species of this group of Amphibians. The present memoir, remarkable alike for the splendid plates with which it is illustrated, and for the care with which the specimens have been described, is devoted to making known to the scientific world the magnificent collection of Labyrinthodont remains which have been from time to time obtained from the Swabian and Würtemberg deposits, and are now preserved in the Museums at Stuttgart, Tübingen, and Munich. Of the seventeen plates with which this work is illustrated, a large proportion are folded ones of very considerable size, while all are especially noticeable for their beauty of execution. They appear to have been printed by some special process from photographs, the finely-preserved specimens of skulls standing out with wonderful clearness from a black background. Even more noteworthy than the unrivalled execution of the plates is the perfection and beauty of the specimens themselves; and we would especially direct attention to the magnificent skull of *Metopias*, represented in plates xii. and xiii. of the work before us, as being the finest Labyrinthodont specimen that has ever come under our notice.

In the introduction to his memoir, the author, after summarizing what has been previously written on the subject, glances at the chief groups into which it has been proposed to divide the Labyrinthodonts. The forms treated of in the present memoir all belong to the typical group, the Euglypta of the British Association Committee of 1874, and the Stereospondyli of Prof. von Zittel. This group has been generally characterized, among other features, by the fully ossified centra of the vertebræ; but Dr. Fraas remarks that it is very difficult to be sure of the nature of their vertebræ, and that, at least in *Mastodonsaurus*, either the caudal vertebræ, or the vertebræ of young individuals, are of that imperfectly ossified and segmented type to which the term "rhachitomous" has been applied, so that fully ossified vertebræ only occur in the trunk region of the adult. This seems to us to indicate very clearly that the pre-Triassic *Archegosauridæ*, in which the vertebræ are always "rhachitomous," can only be separated from the *Anthracosauridæ* and *Mastodonsauridæ* by characters of family value.

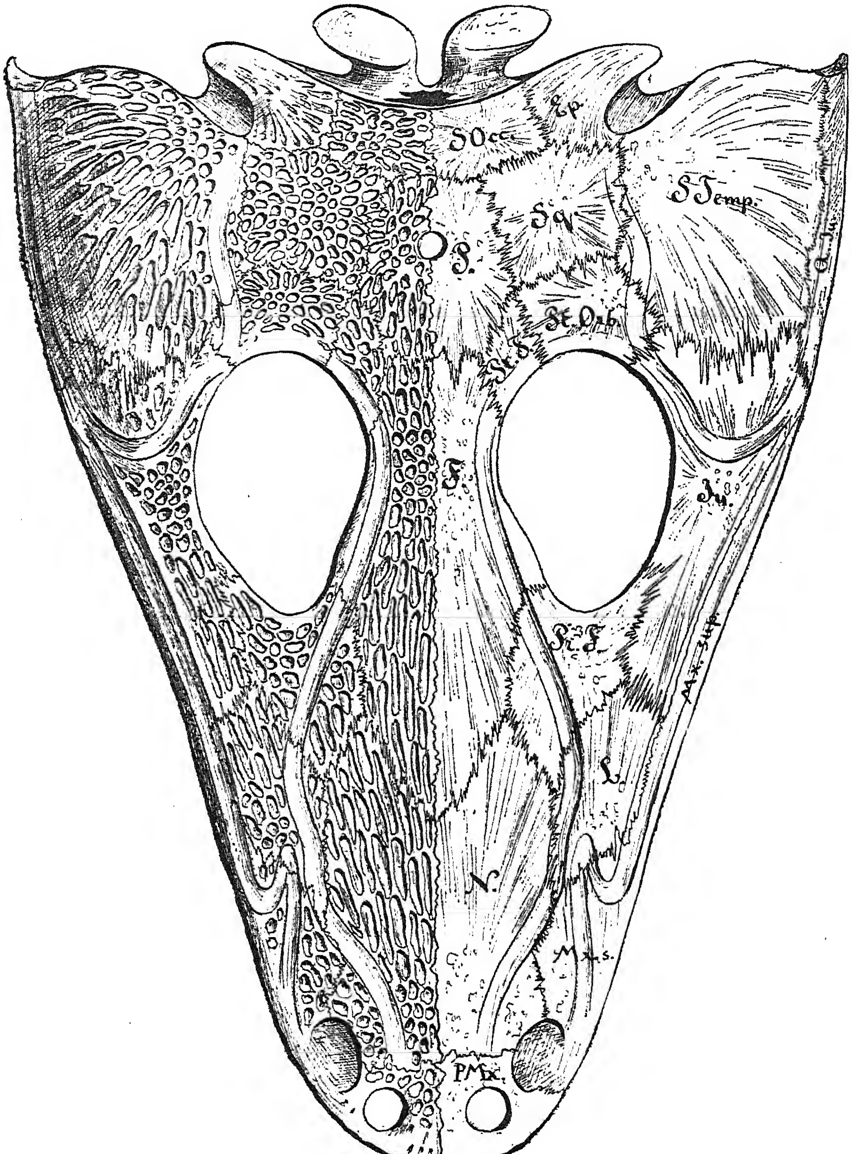
After the introductory portion, the author proceeds to discuss the geological divisions of the German Trias. Here, contrary to the views hitherto generally adopted, four main stages or groups (exclusive of the Rhætic) are recognized, viz. the Bunter-sandstein, the Muschelkalk, the Lettenkohle, and the Keuper. The separation of the Lettenkohle as a primary group distinct from the Keuper (in which it has hitherto been generally included) appears to rest on the ground that it contains many forms of Vertebrates common to the underlying Muschelkalk which do not occur in the typical Keuper. Thus the Sauropterygian genera *Nothosaurus* and *Simosaurus* range up into the Lettenkohle, but stop short of the true Keuper.

[1] I have to thank the Astronomer-Royal and Prof. A. S. Herschel for the correction of an error into which I had fallen in the part relating to light-units in the first draft of this article. The table on p. 546 I have extracted from one of the valuable letters with which Prof. Herschel has favoured me on this subject; it is fuller than the one it replaces.

[2] "Die Labyrinthodonten der schwäbischen Trias," by Eberhard Fraas. *Palæontographica*, vol. xxxvi. (1889), pp. 1–158, plates i.–xvii.

The same is true of *Mastodonsaurus giganteus*, although that form is succeeded by a closely allied species in the proper Keuper; the latter species, by the way, being not improbably the one occurring in the Bristol Rhætic, which was identified by the British Association Committee with *M. giganteus*. A table, with the names of the forms characteristic of each horizon, fully explains the author's views on all these points of geological classification.

Passing to the descriptive portion of the work, we may first of all observe that Dr. Fraas sees no reason to depart from the generally accepted homology of the bones forming the hinder part of the Labyrinthodont cranial roof; and he does not, therefore, accept the view of Dr. Baur that the bone usually termed the squamosal (*Sq.* of figure), is really the supra-temporal, and *vice versâ;* neither does he adopt the suggestion of the



Skull of *Mastodonsaurus giganteus*, one-fifth natural size. *S.Occ.*, supra-occipital; *Ep.*, epiotic; *P.*, parietal; *Sq.*, squamosal; *S.Temp.*, supra-temporal; *Pt.F.*, post-frontal; *Pt.Orb.*, post-orbital; *F.*, frontal; *Pr.F.*, pre-frontal; *N.*, nasal; *P.Mx.*, pre-maxilla; *Mx.sup.*, maxilla; *L.*, lachrymal; *Ju.*, jugal; *Q.Ju.*, quadrato-jugal.

same palæontologist that the bone usually termed epiotic (*Ep.*) really represents the opisthotic of other Vertebrates. The arrangement of the Labyrinthodont cranial bones is well shown in the woodcut of the skull of *Mastodonsaurus* given on p. 44 (reproduced here), in which we notice that the squamosal has a more rhomboidal form given to it than in the restoration published by the British Association Committee in 1874.

Of the several species described, the first is the well-known *Mastodonsaurus giganteus*, of the Muschelkalk and Lettenkohle, of which the author gives a new figure of the fine skull found in the Lettenkohle of Gaildorf in 1833, and so well known in all Museums by means of plaster-casts. This magnificent specimen, we learn, has recently been thoroughly cleaned from matrix, by which means the true relations of the bones can be

more clearly seen. A woodcut of the pelvis of the same species is especially valuable, and shows (as, indeed, had been previously well displayed in Prof. Cope's figure of the pelvis of *Eryops*) that the ossified pubis is very small, and takes no part in the formation of the acetabular cavity for the head of the femur. Another species of this genus from the Muschelkalk and Lettenkohle is described as *M. granulosus*, a second from the Lettenkohle as *M. acuminatus*, and a third from the true Keuper as *M. keuperinus*.

The genus *Capitosaurus* was originally described upon the evidence of a specimen of the skull from the Keuper of Franconia, a second species being subsequently described from the equivalent beds of Würtemberg as *C. robustus*; while *C. nasutus* and *C. fronto* are smaller forms from the Bunter of Bamburg. (It may be observed, in passing, that, in the Report of the British Association Committee, all mention of these two species from the Bunter is omitted, and it is thus only suggested that the genus might possibly be represented in these beds.) Dr. Fraas describes and figures a beautifully preserved skull of *C. robustus* from Stuttgart, which exhibits the very remarkable specific peculiarity that the epiotic gives off a process to join the supra-temporal, and thus converts the auditory slit into a foramen. The author would regard this feature as of sufficient importance to form a generic character, and he accordingly proposes to separate this species from *Capitosaurus*, with the appropriate designation of *Cyclotosaurus*. We are, however, rather inclined to agree with Prof. von Zittel, who regards the feature in question merely as a well-marked specific one.

Of still more importance are the skull and skeleton of *Metopias*. Hitherto, the occipital region of the skull of this genus has been undescribed; and the magnificent skull to which allusion has already been made shows that the restoration of this part by the British Association Committee was not altogether correct. The type species of *Metopias* must have been a huge creature, only second in point of size to *Mastodonsaurus*, its skull being some 2 feet in length. In addition to the skull, the affinities of this genus are illustrated by a slab showing both surfaces of the anterior half of the skeleton. In this beautiful specimen the three plates of the thoracic buckler are preserved in their natural position, and show well-marked differences from the corresponding bones of *Mastodonsaurus*. Thus, the median plate (interclavicle), instead of ending in a sharp posterior process like the corresponding bone (entoplastral) of a turtle, is rounded; while the lateral plates (interclavicles) meet in a long suture in advance of the median plate.

In thus making accessible to the scientific world the wonderful specimens of Triassic Labyrinthodonts preserved in the Museums of Germany, Dr. Fraas has laid all students of this branch of zoology under a deep obligation to him; and his work forms a fitting companion to the volumes containing Dr. Fritsch's description and illustrations of the smaller Labyrinthodonts of the older Permian beds of Bohemia. R. L.

NOTES.

THE proceedings of the Iron and Steel Congress seem to have excited much interest in America. About four hundred members of the Iron and Steel Institute and of the German Metallurgical Association are in New York, taking part in the meetings. The sittings of the American Institute of Mining Engineers began on Monday, those of the Iron and Steel Institute on Wednesday. According to the New York correspondent of the *Times*, the foreign delegates are much pleased with the arrangements made by the Americans for their reception.

THE Harveian Oration will be delivered by Dr. Andrew, at the Royal College of Physicians, on Saturday afternoon, October 18, at four o'clock.

THE eleventh annual "fungus foray" of the Essex Field Club will be held on Friday and Saturday, October 10 and 11, in Hatfield Forest, near Great Hallingbury, a remnant of about 1000 acres of the great forest of Essex. The head-quarters for the meeting will be at Bishop's Stortford. Papers will be read, and an exhibition of fungi and other botanical specimens held, under the direction of Dr. M. C. Cooke and Mr. George Massee. Any of our readers wishing to attend, should communicate with Mr. W. Cole, Hon. Sec., Essex Field Club, Buckhurst Hill, Essex.

THE Fruiterers' Company will hold an exhibition of fruit, at the Guildhall, London, on October 6, 7, and 8. Their objects are (1) to show what excellent fruit can be grown in this country; and (2) to afford information respecting the best sorts to plant, and how to cultivate them advantageously.

AT the annual meeting of the friends of the Manchester Technical School, held on Monday, it was resolved that the property and effects of the school should be transferred to the Whitworth Institute. A letter from Mr. Chancellor Christie, one of the trustees of the late Sir Joseph Whitworth, was read. In this letter he referred to the property of the Institute, and said the trustees were prepared absolutely to give and convey their property in Peter Street, of which at present the Technical School has the use. Upon the representations made to them that it was impossible for the Technical School to be carried on for the present unless it obtained from some source a subvention of £1000 a year, or thereabouts, the legatees had undertaken to provide that sum, if necessary, for a few years, and they had already made some annual payments of this amount. As, however, the Corporation of Manchester, under the powers conferred by the recent Act, had arranged for the payment of £2000 a year to the Technical School, the amount conditionally promised by the legatees would not be required. They were willing, in lieu of this, to contribute the sum of £5000 towards the building fund. The writer added, that while he should like to see a building in every respect adequate and satisfactory, he thought it would be a mistake to contemplate the immediate erection of a building on the scale of that of which plans were prepared about a year since. "The design, indeed, should embrace everything that could be needed for many years to come, but it should be so planned that a portion only could, and should, be immediately erected, leaving the rest for the future. In fact, it would follow the lines upon which the Owens College has been partially built. An expenditure of less than £100,000 should, I think, be sufficient for all the immediate purposes of the Technical School."

WITHIN the last few days, telephonic communication has been established between London, Manchester, Liverpool, and Lancaster by the National Telephone Company, Limited. The line is not yet open for use by the public, but it was placed at the disposal of the Manchester Field Naturalists' Society on Monday evening, when a discussion on the effects of fog and town atmosphere on plant-life was held between members in Manchester and a number of corresponding members in London. Dr. Bailey, of Owens College, proposed a scheme for the chemical examination of fog, with reference to its injurious effects on animal and vegetable organisms. Colonel Mackenzie, Superintendent of Epping Forest, denied its evil effects as far as plants were concerned; Prof. R. Meldola thought that the atmosphere of London was becoming more and more harmful to plants, and that this effect was probably due to the absence of light; Mr. Philip Hartog suggested that an attempt should be made to photograph the absorption spectrum of fog, and that a *daily* analysis of the air in large towns should be made in laboratories devoted to that purpose. A sub-committee was appointed to consider the subject further.

Another determination of the atomic weight of the element beryllium has been made by Drs. Krüss and Moraht, with the purest oxide that has probably ever been prepared (*Berichte*, No. 13, 2552). The result is eminently satisfactory to those who entertain a certain amount of belief in a modified "Prout's law," for the value obtained, 9·05 when oxygen is considered 16, is almost exactly a whole number, much nearer the round number 9 than the value obtained by Nilson and Petterson, 9·11, and the still earlier one of Awdejew, 9·22. The method employed consisted in igniting under special conditions the sulphate $BeSO_4 + 4H_2O$. The advantages attending the use of this salt are that it is capable of preparation by a method detailed in a former communication in an almost absolutely pure state, and it is not hygroscopic. The powdered crystals lose two molecules of their water of crystallization at 105° C., and the remaining two molecules at 250°–260°. When heated to redness the residual anhydrous sulphate is decomposed, beryllium oxide remaining. The last traces of sulphuric acid are completely removed by ignition to bright redness in a stream of air saturated with ammonia gas. The beryllia used for the preparation of the sulphate was obtained from three distinct minerals—leucophone, beryl, and gadolinite. Sixteen separate determinations were made, about 20 grams of the sulphate being ignited in each case. The excellent agreement between the results is seen from the fact that the maximum value obtained was 9·08, and the minimum 9·03, when oxygen = 16. It appears, therefore, that in the case of beryllium the value obtained for the atomic weight approximates the more closely to the whole number 9 the purer the materials and the more perfect the method employed in the work.

Writing to us on the subject of sonorous sand, Mr. Henry C. Hyndman asks whether Prof. H. C. Bolton is aware of an inland locality in South Africa, where it is stated the sands are sonorous. In a recent letter to the *Scotsman*, Mr. Hyndman mentioned that he had come across a paragraph in a work entitled "Twenty-five Years in an African Waggon," by Andrew A. Anderton, published in 1887, in which the author said, "Before leaving this part of the Griqualand West I should like to describe that peculiar sand formation on the west side of the Langberg mountain, which is in fact part of it. I heard from many of the Griquas and Potgielet living near it, that the lofty hills are constantly changing; that is, the sand hills, 500 and 600 feet in height, in the course of a few years subside, and other sand hills are formed where before it was level ground." And then in a footnote it is added, "I regret very much the description of this sand formation has been left out, it being the only extraordinary geological formation known in Africa, and fully describes the musical sand."

There is an interesting article in *Education* on the University Correspondence College (London and Cambridge) and its founder. Mr. William Briggs is the principal and founder, and the idea of teaching by correspondence first suggested itself to him while holding the appointment to a Marquis of Bute Professorship in a Scotch College. The idea soon took root, and the general method of work now adopted is as follows:—Students every week receive a scheme of work, consisting of selections from text-books, indications of important points, hints, notes on difficult portions of each subject that is under consideration. At the end of the first week, in addition to the above, a test paper is sent on the work of the preceding week, the answers to which are sent to the tutor on an arranged day. These are then examined and returned with corrections, hints, and model answers in each subject and solutions of all the difficulties. The advantage this method has over oral teaching is that all difficulties, &c., are committed to writing, and can be looked at over and over again and kept for future reference. The staff employed consists of forty tutors, whose academical careers were exceptionally brilliant, twenty-six of them having taken first places at London University examinations. The best judgment as to the work of the students may be formed from the following information:—In the Intermediate and B.A. examinations, 79 and 70 passes were obtained in one year. In the Honour and M.A. examination, 105 at the recent June examination passed, including the tenth, thirteenth, and seventeenth places in the Honour list; at Intermediate Arts, twenty took honours, one with a first and two with second places; at B.A., sixteen took honours, one being University Prizeman.

The Durham College of Science, Newcastle-upon-Tyne, has issued its Calendar for the session 1890–91. The schedules for the A.Sc. and B.Sc. degrees have been re-modelled, and are now on the same lines as the corresponding examinations in the London and Victoria Universities. Attention may also be drawn to the fact that the list of subjects on which courses of lectures are delivered includes agricultural botany.

The Calendar of the University College of North Wales for the year 1890–91 has been published.

We have received the calendar of the Imperial University of Japan (Teikoku Dargaku) for the year 1889–90 (22nd–23rd year of Meiji), published by the University at Tokyo. This University is under the control of the Minister of State for Education, and depends for its revenue upon annual allowances from the Treasury of the Imperial Government. An accumulation fund, made up of tuition fees and other sources, helps to pay the current expenditure of the University when the cases are of such a nature as to demand the outlay. The whole University, consisting of offices of the University, library, colleges of law, medicine, engineering, literature, and science, the first hospital of the College of Medicine, &c., are situated in the grounds at Molofujicho, Hongo, Tokyo, known as Kaga-yashiki. The Botanical Garden Observatory and the second hospital of the Medical College are all situated within the city bounds, and the Marine Biological Station is situated at Misaki. The Calendar contains information on everything concerning the University, viz. University offices, regulations for colleges, courses of instruction, examinations, scholarships, fees, &c. The appendix gives the address delivered by President Watanabe on the occasion of the annual graduation ceremony, July 10, 1889.

M. E. Drake del Castillo has recently published a memoir, rewarded by the French Academy of Sciences, on the Flora of Polynesia.

A catalogue of numerous works in every branch of astronomy has been issued by Felix L. Dames, Berlin, W., Taubenstrasse 47. To those who may wish to have copies of rare and standard astronomical publications at a reasonable price it will be found extremely useful.

Messrs. G. Philip and Sons, Fleet Street, have published a portable sun-dial adjustable for all latitudes and fitted with a compass. The model has been designed to illustrate simply and accurately the principle of the sun-dial. The equation of time on the 1st, 11th, and 21st of each month is given, so that civil time may be found. No table is given of magnetic variation, hence the fixing of the instrument in the magnetic instead of the geographical meridian involves a certain amount of error. The box into which the model fits is made exactly one cubic decimetre in capacity, and is intended to illustrate the decimal system of weights and measures.

The *Engineer* of the 19th inst. contains an important article on "Railway Axles in India," due to a remarkable statement in the Indian technical press, to the effect that steel railway axles

had not given satisfaction, and that at considerable expense iron axles were to be substituted for them. Our contemporary observes that questions have, naturally enough, been asked, and publishes Sir A. M. Rendel's reply:—"I originally," he says, "recommended the use of steel for axles on the Bombay, Baroda, and Central India metre gauge, because I thought that steel was not only better than iron, but because its price was not more than half that of the class of iron suited for axles. I was further moved to do so by an opinion that, whilst steel was improving in quality, and daily taking the place of the best classes of iron, the quality of those classes of iron must deteriorate, because the price obtainable for them must diminish. In respect to the relative price of steel and iron, I was quite right; in respect to their relative suitability for axles, I have been wrong, or, at any rate, premature. *Commercial steel, when used in axles, seems subject to deterioration, which makes it very brittle after a few years' wear.* The consequence is that we must do now what we should certainly have done at first, had all the facts been then before us—I mean, we must substitute iron for steel. It being also desired that our waggon loads should be increased, it has been found necessary to increase the size of our axles, and their weight in iron is now 231 lbs., instead of 198 lbs. in steel; and their present cost is £2 6*s.*, instead of about £1 2*s.* 6*d.* Had we originally supplied the large iron axles, capital would have been charged, for every such axle supplied, the sum of £2 6*s.* or thereabouts, instead of £1 2*s.* 6*d.*, and I can see no reason why, because we attempted an economy in the first instance, which experience has proved to be impracticable, capital should escape the larger charge now. It appears to me indisputable that the difference between the cost of the axles originally supplied and those now sent out should be charged to capital and not to revenue." The italics in this quotation are those of the *Engineer*, and the statement is a grave charge against steel, and one that the steel manufacturers will no doubt combat strongly. The *Engineer* does not tell us where these metre gauge axles have usually failed, and it is therefore difficult to find a reason for the wholesale rejection of steel as a suitable material for axles, always bearing in mind the general use of this material in this country for that purpose, with the greater mileage run with steel axles, be they crank or straight axles, over that obtainable from iron axles. It is to be hoped that the matter will be thoroughly thrashed out. The question of manufacture must not be overlooked. Steel rails occasionally fail at the ends owing to insufficient "crop" being cut off the rolled rail, *i.e.* the steel is not considered sound for about 3 feet from each end of the rail as it leaves the rolls, and is therefore usually rejected as "crop ends." If these axles are rolled ones, probably this might explain the failures, if they usually took place at the ends and not in the middles.

THE autumn series of science lectures at the Royal Victoria Hall began on Tuesday with a lecture on "Nebulæ," by Mr. J. D. McClure. The arrangements for lectures during the month of October are as follows:—October 7, "The Sun," by J. D. McClure; October 14, "Bees as Florists and Fruit Producers," by Rev. Prof. Cheshire; October 21, "The Colours of Animals and their Uses," by Dr. W. D. Halliburton; October 28, "Mountaineering," by H. G. Willink.

THE additions to the Zoological Society's Gardens during the past week include a Brown Bear (*Ursus arctos* ♂) from Russia, presented by Mr. G. W. Robinson; a Golden Eagle (*Aquila chrysaëtus* ♀) from the Rocky Mountains, Wyoming, U.S.A., presented by Mr. Percy Cooper; two Common Squirrels (*Sciurus vulgaris*), British, a Reticulated Python (*Python reticulata*) from Siam, purchased; five Viperine Snakes (*Tropidonotus viperinus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on October 2 = 22h. 46m. 16s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4827	—	—	22 36 13	+60 42
(2) G.C. 4883	—	—	22 55 43	+29 33
(3) β Pegasi	2	Yellowish-red.	22 58 12	+27 29
(4) π Aquarii...	5	Yellowish-white.	22 19 42	+ 0 49
(5) η Aquarii	4	White.	22 29 42	− 0 41
(6) α Pegasi	2	White.	22 59 18	+14 37
(7) 257 Schj.	9	Reddish-yellow.	21 51 9	+49 59

Remarks.

(1) In the spectrum of this nebula Dr. Huggins has recorded only the chief nebula line near λ 500. It is remarkable that Herschel describes it as easily resolvable, and if this be correct the nebula must be regarded as a cluster of nebulous stars. This important result is well worth checking, both by telescopic and spectroscopic observations.

(2) This nebula is described as "considerably bright; considerably large; little elongated; gradually much brighter in the middle; mottled as if with stars." The spectrum of the nebula is rather vaguely described by Dr. Huggins as follows: "The spectrum does not consist of one or two lines only; I believe it is continuous." The doubt should be removed by a more definite observation.

(3) The spectrum of this star is a very fine one of Group II. The dark bands, however, are by no means so strongly developed as they are in α Herculis or Mira, but still they are well marked, and, in addition, the spectrum abounds with lines. The relative feebleness of the bands, and the presence of so many lines, indicates that the star is well advanced in condensation, and this is further confirmed by my own observation of the comparatively feeble bright flutings of carbon. Most of the lines are identical with those seen in Aldebaran. D is partly hidden by a dark fluting, but lines in other places stand out prominently. They are not quite like those seen in the solar spectrum, and hence the spectrum of the star is one from which useful information relative to the criteria between Groups III. and V. may be derived.

(4) Prof. Pickering has found by photography that the spectrum of this star contains bright lines. Vogel, however, describes the spectrum as a well-marked one of the solar type, and makes no mention of bright lines. In my own observations of the star I found the dark lines characteristic of the spectrum of a star like the sun, but detected no bright ones. It is quite possible, therefore, that the bright lines are not always visible, and the spectrum should be examined with reference to a possible periodicity.

(5) According to the observations of Konkoly, this star has a well-marked spectrum of the solar type, but Gothard and Vogel describe it as one of Group IV. My own observations confirm those of Vogel.

(6) A star of Group IV.

(7) This faint star has a spectrum of Group VI., and Dunér states that the dark carbon band which separates the green and yellow zones is wider than he has seen it in any other star. The blue zone is scarcely perceptible. It seems as if this is a star which has cooled down until it is almost non-luminous, and the observation is important as indicating to a certain extent what the spectrum will be at the last stage of visibility. It appears that the carbon absorption intensifies, until finally all the remaining light is absorbed. A. FOWLER.

THE TELLURIC SPECTRUM.—The current number of *Comptes rendus* (September 22) contains an account of the expedition lately made to the summit of Mont Blanc by M. Janssen, for the purpose of observing the spectrum of the sun from an elevated station. Two years ago M. Janssen ascended Mont Blanc as far as the Grands-Mulets, a station having an altitude of 3050 metres. The spectroscopic observations then made showed that a diminution took place in the intensity of the groups of lines A, B, and a, due to the action of the oxygen in the atmosphere, and indicated very definitely that at the limits of our atmosphere these groups would disappear. To confirm these results it was resolved this year to repeat the observations at a greater elevation,

and after considerable difficulties the Cabane des Bosses was reached on August 18. The altitude of this station is 4400 metres. Work was commenced four days later, and precisely similar results obtained. The B group, which appears to consist of ten well-defined doubles when observed at Meudon, and was almost reduced to the last double at Grands-Mulets, had disappeared altogether.

These observations, in conjunction with those made last year between the Eiffel Tower and Meudon, those made by M. de la Baume Pluvinel at Candia during the annular eclipse of June 17, and those made in the laboratory at Meudon, definitely demonstrate the absence of oxygen from the sun, or, at least, of oxygen in the state that we know it.

M. Janssen thinks that, in the interests of astronomical and terrestrial physics and of meteorology, an Observatory should be established on the summit of Mont Blanc. The difficulties to be overcome in the erection of a station at such an elevation are great, but that they are not insurmountable is evidenced by the observations that have just been made.

ASTRONOMY AND NUMISMATICS.—Dr. A. Vercoutre, in *L'Astronomie* for September, points out how astronomical knowledge may be of service to numismatical science. It is known that on many antique medals, and notably on the coins of the Roman Republic, stars and members of the solar system figure sometimes as symbols and sometimes as heraldic allusions to the magistrate by whom the coin was struck. Thus, on a coin struck by L. Lucretius Trio, 74 B.C., the seven stars in Ursa Major are shown, and this constellation, being named Septem Triones, was evidently used as a phonetic allusion to the surname (Trio) of the magistrate. Again, on a coin struck in B.C. 43, Dr. Vercoutre noticed five stars, one of which was much larger than the others. He therefore concluded that the constellation represented on the coin was Taurus, as this was the only group of five stars known to the ancients in which one was more brilliant than the others. On this account he was enabled to attribute the coin to P. Clodius Turrinus, who apparently used the constellation Taurus or Taurinus as a phonetic signification of his surname. A coin struck by Manius Aquillus, B.C. 94, has figured upon it the first four stars in the constellation Aquila. They are shown in nearly the same relative positions occupied in reality, hence the coin contains the oldest known representation of a portion of the celestial vault. It is therefore possible that an inspection of the stars figured on old coins may be the means of ascertaining the identity of the magistrate under whom they were struck, or, knowing this and the constellation represented, they may be useful for the determination of proper motion.

GEOGRAPHICAL NOTES.

M. ANDRUSOFF, whose researches into the geological history of the Caspian Sea have been mentioned more than once in NATURE, gives now some interesting preliminary results of his exploration of the Black Sea. After having carefully studied all that was previously known about that sea and embodied it in an excellent paper (published in the last issue of the *Izvestia* of the Russian Geographical Society, vol. xxvi., 2), he induced the Hydrographical Department of the Russian Navy to send out a special gunboat for the exploration of the Black Sea, under Captain Spindler and Captain Wrangel. The sea was thus carefully explored from Odessa to Constantinople, and thence to Batum and Sebastopol. It appears that great depths are found everywhere within a short distance of the shore; and that from a depth of 200 metres the water of the Black Sea begins to contain sulphuretted hydrogen resulting from the decomposition of decaying organisms, so that no organisms either vegetable or animal, are met with at depths exceeding 200 metres. The Black Sea, he concludes, is not a sea, properly speaking, but an immense stagnant pond (reaching a maximum depth of 1200 fathoms) which is covered on the surface by the water of the Mediterranean and the rivers which flow into it. The full report of M. Andrusoff is expected soon, and is sure to be full of interest.

A TELEGRAM, dated Tashkend, September 15, gives some extracts from a letter written by M. Grombchevsky on July 20, at Sel-kilian. The expedition had at last reached Tibet from the north; but the hostility of the ruler of Keria compelled them to undertake the journey too early in the spring. On May 21, they were on the Tibet plateau, but weather was most inclement at that time. Hard frosts (20° C. below zero), terrible snow-storms, and a complete want of water—the snow in the mountains not having yet begun to thaw—compelled the expedition to return to Kashgaria without having accomplished the proposed programme of exploration. Later on, the want of money prevented them from returning to Tibet in the summer. M. Grombchevsky also adds that the ruler of Kanjut has entered into vassal relations to the Government of India, and that the fort Shahidulla-hodja is occupied by a garrison of Kashmerees, thus commanding the drainage area of the Raskem-daria and its pasture grounds. Besides, in April last, the beck of Kanjut took possession of the Pamir and Dangarym-bash forts, formerly occupied by Chinese garrisons; so that the fort Pamir, which is now practically under English influence, and the Russian fort Kara-kul are separated by but a three days' march over a territory densely peopled with Kirghizes. We may thus expect that the veil which has for so many centuries concealed those regions from science will soon be entirely lifted, and Northern Tibet will become as well known as Central Asia.

"THE AGE OF SCIENCE."

ON Friday evening last, Lord Derby, before distributing the awards of the Liverpool School of Science, delivered a clear, vigorous, and interesting address on some aspects of science. Ours, he said, would be remembered as pre-eminently the age of science. Our successors might excel us as writers, as politicians, as soldiers; they might surpass even the industrial energies of the present time, but it was not likely—it was scarcely possible—that in the region of science the twentieth century should witness advances greater than, or as great as, those of the nineteenth. The general experience of the world had been that brilliant but brief epochs of advance had been followed by long intervals of stagnation, and sometimes even of retrogression. Retrogression was not likely, but stagnation was quite possible. There was one phrase much employed when people talked on these subjects which, to his mind, contained a fallacy. He meant the common phrase of popularizing science. To popularize science was simply impossible. Anybody could cram up, with the help of an average memory and of easily acquired hand-books, a summary of what had been done in astronomy, in chemistry, or other sciences, but when that result was accomplished he would be very little nearer to any real gain which science could bring to him. It was only labour and perseverance, added to natural capacity, that could give a scientific mind. Some tincture of scientific knowledge was desirable for every educated person. The result might not be great, but the process was valuable. An entire absence of the scientific spirit was no doubt compatible with brilliant talent and high distinction. We did not find fault for a deficiency of that kind in a novelist, a poet, or a writer of light literature, but it was a deficiency notwithstanding. If asked what he meant by a scientific spirit, he thought he knew, but he must confess that it was more easily described in vague and general terms than precisely defined. He meant by it, in the first place, a habit of accuracy and exactness in matters of fact. In the next place, he meant that temper of mind which seeks for conclusions, but does not jump at them—which is equally opposed to the stupid incredulity of ignorance, refusing to accept any idea which is not familiar; to the reverential credulity which accepts as true any statement coming down from old or high authority; and to the careless indifferentism which, so long as a theory looks and sounds well, and especially if it flatters some previously existing feeling of prejudice, does not care on what foundation of reality that theory rests. That the world is governed by laws which we did not make and cannot abolish—laws which will operate whether we recognize or ignore them, and which it is our wisdom therefore to study that we may obey, and in obeying utilize them —that was what was taken to be the outcome of scientific teaching, and if anybody thought that a useless or an unimportant or unnecessary lesson he did not agree with him. Something else science, rightly understood, would teach us to know—what it is that we can hope to know and to understand; and to recognize how little that is, and how much lies, and probably always will lie, beyond the reach of our faculties. One word only he would add—that, having known men of many professions, he should say, as far as his observation went, the happiest lives were those which had been devoted to science. "Every step," said Lord

Derby, "is interesting, and the success of those who do succeed is lasting. What general, what orator, what statesman, what man of letters can hope to leave a memory like that of Darwin? An invalid in health, a man who seldom stirred from home, a man until his later years very little known to the outer world, but who, from his quiet study, revolutionized the thought of Europe, and will be remembered as long as Newton and Bacon. If fame be ever worth working for—I do not say it is—that kind of fame is surely, of all, the most durable and the most desirable. Well, I have perhaps digressed from our proper subject, for it is not likely that we have a future Darwin in this room, but it is no exaggeration to say that, as a rule, no man who has taken to science as the work of his life regrets the choice, while men who have done important work in other lines feel like Renan, who, at the height of his literary eminence, tells us in his autobiography that he has often regretted that science, rather than historical research, had not been the object of his early pursuit."

MIMICRY.[1]

THE relationship of mimicry to other animal colours can only be explained by giving a short account of the latter.

I. The commonest use of colour is for concealment (*cryptic*), enabling an animal (1) to escape its enemies, or (2) to approach its prey. In these (1) protective (*procryptic*) or (2) aggressive (*anticryptic*) resemblances, animals seek concealment by a likeness to some object which is of no interest to enemies or prey respectively. Similar effects may be produced by the use of foreign objects with which the animal covers itself to a greater or lesser extent (*allocryptic*).

EXAMPLES.—(1) *Procryptic Colours.* A green pipe-fish (*Siphonostoma typhle*) conspicuous in the water, but well concealed among the leaves of *Zostera:* the brown lappet moth (*Gastropacha quercifolia*), conspicuous on a smooth deal board, but well concealed among dead leaves.

(2) *Anticryptic Colours.* A large frog (*Ceratophrys cornuta*) from tropical South America, which almost buries itself in a hole in the ground, while the head, which is exposed, harmonizes with the surroundings. In this position it waits till the small animals on which it feeds approach or even walk over it.

(3) *Allocryptic Colours.* A small English crab (*Stenorhynchus phalangium*) which decks itself with pieces of seaweed: another small English crab (*Hyas coarctatus*) was shown with and without its covering of pieces of seaweed (*Ulva*, &c.).

Mimicry is closely related to the colours illustrated above, but differs in that the animal resembles an object which positively repels its enemies or positively attracts its prey rather than one which is of no interest to either. It is better, therefore, to defer its consideration until after the description of the colours which form the models for mimicry.

II. The second great use of colour is to act as a warning or signal (*sematic colour*), repelling enemies by the indication of some unpleasant or dangerous quality (*aposematic* or *warning colours*), or signalling to other individuals of the same species, and thus assisting them to escape from danger (*episematic* or *recognition colours*). In a very interesting group of cases (*allosematic*), the animal warns off its enemies by associating with itself some other animal with unpleasant qualities and warning colours.

EXAMPLES.—(1) *Aposematic Colours.* The two unpalatable English moths (*Spilosoma urticæ* and *S. mendica*, female), when disturbed, assume attitudes which serve to display their conspicuous yellow and black colours. Portchinski has recently shown that an unpalatable European chrysalis (*Limenitis populi*) bears the most detailed resemblance to a chrysalis which has been pecked and rejected by a bird. The American skunks (*Mephitis mephitica*, *Conepatus mapurito*, &c.) possess the power of emitting an intolerable stench, and are extremely conspicuous black and white mammals.

(2) *Episematic Colours.* In the common rabbit the white tail serves as a beacon to other individuals, pointing the way to the burrow.

(3) *Allosematic Colours.* A hermit crab (*Pagurus bernhardus*) is commonly found with a sea anemone (*Sagartia parasitica*) attached to its shell; in another hermit crab (*Pagurus prideauxii*) the association is more constant, and the sea anemone (*Adamsia palliata*) is specialized for life on the shell of the crustacean. Two crabs (*Polydectus cupulifer* and *Melia tessellata*), described by Möbius in some of the islands round Madagascar, invariably held a sea anemone in each claw. Two other groups of animals, sponges, and ascidians, in addition to sea anemones, are avoided by the enemies of the Crustacea, and these are also made use of by the latter. Thus the hermit crab (*Pagurus cuanensis*) is found in shells which are covered with a (generally) brightly-coloured sponge (*Suberites domuncula*): Möbius also describes a hermit crab (*Ascidiophilus caphyræformis*) which lives in a case formed by an ascidian.

III. Mimicry may be defined as false warning or signalling colours (*pseudosematic*), repelling enemies by the deceptive suggestion of some unpleasant or dangerous quality (*pseudaposematic*) or attracting prey by the deceptive appearance of something attractive to them (*pseudepisematic*). Even foreign objects commonly associated with some well-defended and aggressive species may be mimicked by a comparatively defenceless form (*pseudallosematic*).

EXAMPLES.—(1) *Pseudaposematic Colours.* The various degrees of complexity with which protective mimicry occurs in insects was shown by examples of Indian and African Lepidoptera.

(*a*) Both sexes of the Indian *Papilio agestor* closely resemble the much commoner and nauseous butterfly *Euplœa tytia.*

(*b*) An Indian moth (*Epicopeia philenora*) similarly mimics an unpalatable butterfly (*Papilio protenor*), but in this case the male moth mimics the appearance of the male butterfly, and the female moth that of the female.

(*c*) If the mimicking species became common relatively to the mimicked, the deception would be liable to be detected. We therefore find that two or more models are often mimicked by the same species. Thus the male of the Indian *Elymnias leucocyma* mimics *Euplœa binotata*, while the female mimics the female of *Euplœa linnæi.* Both these Euplœas are also imperfectly mimicked by day-flying moths (*Amesia midama*). So also the male of the Indian *Papilio castor* mimics *Papilio chaon*, while the female mimics *Euplœa core:* in the south, *Papilio chaon* is absent, and BOTH sexes of the species (*Papilio dravidarum*) which represents *P. castor*, mimic *E. core.*

(*d*) Female butterflies are exposed to more dangers than the swiftly-flying males, and we find many instances in which the former are mimetic, although the latter are not. Thus the female of *Hypolimnas bolina* mimics *Euplœa core*, while the male is non-mimetic. The same is true of *Hypolimnas misippus*, the female of which mimics *Danais chrysippus.* Two forms closely allied to the latter (some regard them as merely varieties) are also mimicked by the former.

(*e*) The mimetic females also often resemble two or more different species of nauseous butterflies. Thus the female of *Papilio pammon* appears in two forms, mimicking respectively *Papilio hector* and *P. aristolochiæ;* while the females of *Euripus halitherses* (the male of which is probably mimetic) mimic *Euplœa rhadamanthus* and *Euplœa deione.*

(*f*) There are also striking examples in which the non-mimetic ancestor of a mimetic species has been preserved, *e.g.* in an adjacent island. Thus the female of *Elymnias undularis* mimics *Danais genutia* in Sikkim and North-East India; in Rangoon and Burmah there is a variety of the latter with white hind wings which is as common as the typical form, and the female of *E. undularis* is beginning to mimic this variety; in South India *E. undularis* is represented by *E. caudata*, in which the male is also beginning to mimic *D. genutia*, and the female is a more perfect mimic than in the other localities; in the Andaman Islands *E. cottonis* represents *E. undularis*, and both sexes appear to be non-mimetic, while *D. genutia* has never been recorded from this locality. A still more wonderful example is found in Africa and adjacent islands. *Papilio meriones* of Madagascar is non-mimetic, and the sexes are alike; the same is true of a closely-allied species, *P. humbloti*, recently discovered in Grand Comoro, and of *P. antinorii* recently found in Abyssinia. A very nearly related species in West Africa has a closely similar non-mimetic male, while two forms of female mimic *Danais chrysippus* and *Danais niavius.* In South Africa *Papilio cenea* has an almost identical male, while the females mimic *D. chrysippus*, the southern form of *D. niavius*, and two varieties of *D. echeria.*

(*g*) There are also examples which show us the origin of mimicry, in which the resemblance is very imperfect, but, nevertheless, sufficient to afford protection. The blue Euplœas of

[1] Abstract of Lecture delivered by Edward B. Poulton, F.R.S., on Friday, September 5, at the Leeds meeting of the British Association.

India, &c. (such as *E. harrisi*, *E. linnæi*, *E. splendens*, and *E. irawada*) form a very characteristic group, while their general type of appearance is imperfectly mimicked by a group of day-flying moths (*Amesia midama*, *A. aliris*, *A. sanguiflua*). It is extremely probable that the wonderfully close likeness of many mimetic species arose by gradual stages from some general resemblance to a type of colour or pattern possessed by some large group of unpalatable insects.

The above-cited examples are some of them well-known, they were chosen to illustrate the various different ways in which mimicry occurs.

Evidence for the evolution of mimetic resemblance has also been forthcoming as the result of recent and hitherto unpublished work.

Many moths have lost the scales which are characteristic of the order of insects to which they belong, so that their wings become transparent, and they mimic stinging insects such as wasps or hornets. This is the case with two British hawk-moths (*Hemaris fuciformis* and *bombyliformis*). It is known that when these moths emerge from the chrysalis, the transparent parts of their wings are thinly covered with scales which are shaken off during the first flight. The loss of the scales has now been shown to be due to the rudimentary nature of the stalk at the base of the scale and of the socket in which the stalk is inserted; a closely-allied Indian moth (*Hemaris hylas*) was still more completely denuded of scales, but in it also the rudimentary sockets were found to be thinly scattered over the transparent part of the wing. These facts suggested that all moths with transparent wings may be found to repeat, in the course of their own individual lives, the history of the change by which the transparency has been attained by the species. Investigation has supported this suggestion. The examination of two British moths which resemble hornets or wasps was especially instructive. In one of these (*Sesia apiformis*) the mimicry is not so perfect as in the other, and is therefore presumably of more recent date; in this moth the rudimentary scales which fall off are comparatively perfect, while in the other species (*S. bembeciformis*) they are far more degenerate, inasmuch as they have been useless to the species for a far longer period of time. It is interesting to note that these degenerate scales have not been reduced in size in either species, but are, on the contrary, much larger than the scales which are retained for the whole life of the moth. In the allied "clearwings" of the genus *Trochilium*, the transparency of the fore wing has been attained by the transparency of scales which are retained as well as by the loss of scales.

(2) *Pseudepisematic Colours.* This division not only includes the examples of aggressive mimicry in which an animal resembles another, and so is enabled to approach and injure it in some way, but also the cases of alluring colouring in which an animal possesses a lure which is attractive to its prey.

Examples of the former are seen in the flies of the genus *Volucella*, which are enabled to lay their eggs in the nests of humble-bees, &c., because of their close resemblance to the latter. The larvæ of the fly feed upon those of the bee.

Examples of alluring colouring. An Asiatic lizard (*Phrynocephalus mystaceus*) possesses pink flower-like structures at the corners of its mouth, it is probable that flies, &c., are thus allured. A terrapin (*Macroclemmys Temminckii*) from the Southern States of America, when hungry, opens its mouth and moves about two filaments at the anterior end of its tongue. These look like worms moving in a crevice in the rocks, and attract prey. The animal is otherwise perfectly motionless, and resembles a weed-covered rock. The fish *Lophius piscatorius* (the angler or fishing-frog) attracts its prey by a brightly coloured lure placed over its large mouth, the rest of the body being concealed. Certain deep-sea fishes allied to *Lophius* (*Ceratias bispinosus*, *C. uranoscopus*, &c.) have a phosphorescent lure which attracts the other fish on which they feed.

(3) *Pseudallosematic Colours.* A very striking instance was discovered by Mr. W. L. Sclater in tropical South America. The well-defended and abundant leaf-carrying ants (*Œcodoma*) are mimicked by an immature Homopterous insect possessing a shape and colour which closely resemble the ant together with the piece of leaf it is carrying.

IV. *Epigamic colours* are the bright tints and patterns displayed during courtship. As in other classes of colours the same effects may be produced by the use of foreign objects (*Allepigamic*). Examples are found in the various beautiful or curious objects collected by bower-birds for the decoration of their bowers. Especially interesting in this respect is the *Amblyornis inornata* of New Guinea.

Mutual relationship of the above-mentioned classes of colours. It is clear that I. (*Cryptic*) and III. (*Pseudosematic*) colours are closely related; they may be conveniently grouped under one head:—*Apatetic* or deceitful colours. The following scheme will be found to represent the mutual relationships:—

I. *Apatetic Colours.* (Resembling the environment, or some other species, or acting as a lure.)		II. *Sematic Colours.* (Warning and Signalling.)	III. *Epigamic Colours.* (Displayed in Courtship.)
A. *Cryptic Colours.* (Protective and Aggressive Resemblances.)	B. *Pseudosematic Colours.* (False Warning and Signalling Colours.)		
(1) *Procryptic Colours.* (Protective Resemblances.)	(1) *Pseudaposematic Colours.* (Protective Mimicry.)	(1) *Aposematic Colours.* (Warning Colours.)	
(2) *Anticryptic Colours.* (Aggressive Resemblances.)	(2) *Pseudepisematic Colours.* (Aggressive Mimicry and Alluring Colouring.)	(2) *Episematic Colours.* (Recognition Markings.)	
Allocryptic Colours. (Concealment gained by use of foreign objects.)	*Pseudallosematic Colours.* (Resemblance to some foreign object associated with mimicked species.)	*Allosematic Colours.* (Warning Colours of another Animal made use of.)	*Allepigamic Colours.* (Display of foreign objects in Courtship.)

The comparatively new terms employed in the lecture were due to the kind help of Mr. Arthur Sidgwick. The beautifully painted lantern slides were due to the great skill and patience of the artist, Mr. H. M. J. Underhill. The examples of *Allocryptic* and many of those of *Cryptic* and of *Allosematic* colours were painted from the living animals in the Marine Biological Laboratory at Plymouth. Colonel Swinhoe had very kindly suggested good examples of mimicry among Indian butterflies, and had lent from his beautiful collection the specimens for copying. Mr. H. Grose-Smith had kindly lent the African examples. Rev. F. J. Smith had most kindly helped in photographing the examples selected. Mr. W. R. Morfill had kindly translated Portchinski's Russian paper, thus rendering possible the use of some very interesting examples.

FORTHCOMING SCIENTIFIC BOOKS.

MESSRS. LONGMANS AND CO. announce the following:—"The Principles of Chemistry," by D. Mendeléeff, translated by George Kamensky, of the Imperial Mint, St. Petersburg, and edited by A. J. Greenaway; "Text-Book of Chemical Physiology," by Dr. W. D. Halliburton; "Human Physiology," being the substance of lectures delivered at the St. Mary's Hospital Medical School from 1885 to 1890, by Dr. Augustus D. Waller; "Elements of Materia Medica and Therapeutics," by C. E. Armand Semple, illustrated; "Notes on Building Construction," arranged to meet the requirements of the Syllabus of the Science and Art Department of the Committee of Council on Education, South Kensington—Part IV. "Calculations for Structures," illustrated; "Preliminary Survey, including Elementary Astronomy, Route Surveying, Tacheometry, Curve-ranging, Graphic Mensuration, Estimates, Hydrography, and Instruments," by Theodore Graham Gribble, illustrated; "Optical Projection: a Treatise on the Use of the Lantern in Exhibition and Scientific Demonstration," by Lewis Wright.

Among the scientific works promised by Messrs. Macmillan and Co. are the following:—Dr. Lauder Brunton's Croonian Lectures, "On the Connexion between Chemical Constitution and Physiological Action, being an Introduction to Modern Therapeutics"; "A Manual of Public Health," by A. Wynter Blyth; "Dictionary of Political Economy," edited by R. H. Inglis Palgrave, F.R.S.; "The Scope and Method of Political Economy," by I. N. Keynes, second edition; "Outlines of Psychology," by Dr. Harald Höffding, translated by M. G. Lowndes; "The Meteoritic Hypothesis," by J. Norman Lockyer, F.R.S., illustrated; "Electricity and Magnetism," a popular treatise, by Amédée Guillemin, translated and edited, with additions and notes, by Prof. S. P. Thompson, illustrated; "Popular Lectures and Addresses," by Sir William Thomson—Vol. III. "Papers on Navigation"; "Are the Effects of Use and Disuse Inherited?" by W. Platt Ball (Nature Series); new editions of Dr. Russel Wallace's "Contributions to the Theory of Natural Selection: and Tropical Nature and other Essays," and "The Malay Archipelago: the Land of the Orang Utan and the Bird of Paradise"; "The Myology of the Raven (*Corvus corax sinuatus*): a Guide to the Study of the Muscular System in Birds," by R. W. Shufeldt, illustrated; "Text-book of Comparative Anatomy," by Dr. Arnold Lang, translated by Henry M. Bernard and Matilda Bernard, with preface by Prof. Ernst Haeckel, two volumes, illustrated; "Lessons in Elementary Biology," by T. Jeffrey Parker, illustrated; "A Text-book of Physiology," by Prof. Michael Foster—Part III. "The Central Nervous System and its Instruments"; a new edition of "The Chemistry of the Hydrocarbons and their Derivatives, or Organic Chemistry," Vol. III. Part III., by Sir H. E. Roscoe and Prof. C. Schorlemmer; "The History of Chemistry," by Prof. Ernst von Meyer, translated by George McGowan; "Elements of Physics for Public Schools," by C. Fessenden; "Sound, Light, and Heat: an Elementary Text-book," by D. E. Jones, illustrated; "Elementary Applied Mechanics," by James H. Cotterill and J. H. Slade; a new edition of Todhunter's "Plane Trigonometry," revised by R. W. Hogg; "The Geometry of Position," by R. H. Graham, C.E., illustrated; "Manual of Logarithms," by G. F. Matthews; a new edition of "Class-book of Geology," by Archibald Geikie, F.R.S.; two volumes of "Macmillan's Geographical Series," edited by Archibald Geikie—"A Geography of Europe," by James Sime, and "Maps and Map Drawing," by William A. Elderton; and a "Physical and Political School Atlas," by J. G. Bartholomew.

The Clarendon Press announce "Mathematical Papers of the late Henry J. S. Smith, Savilian Professor of Geometry in the University of Oxford," with portrait and memoir, in two volumes; "A Treatise on Electricity and Magnetism," by J. Clerk Maxwell, new edition; "An Introduction to the Mathematics of Electricity," by W. T. A. Emtage; "A Manual of Crystallography," by M. H. N. Story-Maskelyne; "Translations of Foreign Biological Memoirs"—III. "Contributions to the History of the Physiology of the Nervous System," by Prof. Conrad Eckhard, translated by Miss Edith Prance, and a translation of Prof. Van't Hoff's "Dix Années dans l'Histoire d'une Théorie," by J. E. Marsh; and Count H. von Solms-Laubach's "Introduction to Fossil Botany," translated by the Rev. H. E. F. Garnsey, and edited by I. Bayley Balfour, F.R.S.

The Pitt Press announce "The Collected Mathematical Papers of Arthur Cayley, F.R.S.," Vol. III.; "Mathematical and Physical Papers," by Sir W. Thomson, Vol. III.; "A Treatise on Plane Trigonometry," by E. W. Hobson; "A Treatise on Analytical Statics," by E. J. Routh, F.R.S.; "A Treatise on Statics and Dynamics for Schools," by S. L. Loney; and two volumes of the "Pitt Press Mathematical Series"—"The Elements of Geometry after Euclid, Books III. and IV.," edited by H. M. Taylor, and "Elementary Algebra, with Answers to the Examples," edited by W. W. Rouse Ball.

Messrs. Smith, Elder, and Co. have in preparation a new work by Prof. Ferrier, being "The Croonian Lectures on Cerebral Localization," delivered before the Royal College of Physicians, June 1890.

Messrs. Sampson Low, Marston, and Co. announce "The Structure of Fibres, Yarns, and Fabrics: a Practical Treatise for the Use of all Persons employed in the Manufacture of Textile Fabrics," by E. A. Posselt, illustrated; and "Directory of Technical Literature: a Classified Catalogue of all Books, Annuals, and Journals published in England, America, France, and Germany, including their Relations to Legislation, Hygiene, and Daily Life," by Fritz von Szczepanski.

Messrs. Bell and Sons announce a revised edition of Deighton's "Euclid," Books I. and II., and Books I. to III.; "The Elements of Trigonometry," by J. M. Dyer and the Rev. R. H. Whitcombe, assistant masters of Eton College; "Solutions to the Problems in Dr. Besant's Elementary Hydrostatics"; a Key to "Examination Papers in Trigonometry," by G. H. Ward; "Colour in Woven Design," by Roberts Beaumont; and "Structural Mechanics," by R. M. Parkinson.

Messrs. Philip and Son will issue "Commercial Geography," a series of lectures by J. Scott Keltie, Librarian of the Royal Geographical Society, with numerous coloured maps and diagrams; "Across East African Glaciers, being an Account of the First Ascent of Mount Kilima Njaro," by Dr. Hans Meyer; "The Development of Africa," by A. Silva White, Secretary of the Scottish Geographical Society, illustrated with a set of 14 maps, specially designed by E. G. Ravenstein; "Magellan and the Pacific," by Dr. F. H. H. Guillemard, illustrated, forming Vol. IV. of the "World's Great Explorers and Explorations"; "Home Life on an Ostrich Farm," by Mrs. Annie Martin, illustrated; "The Unknown Horn of Africa, an Exploration from Berbera to the Leopard River," by the late F. L. James, illustrated, new and cheap edition, containing the narrative portion and notes only.

Mr. Fisher Unwin will publish a book on "Gypsy Sorcery and Fortune Telling," by Charles Godfrey Leland, illustrated; a second edition of Mrs. Brightwen's "Wild Nature Won by Kindness," with additional matter; and "Everyday Miracles," by Bedford Pollard, a work designed to present the wonders of science to young readers.

"Methuen's Science Series," edited by Mr. R. Elliot Steel, will include, among other volumes, "The World of Science," "Elementary Light and Sound," "Elementary Electricity and Magnetism," and "Elementary Heat."

Messrs. Charles Griffin and Co. will issue Dr. A. E. Garrod's "Treatise on Rheumatism and Rheumatoid Arthritis"; Dr. A. E. Sansom's "The Diagnosis of Diseases of the Heart"; "Foods and Dietaries: a Manual of Clinical Dietetics," by Dr. R. W. Burnet; "Railway Injuries: with Special Reference to those of the Back and Nervous System," by H. W. Page; "Outlines of Practical Histology," by Prof. W. Stirling, and a second and rewritten edition of "Outlines of Practical Physiology," by the same author; a laboratory course on "Pharmacy and Materia Medica," by W. Elborne; "Scientific Amusements," a variety of experiments illustrating some of the chief physical and chemical properties of surrounding objects, and the effect upon them of light and heat, by Dr. C. R. Alder Wright, F.R.S.; Prof. Roberts-Austen's "Introduction to the Study of Metallurgy"; Dr. C. Le Neve Foster's "A Text-book of Ore and Stone-Mining"; "A Text-book of Coal-Mining," by H. W. Hughes; "Aids in Practical Geology, with a Section on Palæontology," by G. A. J. Cole; "A Text-book of Electro-Metallurgy," by W. G. Macmillan; "The Design of Structures: a Practical Treatise on the Building of Bridges, Roofs, &c.," by S. Anglin; "A Zoological Pocket-book; or, Synopsis of Animal Classification," by Dr. Selenka and J. R. A. Davis; the complete volume of Prof. Jamieson's elementary manual of "Magnetism and Electricity"; a thoroughly revised edition of "Seaton's Manual of Marine

Engineering"; "Sewage Disposal Works: the Construction of Works for the Prevention of Pollution of Rivers and Estuaries," by W. Santo Crimp; and the eighth annual issue of the "Year-Book of Learned and Scientific Societies."

Messrs. Cassell promise "Hygiene and Public Health," by Dr. Arthur Whitelegge; "Medical Hand-Book for Colonists," by E. Alfred Barton; new editions of "Climate and Health Resorts," by Dr. Burney Yeo, and "The Story of the Heavens," by Sir R. S. Ball; "The Art of Cooking by Gas," by Marie Jenny Sugg, illustrated; "Nature's Wonder-Workers: being some Short Life-Histories in the Insect World," by Kate R. Lovell; "Object Lessons from Nature: a First Book of Science," by L. C. Miall; "Commercial Botany of the Nineteenth Century," by J. R. Jackson; two new volumes of "Cassell's Agriculture Series," edited by John Wrightson—"Soils and Manures," by Dr. J. Munro, and "Crops," by Prof. Wrightson; and "The Year-Book of Treatment for 1891: a Critical Review for Practitioners of Medicine and Surgery."

Messrs. Whittaker will publish a new and revised edition of Mr. Gisbert Kapp's "Electric Transmission of Energy"; "Electro-Motors," by S. R. Bottone: "Metal Turning," by the author of "Practical Ironfounding"; a fourth and popular edition of Colonel Findlay's "The Working and Management of an English Railway"; and "A Manual of Wood-Carving," by Charles G. Leland.

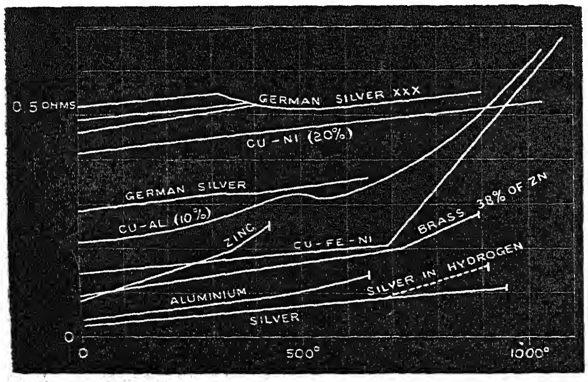
A work on "Animal Life and Intelligence," by Prof. C. Lloyd Morgan, Dean of University College, Bristol, is in the press, and will be published by Mr. Edward Arnold in October.

Mr. Stanford will publish "The Philosophical Basis of Evolution," by Dr. James Croll, and "Through Magic Glasses" by Arabella B. Buckley (Mrs. Fisher). This will be a sequel to the same author's "Fairyland of Science."

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, September 22.—M. Duchartre in the chair.—Account of a scientific expedition to the summit of Mont Blanc, by M. J. Janssen. (See Our Astronomical Column.)—On the modular equation for the transformation of the eleventh order, by Prof. A. Cayley.—On some curious phenomena produced in a current of water, by M. Daniel Colladon. The author presented two photographs taken at Geneva above a river bridge having a grating stretched across its arches. By moving certain of the bars, miniature water-spouts and other phenomena are produced. These forms are conspicuously visible and have been photographed in plan and elevation. The paper contains some observations of their average dimensions.—M. Berthelot announced the death of M. F. Casorati, Professor at the University of Pavia.—Observations of the new minor planet (297) made with the equatorial *coudé* of Algiers Observatory, by M. F. Sy. The observations extend from September 11 to 13.—On the electrical resistance of metals, by M. H. Le Chatelier. The accompanying figure expresses the results obtained with various metals and alloys:—



On the excretory apparatus of some crustacean decapods, by M. Paul Marchal.—Comparative influence of anæsthetics on chlorophyllian assimilation and transpiration, by M. Henry Jumelle. The researches of the author seem to show that anæsthetics increase the transpiration of plants exposed to the light, when sufficient is given to suspend assimilation. This increase of transpiration is evidently due to the action of the ether on the chlorophyll which is exposed to the light, because, from experiments made in the dark, it has been found that the ether acts in an opposite manner on the protoplasm.

Sydney.

Royal Society of New South Wales, June 4.—Dr. Leibius, President, in the chair.—The following papers were read:—A compressed air-flying machine, by L. Hargrave.—On the treatment of slips on the Illawarra Railway at Stanwell Park, by W. Sheilshear.—On native names of some of the runs, &c., in the Lachlan district, by F. B. W. Woolrych.—Remarks on a new plant rich in tannin, by C. Moore.—The following exhibits were shown and described: two new filmy ferns, by C. Moore; the Narraburra meteor found in 1854—specific gravity 7·57, weight 70 lbs. 14 oz., by H. C. Russell, F.R.S. (of which an account was given in Nature of Sept. 25, p. 526).

July 2.—Dr. Leibius, President, in the chair.—Record of hitherto undescribed plants from Arnheim's land, by Baron Ferd. von Mueller, F.R.S.—A new mode of demonstrating the manner in which the mind judges of objects in the outer world, also working models demonstrating the value of the spinal curve in diminishing the evil effects of mechanical violence, by Prof. Anderson Stuart.—Charles, third Earl Stanhope's arithmetical machine, bearing date 1780, also his "demonstrator," an instrument for the performance of logical operations, by Rev. Robert Harley, F.R.S.

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THURSDAY, OCTOBER 9, 1890.

A NEW THEORY FOR THE SENSITIVE PLANT.

Das reizleitende Gewebesystem der Sinnpflanze. By Dr. G. Haberlandt. (Leipzig: W. Engelmann, 1890.)

THE present decade has been a very important one from the point of view of a botanical revival in this country. The seed sown in previous years by Thiselton Dyer and others did not fall entirely on sterile soil, and gradually a school of active workers has arisen, sometimes described, for want of a better name, as "the younger school of botanists." The individuals constituting this "school," though few in numbers, have pursued widely diverging lines of research; some devoting themselves to morphology, others to physiology and minute anatomy, others again to the diseases of plants, &c. Important results have accrued from their labours in the various branches taken up, but in no case have they been more striking than in the field of minute cell anatomy. The readers of this journal hardly need to be reminded that the discovery of the continuity of the protoplasm from cell to cell, and the demonstration of the fact that plant tissues do not consist of a number of isolated masses of protoplasm, cut off from one another by the dead cell-membranes, was largely due to the investigations of Gardiner.

The knowledge of the existence of these uniting filaments seemed from the first to throw light on many intricate and obscure physiological problems. Foremost among these was the possibility that by their instrumentality the transmission of stimuli over considerable tracts might be facilitated. It is interesting to remember that almost the first case of continuity of protoplasm demonstrated by Gardiner was that in the pulvini of the leaves and leaflets of the sensitive plant. Nor did this lose its significance when it was later realized that such a continuity was a very general, if not universal, phenomenon in plant tissues. The view that the stimuli, which undoubtedly travel considerable distances in most of the plants endowed with irritable movements, are transmitted in virtue of these exceedingly fine uniting filaments is one very generally held by botanists in this country, and finds expression in Vines's "Lectures on the Physiology of Plants." Indeed, that this is so, in certain cases, has been experimentally demonstrated. In view of these circumstances, the book whose name heads this review, dealing as it does entirely with the mechanism of stimulus transmission, will be studied with interest, and the more so from the fact that Dr. Haberlandt's "Physiologische Pflanzenanatomie" has done much towards the elucidation of many of the facts of anatomy. Dr. Haberlandt here limits himself solely to the investigation of the means by which a stimulus, set up at some point in the sensitive plant (*Mimosa pudica*), is transmitted to a distance, promoting movements in regions far removed from the point stimulated.

During this century various physiologists have busied themselves with this problem, notably Dutrochet, Pfeffer, and Sachs. The prevalent theory on the Continent, which is associated especially with the name of Pfeffer, briefly amounts to this:—When an irritable portion of one of the pulvini of the sensitive plant is stimulated, the irritable cells lose their turgidity, water passing out of them into the intercellular spaces associated with them; a certain amount of this water is said to enter the tracheides and vessels of the xylem of the vascular bundle, and to upset the hydrostatic equilibrium obtaining there; this disturbance is transmitted to a distance as a wave in the xylem, and stimulates, as it travels along, the irritable cells of the successive pulvini which it passes near, causing them likewise to contract. This may affect merely the pinnules of a single leaf, or, in certain cases, the stimulus may travel from one leaf to another.

This hypothesis is based on very old experiments performed by Dutrochet more than sixty years ago. Dutrochet found that, (1) after the removal of a complete ring or zone of cortical tissue from the stem, a stimulus could still be propagated from one leaf to another, across the decorticated region. A similar result followed when the pith was destroyed, the vascular bundle alone being left intact. The irresistible inference was that the stimulus travelled by the *vascular bundle*. Further, (2) when the woody portion of the bundle was cut into, a drop of liquid was observed to exude immediately, and a stimulus was transmitted upwards and downwards from the point of lesion, causing movements in the nearest leaves and even travelling to more distant ones. The drop which exuded was supposed to come from the wood, and the disturbance of pressure resulting, to initiate the stimulus. Haberlandt deals with this "fundamental experiment," and shows that Dutrochet and the others were in error. To make this clear, it is necessary to briefly indicate the structure of a bundle and adjacent tissues in *Mimosa pudica*. In a transverse section of a stem (and the same holds generally for the petiole) there is externally the epidermis, below which comes the parenchymatous cortex. The cortex passes over into a zone of thick-walled cells, described as collenchyma by Haberlandt, as bast-fibres by some other writers. Within this thick-walled zone is a ring of phloem, and finally the xylems and pith. Dutrochet, when he thought he had dissected away all the tissues outside the xylem in the experiment recorded above (1), had, in point of fact, left not only the phloem, but also the collenchyma-ring intact. His knife had been arrested by the collenchyma, which he had mistaken for the wood. The collenchyma and phloem remained intact, and the inference that the stimulus travelled by the xylem was consequently a false one.

Further, in the case of experiment (2), Haberlandt is at great pains to show that the drops of liquid do not issue from the wood at all, and establishes the fact that they arise in reality from special cells in the phloem. Following the same method of experiment as Dutrochet, Meyen, and Pfeffer, Haberlandt demonstrates clearly—

(1) That the stimulus normally travels inside the collenchyma ring, but outside the xylem of the bundles; in other words, in the phloem.

(2) That, when a stem is cut through, drops exude at the moment of cutting. These drops arise, not from the xylem, but from special cells in the phloem.

This alone marks a considerable advance on the older hypothesis.

Having cleared the ground so far, Haberlandt sets himself two problems for solution, and it is with these that the greater portion of his paper is occupied. These questions are:—

(1) What are the special organs in the phloem which transmit the stimulus?

(2) What are the details of the mechanism of transmission?

As his later inferences and experiments are based on a detailed anatomical study of the sensitive plant, it will be advisable to follow Haberlandt into this matter. The phloem of many Leguminosæ is characterized by the possession of rows of cells, somewhat larger than the true sieve-tubes, which, from the nature of their contents, are known as tannin-sacs. These, in *Mimosa pudica*, are long, tube-like cells, arranged end to end. Each cell possesses a primordial utricle and a large nucleus. The longitudinal walls of these cells are frequently pitted, but the structure of their transverse walls is characteristic. Each possesses a very large, shallow pit. The closing-membrane of the pit is traversed by a number of very delicate protoplasmic filaments. These are much finer than the similar connecting filaments of sieve-tubes, and approximate more nearly to the uniting filaments of adjacent parenchyma cells. Haberlandt shows that it is a portion of the watery content of *these* cells which escapes, when a stem or petiole is cut into; and it is his view that these cells constitute the organs which transmit the stimuli from one point to another in the plant. The watery fluid which exudes from them, usually clear and colourless, gives a very characteristic, and deep reddish-violet colour, with iron chloride, and, if allowed to dry upon a slide, crystallizes out in various forms, usually arranged as sphere crystals or dendritically. This substance is probably of the nature of a glucoside, since, among other reactions, when treated with acids it is broken up into glucose and a resinous body. Accompanying this glucoside is a considerable amount of mucilage.

The distribution of the vascular bundles, in especial relation to these supposed conducting cells, both in leaf and stem, is followed out in detail. The glucoside-containing cells occur, roughly speaking, in two rings in the phloem, one of which is nearer the collenchyma zone, the other nearer the xylem. Some of the former actually touch the collenchyma cells. Where the bundles traverse the pulvini, a much larger proportion of these cells are in contact with the collenchyma. In the leaflets all the larger bundles are accompanied by the glucoside-containing cells, but in the very small anastomoses they die out. Finally, as to the distribution of protoplasmic continuity. This obtains in the soft cortex and in the collenchyma (whose cells are freely pitted). The cells of these two tissue systems are united together by extremely fine filaments in such a manner that an unbroken protoplasmic continuity exists, from the periphery of the pulvinus to the inmost layer of the collenchyma. In the phloem also there exists a similar continuity. Between these two systems, however—the soft cortex and collenchyma on the one hand, and the phloem (including the glucoside cells) on the other—there is, according to Haberlandt, no direct continuity; and although the collenchyma cells are freely pitted on all sides, the closing-membranes of these pits are untraversed by protoplasmic filaments on the side directed towards the phloem (and glucoside cells).

By careful experiments, referred to above, Haberlandt demonstrates (what had been regarded by several observers as probable) that the stimulus travels in the phloem; and in view of the fact that the glucoside sacs emit the drops of liquid on cutting a stem or petiole (thus giving rise to a hydrostatic disturbance), and in view also of inferences drawn from further experiments, to be considered immediately, Haberlandt concludes that the stimuli are transmitted by the rows of glucoside-containing sacs, and this in a purely mechanical manner. Before going on to elaborate his theory, Haberlandt meets and disposes of the hypothesis, mentioned at the commencement of this review, that the stimuli travel from cell to cell by the agency of the uniting filaments of living protoplasm. According to that view, when any pulvinus is stimulated, as by a mechanical shock, its irritable cells contract, losing their turgidity, and a movement results; at the same time the stimulus would be conveyed to the phloem, and there transmitted from cell to cell by the filaments of protoplasm (it is immaterial whether in the phloem-parenchyma, or even in the longitudinally-running series of glucoside sacs) until it reaches another pulvinus, where it would be communicated in the same way to the irritable cells there, and a further movement would result, and so on.

It is necessary at this juncture to explain, so as to make what follows intelligible, that physiologists have availed themselves of two entirely different methods of stimulating the plant—firstly, by submitting a pulvinus to a mechanical shock, without damage to its tissues; and secondly, by cutting the petiole or stem, and causing actual lesion of the conducting tissues. In both cases the stimulus is transmitted, but in the latter to a much greater distance, the method being altogether a more violent one and perhaps quite different in its effects. Pfeffer was content to regard anything of the nature of a "vital" hypothesis of stimulus-transmission as disproved, from the fact that, even when he chloroformed a definite portion of a petiole, a stimulus could still be transmitted through the region subjected to the chloroform. Haberlandt, however, points out that this result must not be taken as final, since there is no proof that the internal tissues had been really acted on by the anæsthetic, as applied by Pfeffer. Further, Vines has pointed out that, although chloroform deprives the *irritable cells of the pulvinus* of their irritability (rendering them rigid), there is no justification for the assumption that it likewise deprives the protoplasm of the *conducting cells* of their conductivity; an objection the validity of which is admitted by Haberlandt. It was necessary, therefore, to make a more crucial experiment to decide this point, and this Haberlandt does by substituting an *actual killing* of the protoplasm in a small portion of a petiole for a mere chloroforming. This was done by steaming a confined zone of a petiole. Under these conditions, the stimulus, to be transmitted successfully, had to pass through a dead region. If the stimulus could be shown to traverse this region, the "vital" hypothesis would be untenable. The

result was most instructive. When an ordinary mechanical stimulus was applied, there was no transmission; when, however, a stimulus, caused by a lesion of the tissues, was applied, it was transmitted, in the majority of cases, over the zone previously killed by steam. From this, Haberlandt concludes that no theory which depends on filaments of protoplasm to conduct the stimulus can be maintained. He considers it improbable that a mechanical stimulus and one due to lesion should travel in different ways, notwithstanding the fact, which he mentions later on, that under special conditions a stimulus can be transmitted even by the vessels and tracheides of the xylem. There is nothing surprising in the transmission over a dead region of a stimulus due to lesion. It is just this sort of stimulus that would cause a considerable upset in the hydrostatic equilibrium in the glucoside cells cut into, and it is conceivable that the disturbance due to this sudden fall in pressure might be conveyed, in a purely mechanical manner, over considerable distances. Hitherto it has not been shown that a *normal mechanical stimulus* can be transmitted over a zone that has been rendered incontestably dead.

If the distribution of the continuity of protoplasm in a pulvinus should be, as stated by Haberlandt, such that the outer system of continuous cells extends inwards only so far as, and including, the collenchyma, and that the phloem is independent of this system, then it is difficult to see how the stimulus could pass (by a vital hypothesis) from the irritable and contractile cells to the conducting cells. This result, however, requires confirmation. Gardiner, who investigated the nature of the continuity in these organs of the sensitive plant, makes no comment on any such marked discontinuity of the protoplasm, and the inference, drawn from a study of his paper in the Philosophical Transactions, is that the cells, from the periphery right up to dead vessels of the wood, constitute one connected whole. What he denies to the sensitive plant, Haberlandt admits for other cases. The phenomenon of transmission of stimuli in the stigmas of *Mimulus* and *Martynia*, in the leaves of *Dionœa* and probably of *Drosera*, and in the tendrils of many climbing plants, would seem to be a function of the protoplasmic fibrils. Having dealt with his refutation of the "vital" hypothesis, as applied to the sensitive plant, we may give a short summary of the theory put forward by Haberlandt, though for its details, and the many questions raised therein, the reader is referred to the original text. Haberlandt holds that both in the case of a mechanical stimulus, and in that of one caused by a lesion of the tissues, the transmission is effected in a purely mechanical manner, as a wave or impulse passing along the glucoside-containing cells. Necessarily the transverse walls, which possess each a broad, shallow pit, are regarded as offering little resistance to the filtration of the contained sap. The protoplasm which is continuous through these pit-membranes is not regarded as playing any important part in this event. When a pulvinus is mechanically stimulated, its irritable cells lose water and contract; the disturbance set up by this fluctuation in pressure will start a wave in the rows of conducting cells, travelling from the point at which the increase of pressure occurs. The wave is what may be described as a positive wave (*Bergwelle*), and the method of its transmission is similar to that obtaining in a closed rubber tube distended with water, when it is pinched at one end. This wave, when it reaches the irritable cells of the next pulvinus, will be communicated to them through the elastic collenchyma layer, probably through the pits, which are numerous. In more sluggish cases it may not, perhaps be till an actual *bending* occurs in the stimulated pulvinus that the increase of pressure will be sufficient to start the wave in the conducting cells.

On the other hand, when the stimulus is due to lesion, as when a petiole or internode is cut into with a sharp knife, the wave is set up in a different manner. At the moment of cutting into the turgid, glucoside-containing cells, a drop of liquid escapes, causing a *fall* in pressure. This is transmitted as a negative wave (*Thalwelle*), and will be communicated to the irritable cells at a distance, by the agency of the pits there. In this case, however, the pit-closing membranes of the collenchyma will bulge slightly inwards, in the former case outwards. The communication from the conducting to the contractile cells is rendered easier from the fact that in the pulvinus a much larger proportion of the former lie adjacent to the collenchyma than at other points on the course of the bundle. This special arrangement undoubtedly seems to favour such a theory as that of Haberlandt. Still it must be remembered that it may be due to quite different causes. As is well known, the arrangement of the bundles is always considerably modified at the pulvini, so that the bending may be interfered with as little as possible.

In conclusion, it must be admitted that the paper here reviewed is a masterly piece of work, though, it may be, many naturalists in this country will hardly agree with the author in the inferences which he draws, and the theory which he builds upon them. It must be borne in mind that possibly too important a part may have been assigned to the uniting fibrils of protoplasm as touching the transfer of stimuli of various kinds from cell to cell. In the first blush of a great discovery, of so far-reaching a nature as that of the continuity of protoplasm, and more especially from the fact that at a very early period it was in the sensitive pulvini that this continuity was shown to exist,—in view of this, the position that has been taken up by workers in this country may have been an over-sanguine one. It may be that the explanation of this phenomenon of protoplasmic continuity (though undoubtedly it facilitates transmission of stimuli in certain cases, *e.g.* stigmas of *Mimulus*, &c.) may have another bearing—that it may in some way affect the nutrition of the pit-closing membrane, or even discharge the purely mechanical function of binding the protoplasts to the closing membranes. For the present, although it must be conceded that Haberlandt has considerably advanced our knowledge of this question, in that he has localized the conducting region to the phloem, and has shown that stimuli due to actual lesion can be transmitted in a purely mechanical manner; nevertheless he has failed to demonstrate conclusively the untenability of the "vital" hypothesis in the case of the normal stimulus (*Stossreiz*). This being so, further results must be awaited before this interesting question can be regarded as finally settled.

F. W. O.

CHRISTY'S "BIRDS OF ESSEX."

The Birds of Essex: a Contribution to the Natural History of the County. By Miller Christy, F.L.S., 8vo. (Chelmsford and London: 1890.)

"HITHERTO," truly observes the author of this work in his Preface, "the birds of Essex have not found a chronicler. It is to supply this omission that I have laboured." The omission has indeed been long regretted, and every page of his book shows that Mr. Christy has laboured hard to supply it, so much so that it would seem an act as ungenerous as it is certainly unpleasing to find any fault with him for the way in which he has performed his task; but the duty of a reviewer is one neither to be lightly entered upon nor lightly executed, and misplaced tenderness may easily be as harmful in a critic as in a surgeon.

There is fortunately in these days no need to dwell on the advantage of county ornithologies—even the worst of them is better than none at all. Mr. Christy's is very far from being among those that are bad; but it does seem to us that more skilful treatment would have secured for Essex a less insipid result than he has given us; for the county of the greatest English naturalist should surely present a more becoming figure than here appears. Its geographical position, its sufficiently varied natural features—and among them especially its wealth of estuaries, so grateful to scores of graceful birds—seem to point it out as one of the most favoured parts of the kingdom. We can hardly admit the value of Mr. Christy's supposition (pp. 2, 3) that—

> "If only our illustrious Ray had made some attempt to produce a list of local birds, similar to that of his contemporary, Sir Thomas Browne, there is no saying how many practical Essex ornithologists it might indirectly have brought out, or to what a pitch of ornithological eminence the county might by this time have been raised."

When will naturalists think the history of their study worth studying? Nothing can be more certain than that the now celebrated "Account of Birds found in Norfolk," by Sir Thomas Browne, remained in manuscript until printed by Wilkin in 1835; and, while very few could have been aware of its existence, fewer still could have read its crabbed handwriting. As a matter of fact, we believe it was unknown to every ornithologist until it was published. On the other hand, Mr. Christy shows that almost from Ray's time to the present day Essex has not been wanting in observers of birds, who really seem to have had it in them to do as much good work as those of the not very distant and more northern county, whom he evidently and not unjustly regards with a kind of modified envy. But our author may get some comfort by looking southward across the wide Thames, and there contemplating in a still nearer neighbour a county whose ornithology, as we have before remarked in these columns, is yet unwritten as it should be.

Mr. Christy rightly remarks that "some detailed attempt" to describe the physical features of every county or district should be an essential part of each local "avifauna," but he unfortunately favours us with barely three pages of such description. Now we are sure that he might have told us a good deal more on this subject that would have been well worth knowing. He divides his county into *five* districts, which is doubtless well enough, if we can forgive the incongruity of the last:—(1) The Chalky Uplands, (2) The Lowlands, (3) The Forests and Woodlands, (4) The Marshes and Saltings, (5) The Open Sea! The first of these, a very small but well-defined area, has probably, through enclosure and tillage, undergone more change within the last 70 or 80 years—or even less—than any of the rest; for the second has been highly cultivated for centuries, and the third—though Mr. Christy thinks that strictly speaking it cannot be separated from the second—in some sort possesses the appearance it must have worn (if not the fauna it harboured) in the middle ages when, if we may believe the chroniclers, the citizens of London went forth to slay wild bulls and wild boars within its precincts—a trace of the practice being retained in the "Epping Hunt" of Easter Monday, which some of us are old enough to remember, and others may be reminded of by Hood's comic verses. But the fourth of Mr. Christy's districts may be considered the most characteristic of Essex, and we think he might advantageously have told us much more about it, especially about the islands—if islands, except in popular speech, they may be rightly called—into which the land, as it were, breaks up—Canvey, Foulness (a most suggestive name), Osea, Mersea, and others. One would think they cannot be all alike, and would like to know wherein they differ. The same may be said of the rivers—and the rivers of Essex are rather fine things in their way; the many-mouthed Crouch is not exactly similar to the spacious Blackwater, any more than is the narrow Colne (in happier times glorying in its abundance of "natives") to the lake-like Stour, which the county shares with Suffolk. As to the fifth district, only a technical objection could be raised, and that would be against the use of the epithet "open." A maritime county must have a sea-border, and it stands to reason that a fair portion of the adjacent salt water should be regarded as *adscriptus glebæ*, but no attempt is made to define that portion. Considering the shallow soundings off the Essex coast, perhaps the political "three mile limit" might be the best to choose; but again, considering the paucity of bird-life in the summer time in this narrow sea, and the knowledge that in winter one part of it is nearly as good as another, this does not much matter, and if Mr. Christy would extend his survey to the halfway line between England and Belgium, there is none to take exception thereto. In truth Essex, owing to its want of cliffs—for there is nothing save near Walton-on-the-Naze entitled to be so-called—and of beaches, such as those of Orford or Dungeness possessed by its neighbours, has nowadays nothing except the Little Tern,[1] that can be called peculiarly a shore-bird, for he properly denies (pp. 100, 101) the claim set up by the late Dr. Bree for the "Mud-lark" (*Anthus obscurus*), and, as all ought to know, the Ringed Plover will breed far inland; but we think he has missed an opportunity in not applying to the Migration Com-

[1] There can be hardly a doubt that the Common and perhaps the Sandwich Tern bred formerly on the Essex coast, but everyone knows how easily a settlement of either may be destroyed in a few hours by some heedless person who thinks himself a sportsman or a naturalist—so that all around our shores both species are being yearly extirpated from spot after spot. Mr. Christy's evidence (pp. 261, 262) as to the Black Tern, not that it is littoral species, breeding in the county, amounts to nothing if properly scrutinized.

mittee of the British Association for the schedules filled up by the light-keepers on the coast of his county. The necessary documents would doubtless have been readily placed at his disposal, and comparatively meagre as the results might have been, they would certainly have enabled him to give a considerable amount of additional information.

Like most of his fellows, Mr. Christy attaches an undue value to the number of species he is able to register, and his number is 272. We have often wished there would arise some strong-minded man who would clearly distinguish between a member of a fauna and a fortuitous straggler. Still, we gladly allow our present author to be more discriminative than many of his brethren, and highly applaud his exclusion of several species which they, or some of them, welcome—yet he more than once bows the knee to the prevailing Baal. It is bad enough for any British faunist to admit one species of Sooty Tern, especially when the alleged single specimen rests on authority not quite beyond suspicion, and, though not ten years have elapsed, has been "entirely lost sight of"; but the inclusion of a second species dulls one's feelings, like an anæsthetic—especially when we are told that in this case it is the captor "who has since been lost sight of" (p. 261), though the specimen is (apparently) to the fore. Then, again, what can be more absurd than the admission of *Porphyrio smaragdonotus* (p. 225)—a species of which living examples are yearly imported in great numbers, and one that possesses faculties of escaping from confinement that would have been envied by a Casanova or a Baron Trenck.

It may be urged that we have picked out trifling faults, but we could reply that we have purposely chosen these instances to show two at least of the failings of faunists. Others we might specify of a rather different kind. It is a remarkable fact for ornithologists in general that the Needle-tailed Swift should have flown across the Old Continent from Eastern Siberia to Essex, but that fact does not make it a "British" bird, and the late Mr. Yarrell—generally too prone to naturalize all stragglers—was in our opinion perfectly justified in refusing it a place in his well-known work, while even the subsequent occurrence of two examples in Hampshire does not affect his rejection of it. As regards the inclusion of "stragglers," the line is in many cases hard to draw, but in one such as this there ought to be no doubt in the mind of anybody who has a decent acquaintance with the geographical distribution of animals.

The present work differs of all others of its kind in two respects, and one of them is deserving of much praise. This is the useful "Biographical Notices of the principal Essex Ornithologists," which are greatly to the point, and generally, as appears to us, well done, though Mr. Christy is somewhat lavish of the expression "excellent ornithologist." That would doubtless be applicable to John Ray, who is not included, but in its literal sense to few if any others. Yet men like Sheppard, Hoy, and Henry Doubleday were worthies who left their mark on British natural history, and fully merit all that is said of them, while Christopher Parsons seems to have been one of those diligent observers who delight in hiding their candle under a bushel, and we feel under an obligation to Mr. Christy for bringing him out of obscurity. Of the other distinctive feature of the work we cannot report so favourably. It is the needless introduction of a considerable number of figures representing the birds mentioned. Some of them, it is true, are reproductions of Bewick's well-known woodcuts, and therefore right in the main, however poor the imitation. Next, if not equal to them, are the few drawn by Mr. Wolf; but the adaptations of the engravings from Yarrell's work, if they cannot be called absolutely bad, are objects about as disagreeable as one ordinarily encounters, and there are others, the source of which we cannot divine, that make one shudder, for the draughtsman has evidently copied too faithfully (as the manner is nowadays) the distortions of the bird-stuffer—as witness the figure of the Swift (p. 144), which reminds one forcibly of the impossible tenants of the air in the familiar willow-pattern plate.

To sum up, let us say that with all its shortcomings Mr. Christy's book is one that must demand the attention of every British ornithologist, for it "means business." There is no attempt at fine writing in it, and yet its composition has clearly been a labour of love to the author. We trust he may be rewarded by a successful sale, which the populous county of Essex ought to insure, and be able to bring out a new edition. If so, let him eschew his woodcuts, and in their place give us more large type.

HYPNOTISM.

Hypnotism. By Albert Moll, of Berlin. "Contemporary Science Series." Edited by Havelock Ellis. (London: Walter Scott, 1890.)

THIS book by Dr. Albert Moll, a physician of Berlin, on hypnotism, now presented to us in a becoming English dress, marks a step of some importance in the study of some difficult physiological and psychological problems which have not received much attention in the scientific world of England. The appearance of a text-book on any subject in a set of hand-books such as the "Contemporary Science Series," indicates a general agreement on the main points of knowledge, and in this case a full admissal of the subject to the category of recognized science. Dr. Moll's work has already been widely accepted as a text-book in the German schools which are beginning to take some interest in his subject. The first edition was published hardly eighteen months ago, and was very rapidly exhausted; the second, from which this English translation has been made, shows good proof of the diligence and care of the author, in the large amount of new matter incorporated with the old, so that on the whole it is well up to date, a matter not so easy to accomplish in treating of a rapidly growing subject such as hypnotism, on which nowadays there are published some 300 books, pamphlets, and articles every year. There has often been in this crowd of minor literature of late years a tone of somewhat indignant, sometimes injured self-assertion, such as is not unnatural to the friends of a young branch of knowledge, who are anxious and perhaps over-anxious to establish its position on equal terms with its seniors. But Dr. Moll's hand-book embodies an essentially non-combative survey of the full breadth of the subject, including both the details of the physical and physiological conditions of hypnotism on the one side, and on the other the alterations of personality and the more delicate points of

psychological interest; and it does not refuse to consider with some careful attention the phenomena of telepathy and thought transference in a hypnotic state, such as are judged by Charles Richet, Gurney, Pierre Janet, Oliver Lodge, and others to take place under conditions which render their explanation by the action of the known senses at present inadequate.

Such a comprehensive review of the present position is in need of a far more extensive and careful historical preface than is usually undertaken; and in this respect Dr. Moll has shown his sense of his responsibilities, and gone much beyond a mere reproduction of the hackneyed account of many of the French writers. He sees that the phenomena generally called mesmeric did not entirely originate with Mesmer (1734–1815), about a century ago; but can be in part traced back to some of the earlier civilizations (cf. Ebers's papyrus, 16th century B.C.), and assumed some of their more modern forms under Paracelsus (1493–1541) and van Helmont (1577–1644), although, of course, their recent growth has been far more rapid. Mesmer is a man hard to estimate rightly. The final account of him has probably not yet been written, nor the final judgment passed, but Dr. Moll furnishes a sketch of some discrimination, based chiefly on contemporary evidence, and showing some sympathy for the mental and moral bewilderment occasioned by the chaos of the Great Revolution with which he was surrounded in Paris. Since Mesmer, he realizes the steps in advance made by Braid (1843), in recognizing the physiological and psychological importance of a state of attention in what he called no longer mesmerism or animal magnetism, but hypnotism; by Esdaile in 1845, in demonstrating the complete anæsthesia that was made possible by hypnotism, even in major surgical operations; by Liébeault (1866), in showing the practical use of post-hypnotic suggestion to dominate at least some morbid habits and minor pains; and by the schools both of the Salpêtrière under Charcot (1878) and of Nancy under Bernheim (1884), in proving to the general public the permanent importance of a deeper study of the subject.

The survey of the methods of induction and the symptoms of hypnotism is founded on much personal experiment and a wide experience in all the European nations. More than 600 authors are quoted, and more than half of these are contemporary. Though fully half of them come from France and Germany together, yet there is a very considerable total of English, Swiss, Austrians, Americans, Italians, Spaniards, Belgians, Dutch, Swedes, Danes, Greeks, and Russians. Hypnotism is certainly not a limited or local fancy. Last year the four days' discussion of hypnotism under its various aspects at the Congrès International de Psychologie Physiologique, in Paris, by such a large gathering of men from all parts of Europe, not interested merely in the medical side of the subject, but in its total results and their relation to other branches of knowledge, gave very tangible proof of this, as may be seen in their *Compte rendu*. And who are the easiest persons to hypnotize? It is very common to find those who have had little or no experience themselves confident that they can point out the most amenable subjects, and choosing generally persons with some obvious weakness. But Dr. Moll shows that neither neurasthenia nor hysteria, nor weakness of will, nor any of the sentimental weaknesses that may be made to render their subjects laughable, really conduce to making them more readily hypnotizable. Hysterical people may be morbidly imitative, and if one is hypnotized many may follow the example, if it is open to their observation; but taken singly, their hysterical tendencies rather hinder than help their hypnotization (p. 316), a point which has unfortunately been rather obscured by the long and important experiments made by Charcot on the hysterics only; for, from the success of many of these, it was hastily and incorrectly assumed, before wider trial, that this class of subject was the most easily influenced. Whether there can be any special capacity in the hypnotizer is a point Dr. Moll does not discuss in detail; he assumes that all of fairly good intelligence are about equal after a little practice at the *technique*. But there are some cases which he mentions (p. 363), of hypnotism at a distance by Dr. Gibert and Pierre Janet, in which, when both the persons hypnotizing and the times of hypnotism were unknown to the subjects, certain persons proved pre-eminently successful (*Bulletins de la Société de Psychologie Physiologique*, 1886, p. 78). The proof or disproof of individual qualifications is, in fact, one of the many difficult points for the settlement of which a wide and very careful experimental research is still necessary. The mesmerists of the early part of the century can be shown to have laid too much stress upon it; it may be that it is too much overlooked now.

Any exact definition of hypnotism, as of other abnormal states of consciousness, is difficult enough, as Dr. Moll very readily acknowledges. "The two characteristics of hypnosis are suggestibility and the power of ending the state at pleasure" (p. 208), he observes, in agreement with most others who have considered the subject; but it is not plain how this is consistent with what he has said just before (p. 201), viz. that "to my mind the dividing line between sleep and hypnosis is merely a quantitative difference in the movements." The mental susceptibility would have seemed to us a more important point of variance. But we are glad to say that, on the more difficult points of theory, Dr. Moll promises us another book at some future date, and it seems wise to allow some considerable time for the collection and attestation of the phenomena before attempting the establishment of theory in matters of such traditional difficulty.

That the practice of hypnotism is very useful in the healing art Dr. Moll is convinced, and offers a good deal of technical medical evidence which it would be hardly appropriate to consider here. The power of post-hypnotic suggestion in checking habits of drunkenness, &c., is one which is just beginning to be confirmed from various quarters, and which opens a wide vista. The possible dangers which arise from the hypnotist's power over the patient's conduct need very careful attention, though Dr. Moll is inclined to point with satisfaction to the very few cases in which any injury has been actually done. We could have wished that he had made plainer that most important preventive practice of Liébeault's, viz. that those who are afraid of the dominion of any hypnotist can be and should be protected against it by hypnotization under other trustworthy hands, and by the suggestion that no one can have any hypnotic or post-hypnotic power over them. In his last chapter on

animal magnetism, Dr. Moll practically admits an unexplained residuum of facts, and in the candid temper of his whole book, shows a truly scientific spirit of genuine interest in their investigation.

A. T. MYERS.

OUR BOOK SHELF.

Text-book of Mechanics. By Thomas Wallace Wright. (New York: D. Van Nostrand Company, 1890.)

THIS book is a most excellent treatise on the science of mechanics, and systematically places before the student the principles which underlie the subject. The differential calculus for the most part is used only when a clear advantage is gained by it, and in the earlier chapters of the work two courses are open to the reader, one with and the other without it. The author in a note rather regrets that words for the unit velocity and unit acceleration have not been proposed, as these would simplify matters: the Rev. J. B. Lock, in his late book on "Dynamics for Beginners," has proposed and used two very good words, "velo" and "celo," for unit velocity and unit acceleration respectively.

On the whole, the practical parts are treated more fully than is usual, and the examples throughout are of a very practical and typical character, and not mere numerical illustrations of formulæ. Many examples the author has treated by the graphical method of solution, but he adds a word of warning to the student against making it a complicated weapon for attacking all sorts of problems which are more easily solved in other ways.

Another important point alluded to is the use of approximate formulæ: the rigorous formula always precedes the approximate one, the latter being reduced from the former, so that the degree of approximation can easily be estimated.

The last two chapters deal with the statics and kinetics of fluids, or, as they are more generally known, hydrostatics and hydrokinetics.

Besides numerous examples there are plenty of figures and woodcuts, and scattered here and there are a few historical notes which give a lively interest to the subject.

W.

An Elementary Text-book of Heat and Light. By R. Wallace Stewart, B.Sc. London. (London: W. B. Clive and Co., 1890.)

THIS volume is one of the University Correspondence College Tutorial Series, which are written specially to meet the requirements of the various London University examinations.

Of the twenty chapters, the first ten deal with the principles which underlie the theory of heat, while the second ten treat of those of light. In each chapter the principles and laws on which the subject-matter depends are fully described, and under the heading of "Calculations" the author explains the various laws in mathematical form, concluding with examples worked out, and in many cases questions from well-known examinations. The chapters on light are treated in a similar manner. Those reading this work should be able to obtain a fair grip of these two subjects, the elementary principles being well and concisely expressed.

At the beginning of the second part of the book, on "Light," the author recommences the numbering of the pages, which we think is rather a mistake, as it is awkward in the first instance for reference, and in the second it has necessitated the use of two indexes. The illustrations, one hundred and fifty in number, are very good and accurate, and the work concludes with an appendix containing a paper of questions set at the London Matriculation examinations under the new 1888 regulations.

W.

The Confessions of a Poacher. Edited by John Watson, F.L.S. (London: The Leadenhall Press, 1890.)

IN an editorial note it is stated that the poacher of these "Confessions" is "no imaginary being." Since that is so, it might have been well for Mr. Watson to explain the precise nature of his own functions as editor. It seems rather odd to find a poacher talking in this way:—"It was the fact that I had, during the small hours of the morning, stood alone on London Bridge. The great artery of life was still; the pulse of the city had ceased to beat. Although bred among the lonely hills, I felt for the first time that this was to be alone; that this was solitude. I felt such a sense as Macaulay's New Zealander may experience when he sits upon the ruins of the same stupendous structure." How much of this is the poacher's, and how much are we to attribute to the editor? The same question often suggests itself, and a good many readers, we suspect, will conclude that at least with the form of the "Confessions" the person supposed to be confessing has had very little to do. The book displays a curious and intimate acquaintance with some forms of animal life, and may be of service in fostering a liking for natural history. Unfortunately, however, grammatical rules are not always treated with the respect which is due to them. Says the poacher: "Whilst preparing my nets and wires, the dogs would whine impatiently to be gone." No doubt the poacher here means that he himself prepared his nets and wires, but what he says is that the dogs prepared them.

Examination Papers in Trigonometry. By George H. Ward, M.A. (London: George Bell and Sons, 1890.)

ONE hundred and twenty examination papers are given in this book; they are arranged progressively and seem to be well chosen, and will be found good substitutes for the questions in the various text-books which become familiar to the student on his second reading. Questions solely on book-work collected together at the end form a useful addition.

Blackie's Modern Cyclopædia. Vol. VII. Edited by Charles Annandale, M.A., LL.D. (London: Blackie and Son, Limited, 1890.)

THIS is the seventh volume of this useful and valuable essence of information, commencing with the word "Potamogeton" and reaching as far as "Skates." The articles on the various subjects are generally well treated, and every reader may find something of interest in them; numerous pictorial illustrations and maps are given. The references to printing, Prussia, railways, Rome, Russia, and Scotland are among the most lengthy in this volume.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Recent Classification of the Shrews.

DURING the present year some very important work has been done with the *Soricidæ*, the family of the Shrews. This has been mainly contributed by the well-known student of the group, Dr. George E. Dobson, F.R.S., the distinguished mammalogist. Dr. Dobson has just published the first fasciculus of Part III. of his work entitled "A Monograph of the Insectivora, Systematic and Anatomical" (Gurney and Jackson, London). This fasciculus deals entirely with the Shrews, it being a quarto illustrated by six fine lithographic plates fully illustrating the dentition of the *Soricidæ*, as its text, in the most admirable manner, presents their characters. Even a still more important paper by the same author appeared in the Proceedings of the

Zoological Society of London for February of the present year, and is entitled "A Synopsis of the Genera of the Family *Soricidæ*." Probably the most extensive collection of these interesting little mammals ever examined by a single investigator, came under the hand of the writer of the works just quoted, wherefrom to make his deductions. His classification is most complete and acceptable, and goes to show that the Shrews are first to be divided into two sub-families, viz. the *Soricidæ*, and the *Crocidurinæ*, the first being characterized by having their teeth red-tipped, while in the latter the teeth are white. Five genera make up the first sub-family—which stand, *Sorex, Soriculus, Blarina, Notiosorex*, and *Crossopus*. In the *Crocidurinæ* we find six genera—namely, *Myosorex, Crocidura, Diplomesodon, Anurosorex, Chimarrogale*, and *Nectogale*. This adds four genera to M. Milne-Edwards's list, and from the same omits the genus *Neosorex*. Dr. Dobson believes that "the red-toothed Shrews diverged from the white-toothed, development having proceeded on somewhat similar lines in the descendants of both according to similarity of environment and modes of life." Of Dr. Merriam's genus and type, *Atophyrax bendirii*, he says that "there are no leading characters which would enable one to define the genus, were I inclined to admit it in my synopsis."

It is refreshing in these days to meet with such classification, and such an able classifier—one who, as Dr. Dobson most emphatically does, draws good strong lines in taxonomy, and discourages the hair-splitting methods adopted by some mammalogists in these days. R. W. SHUFELDT.

Takoma Park, D.C., September 13.

Musical Sands.

In reference to the note respecting Mr. Hyndman's query *re* sonorous sand (NATURE, October 2, p. 554) it may be interesting to him, and others, to know that in our own islands musical sand is by no means rare. In the second edition of my "Musical Sand," shortly to be issued, I shall give a list of the places at which it occurs in England, Scotland, Ireland, and Wales, showing that only *observers* are rare—not the sands.

My investigations since my paper was first published nearly two years ago have brought many new and interesting facts under my notice, not the least being that the musical sands at Studland Bay are always mute during an easterly wind. This I have been able to account for.

About three years ago I propounded a theory to account for the emission of these musical sounds from sands; briefly it is that they are the result of the rubbing together of millions of clean sand-grains very uniform in size: two such grains rubbing together would not produce vibrations audible to us, but the *accumulation* of such vibrations issuing from millions of surfaces, and, approximately, of equal length, would produce a note sufficiently powerful to be sensible to us.

This theory has long been published, and though it has been examined by some of our most eminent physicists, and tested in a variety of ways since, nothing has been suggested which has caused me to abandon it. I shall be pleased to send Mr. Hyndman a copy of my first paper on the subject.

Bournemouth, October 6. CECIL CARUS-WILSON.

With what Four Weights (and a Pair of Scales) can be Weighed any Number of Pounds from 1 to 40 inclusive?

With two weights four amounts can be weighed, viz. each weight and the sum and difference of the two.

With a third, in addition to these four, the sum and difference of each and the third can be weighed. Three weights therefore give 13 amounts. Similarly a fourth weight gives $13 + 2 \times 13 + 1$, or 40 amounts, exactly.

It is therefore evident that each amount must be arrived at by only one combination, and that the sum of the weights must be 40 pounds. To weigh 39 pounds, then, we shall clearly want a 1 pound weight. With 1 and 39 we can weigh 1, 38, 39, 40. For the next weight 2 clearly will not do, as 1 could be arrived at in two ways. Taking 3, we find that 1, 3, and 36 give us 1, 2, 3, 4, 32, 33, 34, 35, 36, 37, 38, 39, 40. Now to get 5 without getting any amount by more than one combination we clearly want 9, and this will be found to solve the question, the weights being 1, 3, 3^2, 3^3. A fifth weight of 3^4 will enable us to weigh any number of pounds up to 121, and so on.

E. R. F.

Protective Coloration of Eggs.

In view of Mr. Grensted's letter to NATURE last year (vol. xli. p. 53), asserting the writer's belief that the egg of the red-backed shrike varies with the tint of the lining material of the nest, and of my own reply to this (same volume, pp. 129–30), I had intended this summer to examine as large a series of nests and eggs as possible, in order to verify or disprove my former observations. I have, however, been unable to devote any time to the matter; and have only obtained two nests—both from the neighbourhood of Evesham. In each of these, I must confess that Mr. Grensted's contention is borne out. The lining of one nest is dull brown in colour; and the eggs (5) are of a mouldy-brown ground-colour, tending towards dull green. The lining of the second is brighter in tone; and contains a small fragment of red flannel. The eggs (5) of this nest show the commoner flesh-coloured ground.

In spite of these two instances, I must hold to my former opinion, that the correlation of ground-colour and environment is very imperfect in the nests and eggs of these birds. Next year I hope to be able to examine a greater number of nests.

E. B. TITCHENER.

Mote House, Mote Road, Maidstone, October 2.

LUNAR PHOTOGRAPHY.

THE idea of employing the process invented by Daguerre and Niépce for the purpose of obtaining photographs of our satellite was first suggested by Arago in a report made to the Paris Academy of Sciences on August 19, 1839. Daguerre acted on the suggestion, but, in spite of a long exposure, he obtained only feeble impressions, in which all details were conspicuously absent (Arago, "Œuvres," vol. vii. p. 458). The first photographic representations of the moon may therefore truly be said to have been made by Dr. J. W. Draper in America by means of a Newtonian reflector of five inches aperture. The specimens were presented to the New York Lyceum of Natural History. The following is an extract from the minutes of that association:

"March 23, 1840. Dr. Draper announced that he had succeeded in getting a representation of the moon's surface by the daguerreotype. . . . The time occupied was 20 minutes, and the size of the figure about 1 inch in diameter."

Dr. Draper also wrote in September of the same year:—

"There is no difficulty in procuring impressions of the moon by the daguerreotype beyond that which arises from her motion. By the aid of a lens and a heliostat, I caused the moon-beams to converge upon a plate, the lens being three inches in diameter. In half an hour a very strong impression was obtained. With another arrangement of lenses I obtained a stain nearly an inch in diameter of the general figure of the moon, in which the places of the dark spots might be indistinctly traced" (*Phil. Mag.*, vol. xvii. p. 222, 1840).

In 1850, W. C. Bond, in conjunction with J. A. Whipple, a photographer of Boston, obtained some really good daguerreotypes of the moon. The instrument used was the equatorial of 15 inches aperture belonging to Harvard College Observatory, and images from two to three inches in diameter were obtained on plates adjusted at its focus. Some of these pictures on glass, and mounted for the stereoscope, were exhibited in London at the Great Exhibition of 1851, and also at Paris ("Annals, Observatory of Harvard College," vol. i. p. clvii.).

Also in 1850, Niépce de St. Victor obtained a strong impression of the full moon in twenty seconds on an albumenized glass plate sensitized with silver chloride. He had only discovered this photographic process a few months previously, and the plate was exposed in order to test the efficiency of the film employed. No attempt was made, however, to follow the moon's motion, so the pictured disk could hardly have exhibited the circular

outline of the object it portrayed (*Comptes rendus*, vol. xxx. p. 710, 1850).

After the discovery of the collodion process by Scott Archer in 1851, lunar photography grew apace. Warren De La Rue exhibited some photographs of the moon at the Royal Astronomical Society in 1853. With respect to these pictures he afterwards remarked: "At the latter end of 1852 I made some very successful positive lunar photographs in from two to thirty seconds on a collodion film, by means of an equatorially mounted reflecting telescope of 13 inches aperture, and 10 feet focal length, made in my workshop, the optical portion with my own hands, and I believe I was the first to use the then recently discovered collodion in celestial photography." No automatic driving motion was attached to the telescope, and the moon's motions both in right ascension and declination were followed by adjusting a sliding frame attached to the eyepiece holder, in the diagonal parallel with the moon's apparent path (Brit. Assoc. Rep., Aberdeen, 1859, p. 131).

In July 1853, Prof. J. Phillips obtained photographs of the moon 1¼ inch in diameter on a collodion plate exposed for five minutes in the first focus of a 6¼-inch refractor. Some of the pictures were exhibited at the Hull meeting of the British Association in September 1853, on which occasion Prof. Phillips read a paper "On Photographs of the Moon," and pointed out the many advantages to be gained by the development of lunar photography (Brit. Assoc. Rep., Hull, 1853, p. 14). He also dwelt on the desirability of using reflecting telescopes for the purpose because of the fact that in such instruments the chemical and optical foci coincide.

The Rev. J. B. Reade, the discoverer of many important improvements in photographic processes, made several not very successful attempts to obtain daguerreotypes, whilst Bond and Whipple were producing such pictures in America. Later, in 1854, by exposing a collodion plate for thirty-five seconds in the focus of a reflector having an aperture of 24 inches, a negative of the full moon was obtained from which enlargements 9 inches in diameter were made. These results were exhibited at the meeting of the British Association held at Liverpool in 1854 (British Association Report, 1854, p. 10).

Mr. Hartnup, of the Liverpool Observatory, in conjunction with Dr. Edwards and Mr. Forrest, also took some lunar photographs in 1854, by means of an 8-inch refractor, and exhibited the results at the above meeting (*ibid.*, p. 66).

Prof. Crookes began work with the same instrument in 1854, and his first step towards obtaining good negatives was the introduction of the purest chemicals. This, and a strict adherence to correct formulæ, enabled him to reduce the exposure from thirty to four seconds. The diameter of the moon's image in the first focus of the instrument used was 1·35 inch, and the negatives obtained bore an enlargement of twenty times, but on account of the proportional magnification of defects in the film, the results were not perfect. To eliminate defects arising from this cause, Prof. Crookes suggested that "The magnifying must be conducted simultaneously with the photographing, either by having the eyepiece on the telescope, or, better still, by having a proper arrangement of lenses to throw a magnified moon image at once on the collodion" (Roy. Soc. Proc., vol. viii. p. 363, 1857).

In 1857, Mr. S. Fry obtained photographs of the full moon by means of an eight and a half inch refractor. With this instrument it was found that the average exposure for the full moon was three seconds, for half moon twelve seconds, and for quarter moon forty-five seconds; collodion plates being used. Mr. Fry observed that the distance of the chemical focus from the object-glass was subject to variation, the change being most probably due to variations in temperature (*Photographic Journal*, vii. p. 80, 1862).

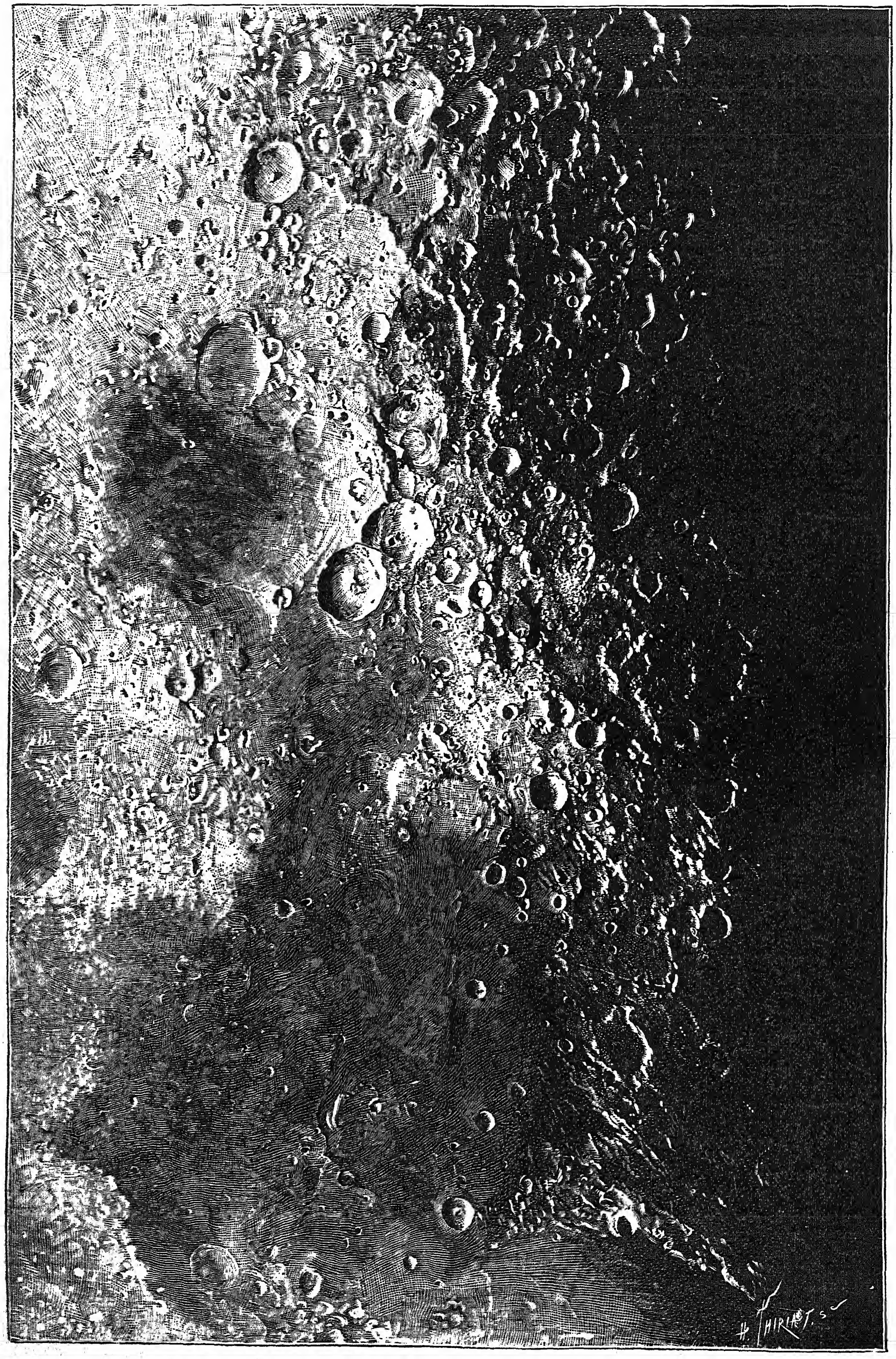
Secchi paid much attention to the photography of particular portions of our satellite, and during the first quarter. The chemical activity of the light of the moon at full and at first quarter was found to be in the proportion of three to one (*Comptes rendus*, vols. xlii., xlvi., 1856, 1858). In 1857, Sir Howard Grubb, using a refractor of 12 inches aperture, obtained photographic images of the moon a little over two inches in diameter with exposures from ten to forty seconds. A sliding back similar to that invented by De La Rue and afterwards improved upon by Lord Rosse, was used to follow the moon's motion. The improvement consisted in the application of clock-work motion to the slide in order to follow motion in declination and in regulating the driving clock of the equatorial to follow the moon in right ascension (Dublin Photographic Society, May 6, 1857.)

Although De La Rue obtained some excellent photographs in 1852, when working under very disadvantageous conditions, it was not until 1857 that he began to produce those detailed representations of the lunar surface that have made his name immortal. The want of a driving clock was the cause of the cessation of lunar photography in the former year, and when this had been supplied, De La Rue continued his work. Numerous positives on glass and negatives slightly more than an inch in diameter were obtained. These were perfectly defined and bore a magnification of more than 16 diameters. In 1859, at the British Association meeting held at Aberdeen, De La Rue reported "On the Present State of Celestial Photography in England," and exhibited some of the fruits of his labour. Amongst the specimens were positive enlarged copies of other negatives, eight inches in diameter, which would bear still further enlargement, and instantaneous pictures of the full moon. It was noted that very strong pictures of the full moon were produced with exposures from one to five seconds, and that the crescent moon required from twenty to thirty seconds. A great part of the report was devoted to a discussion of the methods adopted in taking stereoscopic pictures of the moon, many photographs of this character being exhibited at the meeting.

The extensive multiplication of enlarged copies of De La Rue's negatives renders it unnecessary to expatiate on their excellency. A magnificent series of twelve photos was published in book form, and also enlarged so that the lunar disk had a diameter of seventeen inches. In this series the moon's progress was traced from the time when she was six days old through the waxing and waning periods to the 23½ day. Each of the pictures was a work of art, whilst the many details they contained gave them a high scientific value, and conclusively demonstrated the applicability of photography to the delineation of celestial bodies.

An enlargement three feet in diameter, from a negative taken by De La Rue in 1858 is suspended in the library of the Royal Astronomical Society.

Rutherford began his work in lunar photography in 1858 with a refractor having a focal length of fourteen feet, and an aperture of eleven and a quarter inches. By reducing the aperture of the telescope to five inches for the full moon, negatives were produced which would bear enlargement to fifty diameters, or five inches. In the same year, whilst De La Rue was obtaining stereoscopic pictures in England, Rutherford was working in the same direction in America, and with similar results. To the general public the photographs taken by Rutherford in 1858 left little to be desired, but they did not reach that degree of perfection which is necessary to satisfy a scientifically cultured mind. A mirror having a diameter of thirteen inches was therefore worked and fixed on the tube of the refractor in 1861. The results obtained were, however, still deemed unsatisfactory. The mirror soon became tarnished by the action of the combustion products of the gas used for illuminating purposes, while the motion of vehicles in the



Central Reg.on of the Moon. (From a Photograph by the Brothers Henry.)

neighbouring street gave it vibrations, which by reflection were doubled in amount. The reflector was therefore abandoned, and Rutherford resolved to have an object-glass made of the same size as that formerly used by him, but specially corrected for photographic rays. With the completion of this instrument Rutherford's best results began. In March 1866, some remarkably fine negatives were obtained with exposures from two to three seconds three days after the first quarter, and one-quarter of a second for the full moon. The publication of enlargements from these negatives, having a diameter of twenty-one inches, was much appreciated by astronomers and others interested in lunar photography.

No man has done more in the furtherance of celestial photography than Dr. Henry Draper, the son of the renowned physicist to whom reference has already been made. After a brilliant scholastic career he was associated with his father in many important researches. A journey to the British Isles in 1857 gave Dr. Draper the opportunity of visiting Lord Rosse's observatory at Parsonstown. He was so struck with the power of the great reflecting telescope, that on returning to America in 1858 he began the working of a similar speculum having a diameter of 15 inches. This was afterwards discarded, and a silvered glass Newtonian reflector, having a diameter of 15½ inches, was constructed, and adapted for celestial photography. Detailed descriptions of the construction and testing of the mirror, the method of silvering, and the manner in which it is mounted, are embodied in a memoir by Dr. Draper "On the construction and use of a silvered glass telescope, 15½ inches in aperture, and its use in celestial photography" ("Smithsonian Contributions to Knowledge," vol. xiv. p. 52, 1864). Instead of driving the telescope in the usual way by means of a clock, Dr. Draper used a sliding plate holder, driven by a "clepsydra" specially devised for the purpose. Some perfectly defined negatives were obtained in 1863, about 1½ inches in diameter; many of them were enlarged to 2 feet, and from one a magnificent picture was made in which the lunar disc had a diameter of 50 inches. The beauty of the copies was probably due to some extent to the fact that a concave mirror was used instead of a combination of lenses in the process of enlarging.

From the time when Dr. Henry Draper produced his best results until last year very little remarkable work had been done in lunar photography. In 1866, Mr. A. Brothers took several good negatives 11/16 of an inch in diameter in the first focus of a 5-inch refractor, and by the insertion of a Barlow lens he increased the size of the image to 1¼ inches. Enlargements from these negatives were distributed to many astronomers, and evidence of their excellency is afforded by the circumstance that they were mistaken for some of Rutherford's productions by the editor of a scientific journal, and commented upon as such. Mr. Brothers gave a long account of the development of celestial photography in the paper in which his method of work was described (Proceedings of the Literary and Philosophical Society of Manchester, vol. v. p. 68, 1865–66.)

In 1872, Mr. Ellery, the Director of Melbourne Observatory, presented some remarkably sharp lunar photographs to the Royal Astronomical Society, that he had obtained by means of the great reflector (*Monthly Notices R.A.S.*, vol. xxxiii. p. 219, 1873.)

Amongst other lunar photographs possessed by the Royal Astronomical Society are two taken by Dr. Gould at Cordoba Observatory in 1875–76, in each of which the moon has a diameter of nearly 20 inches. A photograph taken in 1877 by Prof. Pritchard at Oxford, with the reflector used by De La Rue, and some taken in 1880 by Mr. Common with his three-foot reflector, also figure in the above collection as remarkable works of art having an important scientific signification.

In a recently published paper on "Astronomical Photography at the Lick Observatory" ("Publications of the Astronomical Society of the Pacific," vol. ii., No. 9), Prof. Holden gives a detailed account of the photographic apparatus of the great equatorial, and the work done with it. The image of the moon in the first focus of this instrument is nearly five and a quarter inches in diameter, and the negatives bear easily an enlargement of 570 diameters, and even double this amount. In the production of these negatives the aperture of the object-glass was reduced to 12 inches. From an examination of the best pictures yet taken at the Lick Observatory, Prof. Holden finds that parallel walls on the moon whose tops are no more than 200 yards or so in width, and which are not more than 1000 or 1200 yards apart, are plainly visible. A series of copies from the negatives obtained at Lick Observatory has been published.

Some photographs of the moon taken in March last, by the Brothers Henry, at Paris Observatory, appear to eclipse all previous ones. The instrument used was the 13-inch photographic equatorial, and an examination of the plate which accompanies this note will show that real progress has been made. The superiority of the results is due not only to the perfection of the object-glass, but to the use of a secondary magnifier, by means of which the size of the image at the first focus was increased fifteen times. It is manifest that this method of direct enlargement possesses many advantages over that ordinarily used, and its further development will be awaited with considerable interest.

There is no doubt that enlarged photographs of our satellite are capable of affording more information regarding its surface than can be gained by years of diligent observation, whilst their multiplication at different epochs will enable selenographers to readily detect changes of a comparatively minute character. The study of the lunar surface has always excited interest. Hence the contribution to knowledge afforded by the photographs taken by MM. Paul and Prosper Henry will not lack the appreciation it fully deserves.

RICHARD A. GREGORY.

COMPARATIVE PALATABILITY OF INSECTS, &c.

IN the course of last autumn and the present summer we made a series of experiments bearing upon the relative palatability of insects, &c.,; the animals chiefly experimented on being domestic mice, common toads, and a common Mynah (*Acridotheres tristis*). We obtained the following results.

Among beetles, *Carabus violaceus*, which emits a very strong, unpleasant-smelling fluid, was once eaten by the toads, and twice by the mice. As a rule, however, it seemed too large and strong for either. The Mynah, also, was not very fond of it.

Torostichus niger, and the nearly-allied red-legged species, which also emit strong-smelling fluid, were readily taken by all the animals under observation: though they sometimes caused the mice a little trouble.

The small copper-coloured ground-beetles were eaten readily by the Mynah and toads, but in every case refused by the mice.

Melolontha vulgaris was liked by the mice and toads. We did not offer it to the Mynah.

Coccinella bipunctata was invariably licked and refused by the mice, even when hungry. The toads took it readily. We did not offer it to the Mynah.

Ocypus olens, the "devil's coach horse," was taken without hesitation by the toads, even in its defiant attitude, with the head and abdomen erected. On one or two occasions, however, it was immediately ejected. This has also happened with *Torostichus niger*, and appears

to be due to the bite of the insect rather than to the emission by it of unpleasant matter. The *Ocypus* was eaten by the Mynah.

The red soldier-beetle was seized by one mouse, which, however, left the abdomen. It was refused by another, which was feeding rather poorly at the time; though the same animal, immediately afterwards, killed and partly ate a house-fly. This beetle was eaten by the toads. We did not offer it to the Mynah.

The dung-beetle (*Geotropes stercorarius*) was offered only to the toads. It was apparently too large and strong for them.

Among Hymenoptera, only one *Bombus* (*terrestris*) was offered to the mice: they seemed afraid to touch it. We were surprised at this, in the face of Darwin's fact of field-mice attacking nests of Bombi. The Mynah ate wasps greedily. The toads readily took wasps and bees (*Megachile*, *Apis*, *Bombus*), only occasionally refusing the large queens of the Bombi. They were often stung, but did not seem to suffer from this, since they would take three or four of the insects in succession.

We were not fortunate enough this year to take any of the Chrysididæ. Experiments should certainly be made with these.

Of Lepidoptera, the Mynah was offered *Pieris rapæ* and *Vanessa urticæ*. It would eat both; but greatly preferred the latter. We gave the mice *Pierides rabæ* and *napi*, *Vanessa urticæ*, *Tryphæna pronuba*, some other (dull-coloured) *Noctuæ*, and some *Geometræ*. All were eaten. *Pieris rapæ*, *Vanessa urticæ*, *Bryophila perla*, *Plusia gamma*, and several other (dull-coloured) moths, were offered to the toads. Two specimens of the *Bryophila* were eaten; but the other insects were almost invariably unregarded. This appeared to us to require explanation, as the other animals ate butterflies and moths so readily. We kept our toads in an open enclosure; and were therefore obliged to mutilate the wings of the insects given them. The consequence was that these either fluttered violently or remained perfectly stationary; and toads do not seem to take food under either of these conditions.

The silkworm moth was taken by the mice; to which alone we offered it.

The swallow-tail moth (*Urapteryx sambucaria*), of which we only obtained one specimen, was eaten by a mouse.

Green and brown larvæ were taken greedily by the Mynah and toads. The latter also ate the bright-coloured caterpillars of *Pieris* sp. (*rapæ?*), and any hairy caterpillars that were offered them. Among them was that of *Orgyia antiqua*. In one case a hairy caterpillar was not swallowed till two or three attempts had been made to secure it. No hairy caterpillars were offered to the other animals.

Some bright orange-coloured larvæ, with black heads—found feeding, in a web, on hawthorn—were readily eaten by the toads and by one mouse. Another mouse (feeding poorly) refused them.

A scarlet-and-black bug was eaten by the toads; as also was the lace-wing fly (*Chrysopa perla*). Neither insect was offered to the other animals.

Three sword-tailed grasshoppers were readily eaten by a mouse.

Blatta orientalis was eaten by the toads. We did not give it to the mice. The Mynah for a long time refused it, and only took it finally in the dearth of other insects. The same holds good, in its case, of *Lumbricus terrestris*.

A few centipedes were given to the mice and the Mynah. These were never eaten; though the mice, in one case, eagerly seized and killed a large specimen. We offered small frogs to the Mynah, which seized, but did not eat them; leaving them apparently unhurt. The toads eat —though with some difficulty—small newts; which a water tortoise (*Emys* sp.) will not take.

E. B. Titchener.
F. Finn.

A young heron (*Ardea cinerea*), which takes frogs freely, killed, but did not eat, a common toad.

A water-tortoise (*Emys* sp.), though it eats small frogs, will not touch a toad. E. B. Titchener.

Zeuzera æsculi was offered to a prairie owl at the Zoological Gardens; and though eagerly seized, left alive after considerable examination. Queen ants were taken by toads and by the common lizard (*Lacerta vivipara*).

F. Finn.

THE PROGRESS OF BIOLOGY IN CANADA.

We have before us the official account of the formal opening of the new building of the Biological Department of the University of Toronto, on December 19 last. The building is a substantial stone one in Scottish Norman style, replete with the most modern fittings and accessories; and the lecture hall, which may be approached independently of the main edifice, is benched to seat a minimum audience of 250. The work of the institution is presided over by Prof. R. Ramsay Wright. The classes in biology are said to be among the largest in the University, and the excellence of the new arrangements and teaching appliances elicited, at the opening ceremony (from Prof. Osler, an old student of the parent college), the remark that "it is possible for one to live through a renaissance, similar perhaps in kind, less important in degree, than that" directed against mediæval thought. May this be justified! Certain it is that the biological work now in progress in Toronto was begun under most auspicious circumstances.

Prof. Ramsay Wright is well-known and respected in this country and, at the opening of his new building, allegiance was sworn him by Minot and other biologists of the New World whose published researches, like his own, rank high in contemporary literature. Investigations like those upon the spiracular cleft of Ganoids, the nervous system of the tadpole's epiderma and of the liver, which his school has given to the world, are not to be easily matched as thoroughgoing and honest pieces of work. They denote a high standard of attainment, and one which, in face of the inanities of certain trans-Atlantic workers of another type, must be maintained if the biological brotherhood of the New World is to hold its own.

The Biological Department of the University of Toronto exists in connection with a Medical School, and it is therefore not surprising to find signs of a leaning towards bacteriology and those allied branches of study which, as being furthered by Mentschnikoff and his pupils, by Darier, Podwyssozki, Neisser, Ruffer, Macallum, and others, are just now assuming a revolutionary phase. Indeed, the key-note was struck by Prof. Wright in the peroration to his opening address, in which he said that "not only bacteria, but low forms of animal life furnish important pathogenic organisms." We rejoice in this the more, now that an outcry against the biological training of the surgeon-student is being raised at home, by persons who clamour for the restoration of an apprenticeship system. From the utterances of distinguished medical experts made at the Toronto ceremony, it is certain that this proposal will meet with no response from the New World.

The Biological Institute of Toronto is detached from the main University building. The latter was, on February 14 last, almost wholly destroyed by fire. During the preparations for the annual *conversazione*, a wooden

tray covered with lighted lamps fell to pieces, while being carried; a lamp was upset, and, although the burning mass was heroically carried towards the exterior by the Sub-Curator and a caretaker, the building, its valuable contents, museums, and books, were for the most part destroyed. Prof. Wright has been for some months on a tour of inspection in Europe, seeking, among other things, gifts of specimens and books. Truly, our Canadian brethren do not deserve these unless better able to take care of them than in the past. Prof. Wright assures us that such will be the case, and, on his behalf, we appeal to specialists and others who may be possessed of duplicates, and to those who may be otherwise willing, to help. The position is one which threatens to injure seriously the educational prospects of a rapidly advancing country to which we, at home, are much beholden; and it calls for combined action, by which alone a loss such as that we deplore can be made good.

NOTES.

AT a meeting of the Royal Geographical Society of Australasia, held at Melbourne on August 22, a letter from Sir Thomas Elder was read, in which he offered to bear the entire cost of an expedition to the unexplored regions of Australia. A report on the question of Antarctic exploration was also submitted to the meeting. In this report it was stated that public interest in the subject had been revived by the announcement that Baron A. E. Nordenskiöld, after a conference with his friend, Baron Oscar Dickson, had consented to take the command of an expedition to the South Polar regions, on the condition that the Australian colonies contributed a sum of £5000 towards the expenses, Baron Dickson having offered to advance the other moiety, or whatever more might be necessary. "The offers were cordially accepted, and the Antarctic Committee felt itself justified in making the necessary arrangements without delay for collecting the amount to be contributed by the Australasian colonies. The Council of the Society had passed resolutions recognizing a national duty in the exploration of the Antarctic regions, especially that portion lying opposite to Australasia, pledging itself to use its influence in promoting the enterprise, and giving authority to head a subscription list in aid of the Swedish-Australian Exploration Fund with a donation of £200 from the Society's funds. It would appear from the hearty reception accorded to the proposals of the Antarctic Committee that the latter might rely upon the energetic co-operation of all the scientific societies of Australasia, and thus be enabled to collect the amount of the contribution promised towards defraying the expenses of the combined Swedish and Australian Exploring Expedition to the South Polar Regions." The report on being put to the meeting was "received with acclamation."

AN expedition to Greenland will start from Denmark next year, under the command of Lieutenant Ryder, to investigate the east coast between lat. 66° and 73°.

PROF. EDWARD HULL, F.R.S., has severed his connection with the Geological Survey of Ireland, of which he has been Director for nearly 21 years. The one-inch geological survey of Ireland having been completed, the staff has been reduced. The Dublin *Daily Express*, commenting on Prof. Hull's retirement, says he takes with him "the best wishes of his colleagues, who will retain a vivid recollection of the consideration, kindness, and sympathy which he ever manifested towards them."

THE India Store Department lately sent to the Royal Gardens, Kew, specimens of oak-staves which had formed part of a beer-barrel. The barrel was made in the early part of 1889, filled with malt liquor in the autumn, and shipped with others as Government stores in March 1890 to Calcutta. The contents were spoiled, and the authorities at Calcutta reported that some casks were found to have been attacked by wire-worm or borer. Were the casks unsound when shipped from this country, or had they been attacked on board ship during the voyage out? The matter was submitted to Mr. W. F. H. Blandford, Lecturer on Entomology at the Indian Civil Engineering College, Cooper's Hill; and his report, which embodies the results of much ingenious labour, is published in the new number of the *Kew Bulletin*. "Notwithstanding the somewhat scanty material that was available," says the *Bulletin*, "Mr. Blandford has very skilfully traced the cause of the injury, and probably also identified the particular insect concerned. Further, he has shown that the injury to the wood had occurred before it was worked up into barrels, although, owing to the very minute holes made by the insects, it was almost impossible to detect their presence." Other subjects dealt with in this number of the *Bulletin* are: prickly pear in South Africa, Jarrah timber, treatment of mildew on vines, cultural industries in West Africa, and economic plants of Madagascar.

MR. CECIL CARUS-WILSON writes to us that he has recently invented a luminous crayon for the purpose of enabling lecturers to draw on the blackboard when the room is darkened for the use of the lantern. He hopes that the invention may prove of value not only to lecturers who use a lantern, but also (in another form) to those students who wish to take notes.

MR. ROBERT SWORDY, of Dryburn Cottage, Durham, sends us a letter which has been printed in the *Durham County Advertiser*, giving an account of a toad (*Bufo vulgaris*) which he recently saw crawling out of the Pond Wood at Aykleyheads. The muscles of the toad's body were (as usual) arranged in such a fashion that the back of the toad looked like minute nodules of dark gravel embedded in a damp path below trees; but what seemed to Mr. Swordy most remarkable was that on the top of this gravel-like arrangement of muscles there was spread a mesh or network of very fine lichen, with oval-shaped leaves of a lightish green colour, connected more or less to each other by a hair-like process of stems. This lichen spread irregularly over the toad's back, and odd sprays of it were also to be seen on the legs and upper surfaces of the feet. "Now," says the writer, "had the toad been in its regular haunts under the trees and shrubs, with this wonderful counterfeit of gravel and protective colouring, it would have been almost impossible to discriminate its form from the dark gravel, lichens, moss, wood-sorrel, and dead leaves of the place, and I doubt not that this animal's unobtrusive attire would aid it materially in capturing the insects necessary for its sustenance." Mr. Swordy encloses photographs of the toad sitting on a section of lichen-coloured gravel path, taken from near the spot where he found it.

MESSRS. THOS. J. SYER AND CO. inform us that they have, at 45 Wilson Street, Finsbury Square, London, a class-room in which are taught, practically, various trades, such as carpentry, cabinet-work, wood-carving, &c. The winter session is said to have been very successfully opened, and Mr. Syer, who acts as principal, invites anyone who may be interested in the subject to visit the room, and o see the work in progress.

A WORK on art among the Dayaks of Borneo, by Alois Raimund Hein, has been issued at Vienna. The publisher is Alfred Hölder.

MR. WILLIAM P. COLLINS, scientific bookseller, has issued a Catalogue (No. 24) of miscellaneous scientific books.

THE next meeting of the Royal Microscopical Society will be held on Wednesday, the 15th inst., at 8 o'clock, when the following papers will be read:—Note on a new type of Foraminifer, by H. B. Brady; new method of demonstrating intercellular protoplasmic continuity, by P. C. Waite; and, simple form of warm stage for the microscope, by F. Dowdeswell.

THERE is some difference of opinion as to the original meaning of the word "kangaroo." At the meeting of the Linnean Society of New South Wales on August 27, the question was discussed whether, in the dialect of the blacks of the Endeavour River, the word signified "I don't know," and was so used in answer to the queries of Captain Cook's party, or whether, as Cook supposed, it really was the name of the animal in use among the aborigines of the locality.

AT the same meeting Mr. A. J. North criticized the statement of the late Mr. Gould that the gay attire of the members of the genus *Malurus* "is only assumed during the pairing season, and is retained for a very short period, after which the sexes are alike in colouring" ("Hand-book to the Birds of Australia," i. 317). According to Mr. North, full-plumaged males, more particularly in the section of the genus in which blue predominates, are to be met with all the year round.

A VALUABLE contribution to the subject of atmospheric electricity has been lately made by Prof. L. Weber, who, in experiments at Breslau, used a sensitive, earth-connected galvanometer, instead of the electroscope in Exner's method. Using Exner's metallic rod and flame, he found that the currents were extremely small, about a micromilliampere (or the thousand-millionth part of an ampere). They were increased with a longer rod and bigger flame; but much better results were got with a kite or captive balloon. The edge of the kite was coated with silver paper, and the tail was formed with tassels of the paper. A line of fine steel wire was used, and about 12 feet at the upper end were of non-conducting string. Experiments were made on 12 cloudless days. Taking the intensities of current as ordinates, and the heights to which the kite (or balloon) rose as abscissæ, the curve of intensity had its convex side to the axis of abscissæ. On but few days was the current negative, this effect being probably due (the author thinks) to dust charged with negative electricity which it gave to the line. This might neutralize some of the positive electricity set flowing in the wire by the earth's induction. Prof. Weber considers that any experiments on the earth's surface with short conductors can at best give relative values and determine periodical changes. His values differ not inconsiderably from Exner's. At a height of 350 m. (1166 feet) the potential was found to be 96,400 volts; and, assuming a regular increase of potential with height, the fall of potential would here be 275 volts. The potential of the earth is estimated at the enormous value of $1720 \cdot 10^6$ volts. Supposing the volt to be about the electromotive force of a Daniell element, a huge battery of this number of elements would be needed to produce the earth's potential, the zinc pole being connected with earth, and the copper led into space. Prof. Weber considers the question of possible electric repulsion from the earth, and is led to some instructive remarks on rain particles, clouds, &c. Some very interesting effects were obtained from thunder-clouds; but for these and other matters we may refer to the original (an account of these researches appears in *Humboldt* for September).

THE Smithsonian Institution is publishing some interesting reports of the results of explorations by the U.S. Fish Commission steamer *Albatross*. In one of these reports Mr. Charles H. Townsend deals with birds from the coasts of western North America, and adjacent islands. Mr. Townsend, referring to several of the islands visited by the *Albatross*, points out that a ich field awaits the naturalist who may explore them. "The islands of the Santa Barbara group," he says, "have hitherto been very imperfectly explored with regard to their fauna. Clarion and San Benedicte Islands, of the Revillagigedo group, had never before been visited by naturalists. Socorro, an island of the same group, and one abounding in peculiar species of vertebrates, had not been visited since the type specimens were collected by Grayson, about the year 1870. The flora of all the Revillagigedo Islands is practically unknown, as the *Albatross* brought back only a small collection of flowering plants."

THE Royal Meteorological Society have published the first part of vol. x. of the *Meteorological Record*, containing the results of observations for the quarter ending March 31 last, with remarks on the weather by W. Marriott, Assistant Secretary, containing a large amount of useful information, compressed into 20 pages. The remarks show at a glance whether temperature, rainfall, &c., have been above or below the average; for the period in question the temperature of January was, on the whole, very mild, while cold spells occurred from February 3–15, and from February 20 till March 5. During the first few days of March, temperatures were lower than in any other March for nearly 50 years; but, on the whole, the temperature of the quarter, and also the rainfall, were above the average. The tables contain the values of bright sunshine for 31 stations; monthly results of observations at stations of the second order (for 9 a.m. and 9 p.m.) at 25 stations; abstracts of climatological observations, chiefly temperature and rainfall (for 9 a.m.) at 73 stations; earth temperatures observed between 3 inches and 6 feet at various stations; and, lastly, the observations from the Quarterly Reports of the Registrar-General, with remarks on the weather by James Glaisher.

THE second part of the Annual Report of the Chief Signal Officer (U.S.) for 1889 contains a treatise entitled, "Preparatory Studies for Deductive Methods in Storm and Weather Predictions," by Prof. Cleveland Abbe. The object of the paper is to consider the physical principles that are involved in the formation and motion of storms, and that have guided the author in predicting storms in his official capacity in the Signal Service, and it is an able and most instructive exposition, with very few mathematical formulæ, of the progress made in meteorological science during the last thirty years. The author distinguishes between matters that are important, such as the earth's rotation, gravitation, and solar radiation, and those that are unimportant, such as lunar influence, atmospheric electricity, and magnetic disturbances. The general idea that underlies the work is that a storm centre moves towards the region where conditions produce the greatest precipitation of aqueous vapour. Objection is urged against the idea that high westerly currents carry the storms of America eastward. The work is obtainable in a separate form.

A NEW theory of sea-sickness has been recently offered by M. Rochet. Accepting the view that the symptoms are those of cerebral anæmia, he accounts for this anæmia by the disorder brought into muscular contractions through not being used to such sudden movements as those of vessels. He points out the enormous capacity of the reservoir formed by the muscular and perimuscular venous system, and the considerable *rôle* of tonicity and voluntary or reflex muscular contractions in the action of emptying it; also the predominance of reflex muscular actions over voluntary, in keeping one's balance, and in most movements. In the movements of a vessel, the relaxation of muscular tonicity and suppression of reflex movements result in a considerable increase of the peripheric reservoir, and, as a consequence, in cerebral anæmia. Hence it is that the descent of the ship is the most trying motion; and one can understand the benefit of the horizontal position, compression of the abdomen, fixing the body in a tight position, &c. Very young children are not ill, because the education of the reflexes in them is not yet accomplished. On solid ground they reel as on deck. M. Rochet's advice is, not to look to anæsthetics, soothing drugs, &c., for relief, but rather to muscular excitants, and above all to seek in voluntary movements a compensation for the reflex

movements which are not produced. He recommends strychnine, veratrine, ergot of rye, and drinks charged with carbonic acid.

MR. S. V. PROUDFIT has presented to the U.S. National Museum a collection of stone implements from the district of Columbia. In an account of the collection, published in the Proceedings of the Museum, Mr. Proudfit pays a tribute to the handicraft of the aboriginal tribes of the region in which the collection has been formed. The material with which they wrought was, he says, the most obdurate and refractory of all substances found available to any considerable degree among the American Indians. Quartz, quartzite, and argillite for the greater part were used from necessity, no better material being within reach. The first two are very hard, and, in the hand of the workman, full of unpleasant surprises. The argillite, though softer, is not susceptible of receiving or retaining any high degree of finish. Notwithstanding these obstacles, the material was treated "with such patience, care, and skill, that the work of this region, not only in matters of utility, but in points of finish, compares favourably with that of any other."

A PAMPHLET by Dr. Edward Sang, on the exhibition of curves produced by the vibration of straight wires, which was read before the Scottish Society of Arts last November, has been sent to us. The means adopted for obtaining these curves was to make one end of a wire fast in a vice, while the other end was free to move; the motion of the wire being made visible by fixing to the free end a small polished knob capable of reflecting light from some given source. These phenomena were clearly found to be connected with the unroundness of the wires, and the object of the paper was to inquire what would be the result with a given irregularity. The ratios of vibrations having been varied, different results were obtained, and it was noted that when the ratio was expressed by two odd numbers, the closed curve connected two opposite corners and passed through the centre, but that when one of the numbers was even, the closed curve connected two adjacent corners and did not pass through the point of rest. Illustrations of some of these curves are given, showing their delicacy and symmetry similar to those curves formed by the resultant motion of two pendulums of different periods oscillating at right angles to one another. With bent wires particularly, as the author states, "the poetry of motion, the gracefulness of curvature, attract the student of the fine arts, who may find examples ranging from the severe classic and tragic to the extravagant burlesque styles."

IN the last issue of the *Bulletin* of the Moscow Naturalist, Madame Marie Pavloff concludes her excellent studies on the palæontology of the Ungulata. She examines the fossil relics of the *Hipparion* in Russia (*H. mediterraneum* and *H. gracile* which she considers as the same species, and the probably new species of *H. minus*), as well as the relics of the Pleistocene horses found in Russia, and gives a genealogy of the Equideæ since the Mio-Pliocene period. At the beginning of the Middle Pliocene, horses akin to *Eq. hippidium*, which at that time were living in America only, emigrated from West America to Asia, and during the Middle Pliocene epoch they developed in the Siwalik mountains into forms now described under the name of *Equus sivalensis.* Part of the latter migrated from Asia to Africa, at that time connected with Italy, and thus reached Europe. In the Upper Pliocene deposits of Africa and Europe we have the *Eq. stenonis*, which is very near to the foregoing. Having thus reached Europe from Asia during the Upper Pliocene, they left their fossil relics in the *Eq. stenonis* in Italy, Austria, Germany, France, Great Britain, and Russia, and slowly evolved the Pleistocene species of *Equus caballus.* However, one part only of the *Equus sivalensis* having left Asia, the remainder developed at the same time into the *Equus nomadicus* in Asia; while in America a parallel evolution gave rise to the *Eq. excellus* and *Eq. major* and to *Equus caballus fossilis* in Africa. As to the present horses of Russia, they all originated from the same Pleistocene species which already at that time offered great varieties in Europe. The variety of the Russian races depends upon the continued mixture of forms which developed in Russia with those which were imported from Asia on the one side, and from Western Europe on the other side.

SIR WILLIAM GREGORY, a former Governor of Ceylon, has lately been revisiting that island, and has communicated to a local journal a series of notes and observations. In one of these he refers to the well-known Colombo Museum, as to which he says it is hoped that a very liberal extension will be conceded by the Legislative Council without delay. Important objects of natural history have been procured and cannot yet be exhibited, books of value to the general reader and to inquirers have accumulated without the means of arrangement, and the space for large archæological objects which should be carefully and liberally displayed is altogether defective. There is one department, continues Sir William, hitherto much neglected, to which special care should hereafter be devoted, viz. that of geology. A geologist of high attainments ought to be engaged for a fixed period of a few years, during which a general geological survey should be made and a perfect collection be formed of the rocks, clays, and gems which are a specialty of Ceylon. These, if properly exhibited and properly protected from theft, would be one of the most valuable and interesting portions of the Museum to a large number of visitors.

THE *North China Herald* of Shanghai, in a recent curious and interesting article on modern science in China, says that the views now held by intelligent Chinese on the origin of science are that the knowledge possessed by their ancestors leaked out to the men of Western nations, who improved on the information they received, and gradually developed modern sciences and inventions. This idea was started by Mei Wu-ngan in the reign of Kanghi, and has been maintained ever since with singular persistence, and the cultivated class have consoled themselves with this thought during the past two centuries. Those who are really in favour of introducing foreign improvements say:—"We wish to make use of the knowledge of Western men because we know that what they have attained in science and invention has been through the help that our sages gave them. We have a good right to it. What Europe has done she has done through the help we gave. If we did not exactly give science to Europe, we gave it the fruitful germ which produced it. They have the science of optics, but in our 'Motsz' we find that reflection from mirrors was known in the days of Mencius. The men of the West hold that the earth is round. This was believed also by our poet Chü Yuen, who in his ode on astronomy announces this doctrine; and this was not many years after Mencius. This being so, we ought not to be ashamed of the study of Western science. We are the rivals of the Western kingdoms, and it is good policy to use their spears in order to pierce their shields. We ought to train our youth in Western science so that we may know how best to meet them in the struggle to resist their encroachments." Mei Wu-ngan and others read the books translated by the Jesuits, including Euclid and the teaching of astronomy, and they were delighted with the new views. The Jesuits, however, were in high favour at Court, and while they basked in sunshine the native mathematicians shivered in the shade. This was not agreeable, and the native astronomers went home each day from Court dissatisfied. One of them, Yang, ventured to foretell an eclipse. Adam Schaal, a Jesuit, in Pekin, foretold the same eclipse, and his hours, minutes, and seconds agreed with the fact. This was a crucial case. All Pekin was waiting with interest to know the result. The pro-

phecy of the foreigner proved by its fulfilment the errors of the Chinese mathematician, who retired in disgrace from the position which he held. He went back to his home to write the book called "The Inevitable Exposure," which contained a series of calumnies and grossly untrue accusations against the Jesuit fathers. This bad book made him much more notorious than his works on mathematics. The unscrupulous enemies of the Westerns have reprinted it again and again, and they still do so. Very different was the tone of Mei Wu-ngan, who was invited three days in succession by the Emperor Kanghi to converse with him upon mathematical subjects. He had a fondness for mathematics, and read voraciously. He was therefore in a position to criticize Western knowledge in an appreciative manner.

The additions to the Zoological Society's Gardens during the past week include two Grizzly Bears (*Ursus horribilis*) from the Missouri Brakes, Montana, U.S.A., presented by Mr. Ewen Somerlid Cameron; a Raccoon (*Procyon lotor*) from the Catskill Mountains, New York State, presented by Mr. James H. Frodsham; a Greater Black-backed Gull (*Larus marinus*), a Herring Gull (*Larus argentatus*), British, presented by Mr. A M. Bailey; a Common Tern (*Sterna hirundo*), British, presented by Mr. A. C. Howard; two Mississippi Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Miss Edith Baker; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Great Kangaroo (*Macropus giganteus* ♂) from Australia, deposited; a Horned Screamer (*Palamedea cornuta*) from the Amazons, three Violet Tanagers (*Euphonia violacea*) from Brazil, an Ocellated Sand Skink (*Seps ocellata*), South European, purchased; a Chestnut-breasted Duck (*Anas castanea*) from Australia, received in exchange; a Crested Pigeon (*Ocyphaps lophotes*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on October 9 = 23h. 13m. 52s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4892	—	—	22 59 26	+11 44
(2) G.C. 4921	—	—	23 10 22	+ 5 46
(3) χ Aquarii...	5	Yellowish-red.	23 11 9	− 8 13
(4) ξ Pegasi	5	Yellowish-white.	22 40 26	+11 40
(5) 19 Piscium	6	Red.	23 40 46	+ 2 52
(6) R Aquilæ	Var.	Very red.	19 1 5	+ 8 4

Remarks.

(1 and 2) Neither of these nebulæ have yet had their spectra recorded. The first of them is about 4′ long by 2′ broad, and has been described as "a streak tapering at each end"; in the General Catalogue it is described as "pretty bright; considerably large; much elongated in the direction 11°·9; between two stars." The second is described as "considerably bright; pretty small; irregularly round; pretty suddenly brighter in the middle." They are both very conveniently situated for observation.

(3) The spectrum of this star is a very fine one of Group II., all the bands being wide and dark. Carbon comparisons will probably be the most valuable observations of this star, the character of the spectrum indicating that the flutings should be considerably bright.

(4) Secchi thought that this star had a banded spectrum, but Dunér states that it is one of the solar type ("Sp. II.*a*! du type le plus pur"). It still requires observing with reference to Groups III. and V.

(5) The spectrum of this star is one of the very finest of its class (Group VI.). It has been observed in considerable detail by Vogel and Dunér. All the principal and secondary bands are well visible. The carbon band 6 (λ 564) is stated by Dunér to be feebler than the others, and so far as we yet know this is the only band in which any considerable variation is established. It will be remembered that this is the cometary band which varies most, but in comets it varies in position also. It seems possible, therefore, that it may also change in position as well as in intensity in stars of Group VI. Comparisons with a spirit-lamp flame would easily decide this point.

(6) This variable has a well-marked spectrum of Group II. Dunér describes it as "very fine," and states that the bands are very wide and dark throughout the spectrum. The period is about 345 days, and it ranges from 6·4–7·4 at maximum to 10·9–11·2 at minimum. The star falls in species 9 of the subdivision of Group II., and if it behaves like other variables with similar spectra, bright lines should appear at or about maximum. The spectrum should also be observed for brightenings of the carbon flutings.

A. Fowler.

Observations of Comets.—Prof. E. E. Barnard, of Lick Observatory, contributes a note on Comets 1889 I. and II. to the *Astronomical Journal*, No. 225, and makes some suggestions as to the possibility of seeing the short-period comets at aphelion. The majority of observers neglect comets as soon as they become faint or difficult to see. Prof. Barnard has made it a point to take up comets when they have been dropped elsewhere, and to observe them as long as they can be seen. Comet 1889 I. has been observed at Lick Observatory from September 2, 1888, to August 18, 1890—that is, for very nearly two years; and Comet 1889 II. from March 31, 1889, to August 24, 1890—that is, for 16 months 24 days: hence the duration of visibility of each exceeds that of the great comet of 1811, which was followed for 16 months 20 days. At the observation of Comet I. 1889, on August 17, its distance from the sun was 6·25 times the earth's mean distance. On August 24, Comet II. 1889 was 5·06 times the same unit from the sun, and Prof. Barnard thinks he will be able to follow it for quite six months longer. The following are the aphelion distances of short-period comets (excepting Tuttle's) recognized at more than one return:—

Encke's ...	... 4·10	Brorsen's ...	5·66
Tempel's II.	... 4·66	D'Arrest's ...	5·72
Tempel's I.	... 4·82	Faye's ...	5·92
Swift's 1880	... 5·14	Biela's ...	6·19
Winnecke's	... 5·50		

It will be seen that Comet I. 1889 is now being observed at a distance from the sun greater than it is possible for any of the short-period comets to attain. It would appear, therefore, that some of the latter class of comets ought to be followed throughout their entire orbits.

Photographing Stars in the Daytime.—In the *Astronomical Journal* for September 16, 1889, Prof. Holden gave an elementary theory of the subject of photographing stars projected against a bright background. He showed that, if the intrinsic brilliancy of a star be ten times as great as its background, the photographic image in the Lick telescope was 4124 times brighter than that of the sky. It was also proved that small photographic contrasts of this character could be increased with a given telescope by simply cutting down the aperture. Recently, Prof. Holden writes (*Astr. Journ.*, September 19, 1890), "the question has been examined experimentally by Mr. W. W. Campbell and myself, using the great telescope (focus, 570 inches) and apertures of 33, 15, 8, and 4 inches. Photographs of Venus, Mercury, the moon, and of Alpha Lyræ have been taken in broad daylight (2 to 5 p.m.) with the apertures named, with a constant exposure of 0·13s., and on Seed 26 plates. In general, the smallest apertures used have given the darkest images, as demanded by the theory."

PHYSICS AT THE BRITISH ASSOCIATION.

In Section A, nine Reports of Committees and fifty-four papers were read. Perhaps the distinguishing characteristic of the Section is its tendency to bifurcation on the slightest provocation. Several Sections do not meet at all on the Saturday, and manage to get through their business comfortably by the Tuesday. Not so Section A. On the Saturday, under the influence of electrolytical attractions and repulsions, there occurred a dissociation of the Section into its constituent elements, accompanied by a migration of ions from places of high potential (in a Bramwellian sense) to places of low, or *vice versâ*. In accordance with the law of ionic migration enunciated by Sir Frederick at the concluding meeting, the ions collected at the

kathode were found to far exceed in number those collected at the anode.

To give even an outline of all the voluminous and multitudinous contributions to the Section would occupy many pages, and would require that the writer should have received the training of a Succi or a Jacques before undertaking the task.

M. Du Bois read a paper on refraction and dispersion in certain metals. Kundt's method of observation with very thin electrolytic metal biprisms was used in this investigation. The dispersion was determined with all possible care, using four kinds of light defined by spectral lines. It was found that light, on passing from iron, cobalt, and nickel into air, begins by following Snell's law for small angles of emission, the refractive index being mathematically defined as the limit of the ratio of sines when the angle of incidence approaches the limit zero. The dispersion in the case of each of the three metals mentioned was found to be anomalous.

Sir William Thomson, F.R.S., in a paper on an illustration of contact electricity presented by the multicellular voltmeter, called attention to the modification of the force between the aluminium needles and the brass cells of the instrument arising from the "contact electricity" difference between polished brass and polished aluminium. In the instrument as at present made, the observed difference of potential on reversal amounts to as much as ¾ volt. Thus the use of the multicellular electrometer gives a new and very interesting direct proof of Volta's contact electricity.

Lord Rayleigh, Sec.R.S., read a paper on defective colour-vision, in which he pointed out that the existence of a defect is probably most easily detected, in the first instance, by Holmgren's wool test; but this method does not decide whether the vision is truly dichromic. For this purpose, Maxwell's colour-disks may be used. Lord Rayleigh found, in the case of some colour-blind persons he was examining, that it looked as though the third colour-sensation, presumably red, was defective, but not absolutely missing. When a large amount of white was present, matches could be made, in spite of considerable difference in the red component. But when red light was nearly isolated, its distinctive character became apparent. This view was confirmed by experiments with the colour-box.

Mr. J. Swinburne, in a paper dealing with the question of the production of high vacua, called attention to the great superiority of the Geissler over the Sprengel form of mercury pump.

Profs. Barr and W. Stroud, in a paper on the use of the lantern in class-room work, described a simple and convenient form of lantern for horizontal and vertical projection, and exhibited an apparatus for the preparation of lantern-slides in large numbers from books, periodicals, &c.

Mr. W. N. Shaw read a paper on the general theory of ventilation, with some applications, in which general laws of ventilation are established similar to Kirchhoff's laws relating to the distribution of currents in a network of conductors.

On Friday, September 5, there was a discussion on electrical units, opened by Mr. Glazebrook with a paper on recent determinations of the absolute resistance of mercury, in which he carefully compared and criticised the different methods employed by various observers. The best determinations of the ohm showed that it was very nearly indeed equal to the resistance of a column of mercury 106·3 cm. long and 1 square millimetre cross-section at 0° C. Mr. Glazebrook strongly advocated the adoption of the number 106·3 instead of 106; and Sir William Thomson, Prof. Rowland, Prof. Barker, and Mr. Preece expressed their concurrence in the desirability of the change.

Principal J. V. Jones followed with a paper entitled "Suggestions towards a Determination of the Ohm," in which he described the results of experiments undertaken at University College, Cardiff, in the spring of the present year. These experiments gave the ohm equal to the resistance of a column of mercury 106·307 cm. long and 1 sq. mm. sectional area. The method adopted was a modification of that due to Lorenz, in which a metallic disk is made to rotate in the mean plane of a coaxial standard coil. Wires touching the centre and circumference of the disk are led to the ends of the resistance to be measured, and the same current is passed through this resistance and the standard coil.

The features of special interest in the method employed were:—(α) The employment of a long trough for holding the mercury; and, instead of measuring the distance between the electrodes, one electrode is kept fixed, while measurement is made of the distance moved through by the other between two positions of equilibrium of the galvanometer corresponding to two different rates of rotation of the disk. The latter measurement it is easy to make with accuracy, for the movable electrode may be rigidly attached to the movable headstock of a Whitworth measuring-machine placed parallel to the length of the trough; and the two equilibrium positions may be taken near the middle of the trough, so as to avoid danger of curvature in the equipotential surfaces passing through the electrode in its two positions. A new difficulty is, however, now encountered, viz. the determination of the section of the mercury column. The capillary depression at the sides of the trough would make it a most serious task to determine the section by direct measurements to the required degree of accuracy. This difficulty is overcome by a further differential method, viz. by making observations with the mercury at two different heights in the trough. The sides of the trough in that part of it traversed by the movable electrode are assumed plane, parallel, and vertical. The trough used in the experiments was cut in paraffin wax contained in a strong casting of iron with its sides strengthened by outside ribs. The channel was 43·5 inches long, by 1·5 inches broad, by 3 inches deep. Paraffin was found, however, not to be perfectly satisfactory, and Prof. Jones expressed the opinion that a trough of worked glass or scraped marble would have been preferable. The position of the mercury surface in the trough was determined electrically by using a pointed steel spherometer screw. The screw may be moved downwards until an electric circuit comprising the screw and the mercury is completed. (β) The employment of a brush of special form to secure good electrical contact at the periphery of the rotating disk. The brush consisted of a single wire perforated by a channel through which a constant flow of mercury might be maintained from a cistern of adjustable height. (γ) In connection with the measurements necessary to enable the calculation of the coefficient of mutual induction to be performed, Prof. Jones employs a coil consisting of only one layer of wire, the advantage of which is that every part is visible, and that nothing is done to alter the position of the wire after measurements have been made. If a coil consist of many layers, it is not quite easy to say where, after measurement, the lower layers go to under the pressure of the superincumbent ones.

In conclusion, the main suggestions offered for consideration, were:—

(1) That the time is ripe for a new determination of the ohm that shall be final for the practical purposes of the electrical engineer.

(2) That such a determination may be made by the method of Lorenz, the specific resistance of mercury being obtained directly in absolute measure by the differential method described.

(3) That the standard coil should consist of a single layer of wire, the coefficient of mutual induction being calculated by the formula given in the paper.

Sir William Thomson, in a paper on alternate currents in parallel conductors of homogeneous or heterogeneous substance, pointed out that when the period of alternation is large in comparison with 400 times the square of the greatest thickness or diameter of any of the conductors, multiplied by its magnetic permeability and divided by its electric resistivity, the current intensity is distributed through each conductor inversely as the electric resistivity; the phase of alternation of the current is the same as the phase of the electromotive force; and the current across every infinitesimal area of the cross-section is calculated, according to the electromotive force at each instant, by simple application of Ohm's law. Further, that when the period is very small compared with 400 times the square of the smallest thickness or diameter of any of the conductors, multiplied by its magnetic permeability and divided by its electric resistivity, the current is confined to an exceedingly thin surface-stratum of the conductors. The thickness of this stratum is directly as the square root of the quotient of resistivity, divided by magnetic permeability, of the substance in different parts of the surface. The dependence of the total quantity of electricity carried on extent of surface justifies Snow Harris, and proves that those who condemned him out of Ohm's law were wrong, in respect to his advising tubes or broad plates for lightning conductors, but does not justify him in bringing them down in the interior of a ship (even through the powder magazine) instead of across the deck and down its sides, or from the masts along the rigging and down the sides into the water.

Sir William Thomson read a paper on anti-effective copper in parallel conductors, or in coiled conductors for alternate currents. It is known that by making the conductors of a circuit too thick

we do not get the advantage of the whole conductivity of the metal for alternate currents. When the conductor is too thick, we have in part of it comparatively ineffective copper present; but, so far as is known, it has generally been supposed that the thicker the conductor the greater will be its whole effective conductance, and that thickening it too much can never do worse than add comparatively ineffective copper to that which is most effective in conveying the current. It might, however, be expected that we could get a positive augmentation of the effective ohmic resistance, because we know that the presence of copper in the neighbourhood of a circuit carrying alternating currents causes a virtual increase of the apparent ohmic resistance of the circuit in virtue of the heat generated by the currents induced in it. May it not be that anti-effective influence such as is thus produced by copper not forming part of the circuit can be produced by copper actually in the circuit, if too thick? Examining the question mathematically, Sir William finds that it must be answered in the affirmative, and that great augmentation of the effective ohmic resistance is actually produced if the conductor is too thick, especially in coils consisting of several layers of wire laid one over another in series around a cylindric or flat core, as in various forms of transformer.

Prof. J. A. Ewing, in a most interesting and important communication (*vide Phil. Mag.*, September 1890), exhibited a model to illustrate some novel ideas on the molecular theory of induced magnetism. The present notion of a quasi-frictional resistance opposing the turning of the molecular magnets lends itself well to account for the most obvious effects of magnetic hysteresis and the reduction of hysteresis by vibration. On the other hand, it conflicts with the fact that even the feeblest magnetic force induces some magnetism. Reference was made to another (and not at all arbitrary) condition of constraint, which not only suffices to explain all the phenomena of hysteresis, without any notion of friction, but seems to have in it abundant capability to account for every complexity of magnetic quality. Prof. Ewing supposes that each molecular magnet is perfectly free to turn except in so far as it is influenced by the mutual action of the entire system of molecular magnets. A model molecular structure was exhibited, consisting of a large number of short steel bar-magnets strongly magnetized, each pivoted upon a sharp vertical centre, and balanced to swing horizontally. The bars swing with but little friction, and their pole-strengths are sufficient to make the mutual forces quite mask the earth's directive force when they are set moderately near one another. The group is arranged on a board which slips into a large frame wound round the top, bottom, and two sides, with a coil, through which an adjustable current may be passed to expose the group to a nearly homogeneous external magnetic force.

Sir William Thomson read a paper on a method of determining in absolute measure the magnetic susceptibility of diamagnetic and feebly magnetic solids. The method proposed consisted in measuring the mechanical force experienced by a properly shaped portion of the substance investigated, placed with different parts of it in portions of magnetic field between which there was a large difference of the magnetic force. A cylindrical or rectangular or prismatic shape terminated by planes perpendicular to its length was the form chosen; the component magnetic force in the direction of its length was equal to $\frac{1}{2}\mu(R^2 - R'^2)A$; where μ denotes the magnetic susceptibility, $R R'$ the magnetic force in the portions of the field occupied by its two ends, and A the area of its cross-section.

Lord Rayleigh read a paper on the tension of water surfaces, clear and contaminated, investigated by the method of ripples. The ripples were rendered visible by a combination of Foucault's optical arrangement with intermittent illumination. Two frequencies were used, about 43 and 128 per second. The surface-tension of a clean water surface, in c.g.s. measure, was found to be 74·0, thus confirming observations made with capillary tubes. Water saturated with olive oil had a surface-tension of 41·0, and saturated with oleate of soda a surface-tension of 25·0.

Mr. W. N. Shaw reported on the state of our knowledge of electrolysis and electro-chemistry.

Mr. J. Hopkinson read a paper on the inland compared with the maritime climate of England and Wales. For special reasons Buxton, Woburn (Apsley Guise), Croydon, Cheltenham, and Churchstoke were chosen to represent the interior of the country, while Scarborough, Lowestoft, Babbacombe, Worthing, and Llandudno were chosen to represent the sea-coast. The places were so chosen that the mean position, latitude, and longitude of the five inland places should closely approximate to those of the maritime. As the result of observations extending over the decade from 1880-89, he concluded that, so far as regards our comfort and most probably also our health, our maritime climate is on the whole superior to our inland climate, being warmer, owing (it is most important to observe) to the nights not being so cold, while the days are no hotter, the extremes of temperature being much less, the air rather less humid, the sky less cloudy, and the rainfall less.

Prof. Ramsay read a paper on the adiabatic curves for ether, gas and liquid, at high temperatures. The method adopted in the experiments was an ingenious one, and consisted in determining the velocity of sound in the vapour by Kundt's dust-figures, from observation of the wave-length and the pitch of the note emitted by the stroked tube containing the vapour. This process gives the ratio of adiabatic and isothermal elasticity from which the former elasticity can be calculated as the latter is known.

Prof. Ostwald read an interesting paper on the action of semi-permeable membranes in electrolysis, in which he gave an account of experiments upon the passage of an electric current through solutions in series separated by semi-permeable membranes, and pointed out the importance of such phenomena to physiology. He explained that a semi-permeable membrane would allow ions of one kind to pass through, but arrest ions of another kind, and thus act as though it were a metallic electrode.

Prof. C. Piazzi Smyth sent a paper on photographs of the invisible in solar spectroscopy. Two photographs were shown, each measuring 40 inches long × 20 inches high. They represent in reality, only very small portions of the faint ultra-violet of the solar spectrum, but on a whole scale of 57 feet long from red to violet; and are located quite outside the spectral limit of variability to the human eye, with the grating spectroscope concerned, whether under summer or winter sun.

Profs. Rücker and Thorpe contributed a paper on regional magnetic disturbances in the United Kingdom, and this was followed by a paper upon similar disturbances in France, by Prof. Mascart. A point of great interest in connection with these papers was the continuous nature of the disturbances extending from the one country across the Channel to the other.

Prof. Lodge, in a paper on electrostatic forces between conductors, gave an account of an investigation into the forces between electric resonators as examined experimentally by Boys, and therefrom branched out into several allied subjects connected with the mechanical forces of electric pulses and waves.

Prof. Fitzgerald communicated several papers on mathematical physics to the Section. One of these bore what would have been an attractive title, "An Episode in the Life of J," had it not been for a parenthetical addition, viz. "(Hertz's Solution of Maxwell's Equations)." It may be remarked that J has nothing to do with Joule or his equivalent, and that the episode referred to was not of the popular anecdotal type.

Mr. W. Barlow, in a paper on atom-grouping in crystals, called attention to some very interesting properties of the simpler kind of symmetrical grouping of points, and pointed out an easy and effectual method of studying them by using a model consisting of equidistant parallel planes of homogeneously distributed points represented by beads.

Mr. W. H. Preece read a paper on the character of steel used for permanent magnets. Samples of steel for the experiments were obtained from all the leading firms, and after magnetization were tested by a magnetometric method. The marked superiority of the Marchal magnets over those made of English steel is due either to the quality of the steel, or to the mode of tempering—most probably the latter.

Prof. S. P. Thompson read a paper on the use of fluor spar in optical instruments, in which he referred to the existing uses of fluor spar for experiments on radiant heat, and in the "apochromatic" microscope lenses of Zeiss. The latter application derives its importance from the extremely low dispersion relatively to the mean refractive power of the material. To these applications the author now added that of the construction of spectroscopic direct-vision prisms; and he described two prisms, both constructed for him by Mr. C. D. Ahrens—one consisting of a fluor prism cemented between two flint-glass prisms, and the second consisting of one Iceland-spar prism cemented between two fluor prisms. The former was considerably shorter than the ordinary direct-vision prism of equal power: the latter had the property of polarizing the light as well as dispersing it, and presented the novel feature of a true polarispectroscope.

Mr. F. T. Trouton read a paper advocating the introduction of a coefficient of abrasion as an absolute measure of hardness.

Mr. F. H. Varley exhibited and explained the action of a new direct-reading photometer—an ingenious and compact instrument, in which intermittent illumination is employed for equalizing the intensity of illumination from two sources of light.

BIOLOGY AT THE BRITISH ASSOCIATION.

ALTHOUGH the number of papers in Section D was not quite so large as usual, it was found sufficient to occupy the time fully. As on previous occasions, the most attractive part of the proceedings was a discussion on a subject of general interest arranged beforehand, and opened by set papers.

After the President's address on Thursday, Prof. Newton gave an interesting account of the ornithology of the Sandwich Islands, discussing its peculiarities and probable affinities. He showed that the fauna is now undergoing modification, and is in danger of extermination on account of the changes which are rapidly being made in the vegetation of the islands; and he urged strongly the necessity of making a thorough examination of the fauna and flora of this important region while it is still possible. This paper led to the appointment of a committee, with a grant, for the purpose of seeing that the necessary exploration was carried out at once.

The usual reports on the zoology and botany of the West India Islands, on the migration of birds, on the disappearance of native plants, on a deep-sea tow-net, on the Botanical Station at Peradeniya, Ceylon, on the Biological Laboratory at Plymouth, and on the Zoological Station at Naples, were read, and the committees were reappointed.

The greater part of Friday's meeting was occupied by an important and interesting discussion on the teaching of botany, and especially the teaching in schools or to the young. The subject was opened with papers by Prof. Marshall Ward, Prof. F. W. Oliver, and Prof. F. O. Bower. Prof. Marshall Ward discussed the teaching of botany under the three heads: (1) elementary or school teaching; (2) more advanced or academic teaching; and (3) applied or special botany, such as forestry. He urged strongly the advantages of an early training in botany, and showed the suitability of the subject for school teaching, not however from books, but practically, and especially by means of field-work. In the teaching of applied botany he considered that principles and generalizations were of more importance than masses of facts, even in the training of the so-called practical man.

Prof. Oliver treated chiefly of the teaching of elementary botany to medical students at our colleges; and Prof. Bower dealt also with the arrangement of the usual junior University course, which he considered should be wide in its range and suggestive, rather than more restricted and exhaustive.

A number of other teachers of botany joined in the discussion; and Dr. Forsyth, of the Leeds Higher Grade School, showed that many of the suggestions which had been made were being carried out at his school, where the pupils were taken periodically to the fields to collect the specimens for their object lessons.

Prof. Marsh then gave an interesting account of the Cretaceous mammals of North America, of which he had now in his possession over 1000 specimens, all obtained during the last year or so. These remains all appear to belong to the lower forms of Mammalia, such as Monotremes and Marsupials, and are all of small size, although they are found in the same beds with the gigantic Dinosaurs, such as *Triceratops*.

Prof. Denny gave an account of an abnormality which he had found in three successive seasons in some flowers of *Tropæolum*, and which consisted in the inversion and in some cases duplication of the spur. Prof. Denny suggested that these abnormal flowers seeemed to indicate that the spur was really the representative of the two missing stamens.

Canon Tristram contributed some notes on the natural history of Hierro and Graciosa, two outlying islands of the Canary Group. A paper by Mr. E. H. Hankin dealt with the modifying action of ferments, such as trypsin and pepsin, upon diseases caused by bacteria, *e.g.* anthrax. It is suggested that the injection of the ferment causes a "defensive proteid" to be formed and thrown into the circulation for the purpose of killing the bacteria.

On Monday, Prof. Miall and Mr. Hammond gave an account of the development of the head of the adult fly in the life-history of the dipterous insect *Chironomus*, commonly found in impure water. Prof. Marshall and Mr. Bles called attention to variability in development amongst allied animals, and even amongst individuals of the same species. Dr. P. H. Carpenter contributed notes on the anatomy and morphology of the Cystidea.

Mr. S. F. Harmer discussed the regeneration of lost parts in the Polyzoa, including the formation of new polypites in *Pedicellina* on the tips of the old stalks where no endodermal tissues are present. Dr. S. J. Hickson gave two papers on the Hydrocorallina—the one dealing with the meaning of the ampullæ in *Millepora murrayi*, which were found to contain modified dactylozooids bearing only very large sperm sacs; and the other being on the gonangia of *Distichopora* and *Allopora*. An important conclusion drawn from these investigations was that, as regards the position and character of the gonads, *Millepora* is not related to any of the Stylasteridæ.

Amongst a number of botanical papers read on Tuesday were: one by Mr. R. Warrington, showing that certain bacteria have the power, usually supposed to be peculiar to chlorophyll-bearing organisms, of forming organic compounds from inorganic materials; one by Prof. Bower, on the phylogenetic relationships between the different groups of Ferns; one by Prof. P. Geddes, on the origin of protandry and protogyny; and one by Dr. J. M. Macfarlane, on hybrids, in which it was shown from a number of genera of plants that certain hybrids which had been produced were intermediate, not only in appearance and general structure, but even in the most minute histological details, between the two parent species.

GEOGRAPHY AT THE BRITISH ASSOCIATION.

IN the quality and scientific value of the papers, this Section was considerably above the average of last year. There were only about a score of papers altogether, but the Organizing Committee had determined rather to be short of papers than to accept any of trivial importance. As it was, the time of the Section during the four days on which it met was well filled up. The sittings were well attended, and sometimes almost crowded; which is saying much, considering the size of the hall in which the Section met. The Section adopted a plan which answered admirably. It adjourned each day from 1 to 2 p.m., and invariably a good audience assembled for the afternoon meeting.

The hall was well filled at the President's address, which was an excellent *résumé* of the physical geography of the Mediterranean and the regions around its shores. On two other occasions the lower part of the hall was quite filled by audiences evidently greatly interested. First, on the Monday, when there was a joint meeting of Sections E and F to discuss the important subject of the Lands of the Globe still available for European Settlement. Mr. E. G. Ravenstein opened the conference with a paper giving what may be called the geography of the subject. He excluded from consideration the Polar areas, desert areas, and tropical areas unsuited to a European population. He showed that, dealing with the subject from a purely theoretical point of view, the population of the world, at the present rate of increase, would, in about three or four generations, amount to something like 5000 millions. This, of course, sounds very alarming, but as in the case of the prediction of the exhaustion of our coal supply, it was shown during the discussion that we may keep our minds at ease. Prof. Marshall, Sir Rawson Rawson, Dr. Cunninghame, Mr. Bourne, and others who took part in the subsequent discussion, mainly from the economical point of view, suggested various considerations in modification of those derived from the purely theoretical standpoint. The earth has still vast undeveloped resources; a more equable distribution of these among mankind is possible, and even desirable; the theoretical rate of increase will certainly be modified in various ways; the so-called deserts may, actual experiment has shown, be made, by means of irrigation from underground supplies, both fertile and habitable. The great truth which came clearly out of the discussion—a truth which ought to be widely realized now that tropical Africa is being opened up—is that European colonization, in the proper sense of the term, is impossible, so far as present experience goes, between the tropics. There are, no doubt, modifying circumstances in some cases, but these are rare. As usual in such discussions, there was a certain amount of irrelevant talk;

but, on the whole, the conference quite fulfilled the expectations and the object of those who arranged it.

The hall was even more crowded, with an audience even more interested, on Tuesday morning, when Miss Menie Muriel Dowie read her paper on a journey in the Eastern Carpathians. It is the fashion in certain quarters to regard Europe as beyond the pale of geography; but to all but a very few of those who listened to Miss Dowie's delightful paper, what she had to tell about the Carpathians and their people was as new as Mr. Stanley's account of his great African forest.

Africa, of course, occupied a prominent place in the proceedings of the Section, one whole day being devoted to it. Dr. Kerr Cross, who has been stationed for many years in the Lake Nyassa region, and is well qualified for scientific observation, read a paper on the interesting plateau country lying between Lakes Nyassa, Hikwa or Leopold, and Tanganyika. The paper gave a most instructive picture of the extensive and varied area with which Dr. Cross is familiar. The information he gave about the little-known Lake Hikwa, east of the south end of Tanganyika, was specially valuable, as it had only been seen at a distance before by Mr. Joseph Thomson and a German explorer. It is brackish, of a long curved shape, and lies in a deep depression of the plateau, its basin being a parched-up wilderness. Though there has been abundant rain on the plateau around, for three years not a drop has fallen in the lake valley. Mr. E. A. Maund described in some detail Matabeleland, where he himself has resided for some years. Dr. R. A. Freeman's account of his journeys in Ashánti and neighbouring regions was of special novelty and value. The paper described a journey through a tract of country in and to the north of Upper Guinea, comprising the territories of Fánti, Assín, Ádánsi, Ashánti, Jáman, and Grúinsi. The tract extends from 5° N. to 10° N., and from 0° to 4° W. The first four countries are inhabited by various branches of the great Ōtshwí family, and the remainder by certain pagan aboriginal tribes, and by numbers of Wongára or Mandingo immigrants. Journeying from Cape Coast, through Ashánti to Bontúku, the capital of Jáman, the author crossed three zones of country: (1) open country covered with low bush about 30 miles broad; (2) dense forest about 180 miles broad; (3) open park-like country which, alternating with grassy plains, seems to occupy the greater part of Central and Eastern Africa. On arrival at Kumassi, the capital of Ashánti, the author was received by the king and principal chiefs with great ceremony, the court of Kumassi retaining much of its former splendour. The town of Kumassi is much dilapidated, but presents many relics of great interest. Jáman is a kingdom situated to the north-west of Ashánti, about 9300 square miles in extent; its capital, Bontúku, is a large town closely resembling in appearance the towns of the Tawarek and Upper Niger. It is inhabited almost exclusively by Mahommedans, and forms an important slave depot, as do also the Grúinsi towns of Wà and Bóri. The commercial resources of the tract of country here described are considerable; over the whole of it gold is fairly plentiful, and the forest abounds in rubber plants both in the form of trees and vines. Hard woods are very plentiful, and are of great value in Europe, notably the Odúm and Pappáo, both of which trees reach a height of nearly 200 feet. The Kola nut also, which grows abundantly in the forest, has a great and increasing commercial value. The country is intersected by several considerable rivers which might be easily rendered navigable, and thus form great highways of trade. There are, moreover, no special obstacles to the construction of railways, and the district may thus be expected to form one of the great commercial centres of the future.

Mr. J. S. Keltie's paper on the Commercial Geography of Africa dealt with the varied physical conditions of the continent, and endeavoured to indicate the bearings of these on its industrial development and colonization. It was shown that the vast tropical region, in which Nature is most exuberant, is of insignificant commercial value compared with the countries along the Mediterranean and the region south of the Zambesi. Central Africa will only become of commercial value when, as in North and South Africa, man is able actively to interfere; the spontaneous animal and vegetable products of tropical countries can never be of great commercial importance.

Mr. A. Silva White followed with a paper showing in detail the partition of Africa among the Powers of Europe.

Two other papers of special African interest were read by Mr. Cope Whitehouse and Dr. Schlichter. The former sought to prove that in the oldest Ptolemaic maps a depression (Lacus Meridis) is shown, exactly corresponding to the Wadi Ráyan and Wadi Mullah. Dr. Schlichter, in an elaborate paper, discussed the whole subject of Ptolemy's knowledge of North-East Africa, and sought to show that many of his positions exactly corresponded with those of modern maps, obtained by quite recent explorations.

There were three papers connected with Asia. Mr. Theodore Bent's paper on his recent explorations in North-Eastern Cilicia was mainly of an archæological character. Sir Frederic Goldsmid read a paper on a railway through Southern Persia, as a link of communication in the great railway route that will one day connect England with India. Surveys and reports by recent travellers have rendered it easy to supply this link, which may be appropriately called the Baghdad-Bandar-Abbas section, or, more minutely, the Baghdad-Shiráz and Shiráz-Bandar-Abbas sections. As to the route from Bandar-Abbas to Karáchi on the east, and from Tripoli to Baghdad on the west, any doubts or difficulties that present themselves are already ripe for discussion, and their solution cannot be treated as dependent upon further travel and research. It is proposed to carry the line from Baghdad through Persian Arabistan, either by way of Dizful and Shustar, continuing along the recognized track from the latter place to Bebehan; or by an alternative route down the left bank of the Tigris, and *viâ* Haweizah to Ahwaz, whence Major Wells, R.E., has furnished full details of route from his own experiences. The same officer has made, moreover, very valuable suggestions on the mode of reaching Shiráz from Bebehan.

Mr. H. F. B. Lynch dealt with an allied subject in his paper on new trade routes into Persia. In the course of the paper Mr. Lynch, from his own personal knowledge, gave much information as to the physical geography of Persia, and especially the region watered by the Karun River.

South America was dealt with in two papers. Mr. J. W. Wells described the physical geography of Brazil in its bearing on the industrial development of the country, and M. A. Thouar sent an abstract narrative of his journeys during the past few years in the Argentine, Peru, and Bolivia.

Mr. Coutts Trotter gave a most useful summary of exploration in British New Guinea in recent years, dealing mainly with Sir W. Macgregor's journey to the summit of the Owen Stanley Range, already described in NATURE.

Dr. H. R. Mill gave a *résumé* of his investigations on the vertical relief of the globe, details of which have been published in the *Scottish Geographical Magazine*. He also gave an interesting account of his observations on the methods of teaching geography in Russia and of Russian geographical text-books.

Mr. Henry T. Crook's paper on the present state of the Ordnance Survey, and the paramount necessity for a thorough revision, led to the Sectional Committee's requesting the Council of the Association to move the Government to take steps for the rapid completion of the Survey, and for rendering the Ordnance maps much more accessible for purchase by the general public than they are at present.

ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

ON Thursday, September 4, after the President's address, a paper by the Rev. F. O. Morris, on the doctrine of hereditism was read, and gave rise to a lively discussion, or rather a chorus of condemnation of the views advanced by the author.

In a paper by Mr. Horatio Hale, which forms the introduction to the Report of the North-Western Tribes of Canada Committee, attention was called to some of the chief peculiarities of British Columbian ethnography, the great number of linguistic stocks which are found in this comparatively small territory and the singular manner in which they are distributed, especially the surprising variety of stocks clustered along the coast as contrasted with the wide sweep of the languages of the interior. All the languages of British Columbia have a peculiar phonology; their pronunciation is singularly harsh and indistinct; and it would appear that this is due mainly to climatic influences, for, south of the Columbia River, the harsh utterance suddenly ceases and gives place to softer sounds.

A paper by Mr. J. W. Fawcett was read on the religion of the Australian aborigines. The author stated that the Australians believe in a Creator, in a future life, and in good and

evil spirits. They have a strict sense of right and wrong, and have religious ceremonies, which are always held in secret on ground that is held very sacred, so much so that if it is touched by the foot of a white person it loses all sanctity.

Another paper by Mr. Fawcett was read on the aborigines of Australia, in which he traversed certain statements that have been made by Mr. Carl Lumholtz.

On Friday, September 5, Mr. F. W. Rudler, who, in the absence of Dr. Evans, presided over the Section, read a paper on the present aspect of the jade question.

It has long been known that implements worked in jade have occasionally been found in ancient graves in France and Western Germany, and in certain Neolithic stations on the Swiss lakes. Some of these implements are wrought in nephrite, or true jade, and others in jadeite. As neither of these minerals had been found *in situ* in Europe, while both were known to occur in Asia, it had been conjectured that the European jade implements must have had an Oriental source, and that either the implements themselves or the raw materials of which they were made had been brought to Europe in prehistoric times. But within the last few years Herr Traube, of Breslau, has discovered nephrite *in place* near Jordansmühl, and near Reichenstein, in Silesia. Pebbles of nephrite have also been recently recorded, by Dr. Berwerth, from the valleys of the Mur and the Sann, two rivers in Styria. A pebble believed to be of jadeite was found by M. Damour at Ouchy, on the Lake of Geneva, and the same mineral has been recorded from Monte Viso, in Piedmont.

Jade implements are found along the coast of British Columbia and Alaska, and it has been suggested that these, or the raw jade, had been obtained from Siberia, where the occurrence of nephrite is well known. Dr. G. M. Dawson has, however, recorded the discovery of small boulders of jade, partially worked, in the lower part of the Frazer River Valley; and Lieutenant Stoney has obtained the mineral *in situ* at the Jade Mountains in Alaska, 150 miles from above the mouth of the River Kowak.

The present aspect of the jade question is, therefore, quite different from that which it presented when the late Prof. H. Fischer and others strongly favoured the view that the jade implements of Europe and America had an exotic origin. In both these continents jade has now been found *in situ*, and it seems, therefore, probable that the material of the implements is indigenous, as maintained by Dr. A. B. Meyer for those of the Old World, and by Dr. Dawson, Prof. F. W. Clarke, Mr. G. F. Kunz, and others, for those of the New World. If future discoveries should confirm the indigenous view, the famous jade question will be lifted out of the domain of anthropology.

A paper entitled "Is there a Break in Mental Evolution?" was contributed by the Hon. Lady Welby. The introduction of the idea of "ghost" marks mental degeneration. If the idea of "spirit" had its origin in primitive man, it would have to undergo the most primitive tests, viz. *contact*, *odour*, and *flavour*. The author contended that we must either suppose an absolute break and reversal in the evolution of mind wherein a permanently distorted picture of the universe is created, and the real and significant suddenly abdicates in favour of the baseless and unmeaning; or, we must ask whether there is some reality answering to these crude conceptions, which thus form part of a continuous mental development, and may be described as faulty *translation*, rendered inevitable by the scantiness of primitive means of analysis and expression.

To adopt the first alternative is to strike a blow at the doctrine of continuous ascent in evolution: while the second might lead us to conclude that what we want is a greater power of interpreting primitive ideas as expressed in myth and ritual, notably in relation to recent developments and present researches in psychology itself and the psychological aspects of language.

Dr. Phené read a paper on an unidentified people occupying parts of Britain in pre-Roman-British times. From extensive investigations in France, Italy, &c., he showed that certain names and words continued from Britain to the Mediterranean along ancient routes of traffic, and the works and constructions along, and in connection with, the same routes, were so alike as to be identical in design. These constructors and merchants were not British, and the traffic appears carried back long prior to the time of Cæsar.

The other papers read were on the Yourouks of Asia Minor, by Mr. T. Bent; the Aryan cradle-land, by Mr. J. Stuart Glennie; and reversions, by Miss Nina Layard. The Report of the Notes and Queries Committee was also presented.

On Monday, September 8, Dr. G. W. Hambleton read a paper on physical development, in which he described the results of a practical experiment in physical development which is being carried on at the Polytechnic Institution. Fifty per cent. of the 200 members of the author's Physical Development Society had obtained an increase of chest-girth of one inch and upwards, the average increase being a little less than two inches. In one case the increase was $6\frac{1}{2}$ inches. The increase has taken place in small as well as in large chests, whether the men were tall or short, under or over twenty-one years of age, and with or without gymnastic training.

Dr. Munro described some archæological remains bearing on the question of the origin of the Anglo-Saxons in England. The relics in question have been recently brought to light on the coasts of Holland and North Germany, more especially in Friesland and the low-lying district northwards as far as the River Elbe, and show a remarkable similarity to Anglo-Saxon antiquities found in England. Dr. Munro also contributed a paper on prehistoric otter and beaver traps, in which he described some curious wooden machines which have been discovered in various peat bogs in different parts of Europe, and of which hitherto no satisfactory explanation has been offered.

Rev. E. Maule Cole read a paper on the Duggleby "Howe." This great mound on the Yorkshire wolds was opened by Mr. J. R. Mortimer on behalf of Sir Tatton Sykes in July last. The diameter of the mound was found to be over 120 feet, and the height was originally about 30 feet. In the process of excavation it turned out that there was an outer mound of rough chalk, of some 15 feet or more in thickness, surrounding an inner mound, and that the centres of the two did not exactly correspond. In the grit and lower clay were found fifty-three deposits of burnt human bones, but without any urns. Some beautiful flint weapons and tusks of the wild boar were discovered with human bodies in graves cut out of the solid rock.

Mr. J. R. Mortimer described a Romano-British graveyard in the parish of Wetwang-with-Fimber, which he believes to be the site of the long-lost Delgovitia.

Mr. Mortimer also contributed a paper on a supposed Roman camp at Octon, close to the road from York to the coast. The rectangular corners of the camp, and the width of the ditches ($7\frac{1}{2}$ feet) at the bottom, encourage the belief that this is not a British work, but Roman.

The other papers were on minute Neolithic implements, by Dr. H. C. March; indications of retrogression in prehistoric civilization in the Thames Valley, by Mr. H. Stopes; and a suggestion as to the boring of stone-hammers, by Mr. W. Horne.

On Tuesday, September 9, Dr. Wilberforce Smith read a paper on stethographic tracings of male and female respiratory movements. The investigation of the author, so far as it has yet proceeded, totally fails to confirm the view commonly put forth in physiological text-books that there is a natural difference between the sexes in regard to respiratory movements. Mr. W. F. Stanley exhibited and described a new spirometer, constructed upon the principle of the class of gas-meters used for testing. Dr. J. G. Garson contributed some notes on human remains found by General Pitt-Rivers, at Woodyates, Wiltshire. The measurements of the limb bones showed the stature of the persons to have been greater than that of those who were interred in Woodcuts and Rotherley. The characters of the skulls showed a considerable range of variation in size and proportion, indicating that they did not belong to a homogeneous people, but to individuals of mixed race. Variation was found not only in the facial portion, but also in the form of the calvaria. As far as the author was able to judge, the mixture is due to crossing between the Romans and the early dolichocephalic British race. There is no evidence of mixture arising from crossing between either of these races and the Celtic population.

The following papers were also read: Mr. B. Hollander, old and modern phrenology; Dr. Wilberforce Smith, diagrams for reading off indices; General Pitt-Rivers, excavation of the Wandsdyke at Woodyates; together with the report of the Anthropometric Laboratory Committee; report of the Prehistoric Inhabitants Committee; report of the Nomad Tribes of Asia Minor Committee; report of the North-Western Tribes of Canada Committee; and the report of the Indian Committee.

THE VOLCANOES OF THE TABLE LAND OF MEXICO.

THE following account with extracts is based on information gathered from a notice which appeared in the *Philadelphia Public Ledger*, sent us by the courtesy of Prof. Heilprin. He had been obliged to issue the notice in advance of the full publication of his papers by the Academy of Natural Sciences of Philadelphia, as he found on his return many varying reports of the work carried on in Mexico.

Amongst the most recent determinations of the heights of these great volcanoes are those made by the Mexican Expedition lately organized under the auspices of the Academy of Natural Sciences of Philadelphia. Prof. Heilprin has recently placed on record his barometric determinations of the four loftiest summits of the Mexican Republic: Orizaba, Popocatepetl, Ixtaccihuatl, and the Nevado de Toluca. In this paper he points out that the highest point of the Republic is the Citlaltepetl or Star Mountain, more commonly called the Peak of Orizaba, and not Popocatepetl as is generally assumed by the Mexican geographers. All his observations were made "with a carefully tested aneroid barometer, and the data were computed from almost simultaneous observations at the Mexican Central Observatory of the City of Mexico, and from barometric readings made at the sea-level at Vera Cruz. The equable condition of the atmosphere, at the time these observations were made, rendered the possibility of the occurrence of possible errors of magnitude almost *nil*." From the above quotation it will be seen that great care was taken to eliminate all errors and to get as accurate measurements as possible.

Popocatepetl was commonly accepted as the highest peak, and Alexander von Humboldt recorded it in 1804 as 17,720 feet. Since the above date, many trigonometrical surveys have been made, and the results vary from 17,200 feet to a few feet over 18,000. The latest measurements by Prof. Heilprin give 17,523 feet as the height, being 200 feet lower than Humboldt's estimate. This determination has been derived from the newer data which have been made possible through the levelling of the Mexican Railway, which was constructed a few years since, while geographers have almost universally accepted Humboldt's determinations and figures. From these new data it was shown that "the estimate of the elevation of the city of Mexico (7470 feet), and of the adjoining plateaus, which have served as a basis for most of the angle measurements of the mountains, have been placed 123 feet high. Allowing for this excess, a striking correspondence is established between the early measurements and those obtained in the spring of the year by the Philadelphia Expedition."

Prof. Heilprin and Mr. F. C. Baker made the ascent of this mountain on April 16 and 17, reaching the "rim of the crater at 11.30 o'clock on the morning of the 17th, and the culminating point early in the afternoon of the same day. Little difficulty was encountered in the ascent beyond that which is due to inconvenience arising from the highly rarefied atmosphere. The snow-field was found to be of limited extent, and not more than from five to ten feet in depth, and was virtually absent from the apex of the mountain. The surprisingly mild temperature of the summit, 45° Fahrenheit, rendered a stay of several hours in cloudland very delightful."

The supposed second highest summit of the Mexican Republic is the mountain of Orizaba or Citlaltepetl, and "the results of Prof. Heilprin's determinations show more marked variations from those of most of the earlier investigators, and more particularly from those of Humboldt." The height determined by Humboldt by means of angles taken from near the town of Galapa, was 17,375 feet, while Ferrer's determination in 1796 gave 17,879 feet, as recorded in the Transactions of the American Philosophical Society. The latter value is generally adopted by the German geographers, while the Mexican geographers, on the other hand, adopted the measurement of Humboldt, or "that which was obtained by the National Commissions of 1877, indicating a height of 17,664 feet."

The following is a short account of the ascent:—"Prof. Heilprin, with three of his scientific associates and eleven guides, made the ascent on April 6 and 7, or ten days before the ascent of Popocatepetl. The last camp, at a height of some 13,000 feet, was left shortly before five o'clock in the morning of the second day, and after a difficult and continuous struggle of twelve hours through loose boulders, sand, and a much cut up ice cap, the party—or rather the fragment which succeeded in holding out—finally reached the rim of the crater." At this point, about 120 feet below the apex of the cone, Prof. Heilprin made a measurement which indicated a total height of 18,206 feet, exceeding Ferrers and Humboldt's measures by 325 and 800 feet respectively.

As upon Popocatepetl, "the snow cap, upon Orizaba, although arising 2400 feet, or nearly half a mile above the summit of the highest peak of the Alps, was a comparatively insignificant development." The time spent on the summit was short, lasting only a quarter of an hour, and then followed the descent through the numerous seracs of the ice, which proved most difficult. At a little past eight o'clock in the evening the camp was reached, thus completing "a remarkable round of mountain climbing of fifteen successive hours."

Prof. Heilprin describes the views from the slopes of the mountain as surpassingly grand, exceeding anything that he had seen in his travels. "Far off to the west the giants Popocatepetl and Ixtaccihuatl were clearly outlined against the sky at a distance of about 100 miles, while to the east and south the eye wandered over a seemingly endless expanse of plateaus and lowlands, penetrating through a series of successive cloud planes." The measurements of both the peaks of Orizaba and Popocatepetl were made under very similar conditions of the atmosphere; the same instruments were used, and there was only an interval of ten days between the measurements, which points to the conclusion that "the first place among Mexican volcanoes must be accorded to the Star Mountain."

On the 27th of the same month the ascent of the third highest peak, called the Ixtaccihuatl, was made. The general appearance of this mountain differs considerably from the two mentioned above; instead of having a symmetrical or conical outline, it has "a strong flowing crest, covered with a heavy deposit, some 75 or 100 feet in thickness, of snow and ice, which serves readily to distinguish the familiar 'White Woman' of the plain of Anahuac."

The measurement obtained by Prof. Heilprin of the height of this mountain is 16,962 feet, which height differs by 800 to 1300 feet respectively, from those formerly obtained by the Mexican geographers. Sonntag, in the year 1857, also determined its height, and his result accords very closely (within 11 feet) with Prof. Heilprin's. The temperature on the summit was found to be lower than that on either of the other two peaks, the thermometer indicating 32° F.

In view of the close proximity of this mountain to Popocatepetl, it is difficult "to account for the low value given by Humboldt and the Mexican geographers. So nearly do they appear of equal height that the eye at first fails to distinguish which of the two summits is the higher. German geographers, however, in a few cases, have adopted Sonntag's measurements, neglecting, as in the case of Popocatepetl, to make allowances for the error in this case of 125 feet which is indicated by the levelling of the Mexican Railway."

On April 2, Prof. Heilprin and Mr. Baker ascended the fourth highest summit of the Republic, the Nevado de Toluca. The ascent of this mountain is much easier than the others, and the summit can be reached on horseback to within a distance of 900 feet. The rim of the broken crater "is extremely ragged and narrow, descending with equal abruptness to the inner and outer faces of the volcano. At some points the crest is so attenuated that it can be readily straddled." The height of this mountain was found to be 14,952 feet, which approximately corresponds to the mean between Humboldt's determination and those made by a class of students from the School of Engineers of the city of Toluca.

The results of the measurements of this mountain are very divergent, as will be seen by the following list. La Pérouse, in 1786, gave the height as less than 13,000 feet. The British Hydrographic Chart of 1872 gave 14,970 feet, and this estimate is the one which is generally followed by the English and a number of American geographers. Malespina, in 1791, by means of angles taken from positions near Fort Mulgrave, determined the height to be 17,851 feet, while Tebenkoff reduces this figure by about 900 feet.

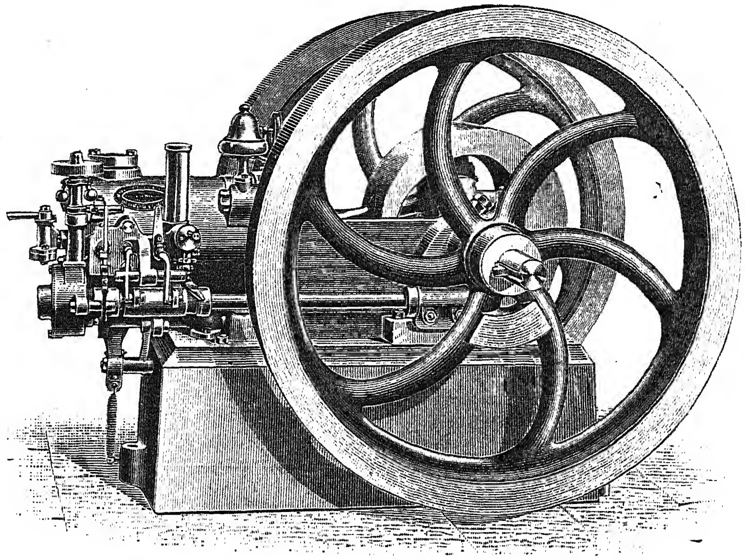
The most carefully conducted series of measurements are "those which were made by Mr. W. H. Dall, under the auspices of the United States Coast Survey, 1874. These yielded results ranging from a little more than 18,000 to nearly 20,000 feet. The measurements were made from distances 69, 127, and 167 miles, and it is more likely that the discrepancy in the results obtained is due to an uncertainty regarding the actual position of

the mountain." Even in these latter measurements we have results in which the extreme variation is about 2000 feet, and this distance compared with 4 miles (about the height of the mountain) is a large quantity, and shows that a still more accurate determination must be made before its height is placed beyond doubt.

From the results of the measurements of the heights of these four mountains, the existing evidence seems to point to the "Star Mountain" of Mexico, the peak of Orizaba, with its 18,200 feet, as the culminating point of the North American continent.

A NEW ELECTRIC LIGHT OTTO GAS-ENGINE.

ELECTRIC lighting is becoming so universal in all parts at the present day, that we give an illustration of the latest form of gas-engine made by Crossley and Brothers, Manchester. This engine, called the "High Speed Electric Light Otto Gas-Engine," runs at 250 revolutions, and is designed throughout to run at this exceptional speed. It is fitted with most of the latest improvements, such as Crossley's tube ignition, patent timing valve, and a special electric light governor, which makes it a very steady running engine for this kind of



work. The makers claim that "electric light lamps can be driven direct from the dynamo without fitting the dynamo with a fly-wheel or disc, as has hitherto been done when a small gas or steam-engine has been used; and that the light will be as absolutely steady as is possible with any form of motive power."

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for September contains an article, by Prof. H. A. Hazen, on Espy's experiments on storm generation and the liberation of latent heat on cloud-formation; these were made about 50 years ago, and Prof. Hazen states that they have never been checked, but have been accepted without question by meteorologists. His own experiments have led to different results, and he finds that deposition from *moist* air does not set free latent heat.—E. B. Garriott contributes an article on the origin of storms; he attributes their development to an excess of heat from the earth's surface by radiation, and their progressive movement to the precipitation of aqueous vapour at a considerable elevation, while the direction in which they move is regulated by the disposition of cold dry air found in areas of high pressure. For a verification of these facts, he points to the storms of the North American continent, a large majority of which originate over the great plateau region in the lee of the Pacific coast ranges of mountains, and advance towards the regions of greatest moisture which embrace the Great Lakes, the Gulf of Mexico, and the valleys of the principal rivers.—M. Faye has a supplementary article on trombes and tornadoes, for the purpose of introducing the figures illustrating his previous papers in the *Journal*.—Mr. M. W. Harrington contributes an instructive paper on forests and soil temperatures. He has taken various sets of observations published in Germany and elsewhere, amounting altogether to 150 years, and has discussed them by harmonic analysis for various periods, with the view of finding the distribution of temperatures in the soil within and out of the forest, at any depth, and at any time. The greatest difference between the forest soil and that of the open fields is at the surface, the mean difference of forest below open field being about 3°, but below the surface the differences between forest and open field do not progress uniformly. There appears to be a gain of heat in the upper soil of the woods which the open fields do not have.

American Journal of Mathematics, vol. xiii., No. 1 (Baltimore, October 1890).—The opening paper (pp. 1–52), entitled "Ueber die zu der Curve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ im projectiven Sinne gehörende mehrfache Ueberdeckung der Ebene," is by Mellen Woodman Haskell, a name not familiar to us, but belonging evidently to a mathematician of power. The discussion is exhaustive, and is fully illustrated with diagrams in the text, and two large-paged tables containing shaded diagrams. The reader who is familiar with Klein's "Ueber eine neue Art Riemann'scher Flächen" will easily follow the author's work. An index supplies the student with a ready key to the matters handled.—Prof. Cayley (pp. 53–58), in a note on a soluble quintic equation, discusses one of the equations given in Mr. Young's paper, "Soluble Quintic Equations with Commensurable Coefficients" (vol. x. pp. 99–130). The example considered is $x^5 + 3x^2 + 2x - 1 = 0$, the solutions of which the author shows admit of being put in much simpler form than those given by Mr. Young.—Then there is an instalment of a course of lectures delivered at the Johns Hopkins University during the months of January and February 1889, by Oskar Bolza. Its title is "On the Theory of Substitution-groups, and its Applications to Algebraic Equations." The paper is divided into two parts. The *first* develops the fundamental propositions, and concludes with a

short sketch of the extension of the theory to groups of operations in general. The *second part* deals with Galois's theory of algebraic equations, in particular their solution by radicals. The material is taken from Jordan, "Traité des Substitutions"; Serret, "Cours d'Algèbre Supérieure"; and Netto, "Substitutionen-Theorie." Other authors have been consulted, and the whole has been strongly influenced by a course of lectures on the subject by Prof. Klein. The editor of the *Journal* expresses a belief that this development will prove extremely useful to students.—This being the opening number of a new volume, is graced by a fine portrait of Prof. Cayley, which gives a very truthful presentment of this eminent mathematician's characteristic features.

IN the numbers of the *Journal of Botany* for August and September is an interesting mycological contribution from Dr. A. Barclay, describing some of the Ustilagineæ and Uredineæ parasitic on cereal crops and other crops in India. The most important of these are the following: *Puccinia Sorghi* on *Sorghum vulgare*, *Melampsora Lini* on *Linum usitatissimum*, *Uromyces Pisi* on *Cicer arietinum* and on *Lathyrus sativus*, *Puccinia Fagopyri* on *Fagopyrum esculentum*. Mr. W. H. Beeby contributes a paper on the British species of *Sparganium*; as regards fertilization, he states that they are rarely visited by insects; they are all proterogynous, and mostly wind-fertilized. Among the "Short Notes" is the very interesting record of the occurrence of the very rare *Ranunculus ophioglossifolius* in Gloucestershire.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 29.—M. Duchartre in the chair.—On the theory of infectious disease, of recovery, of vaccination, and of natural immunity, by M. Ch. Bouchard.—On the absorption of carbon monoxide by rocks, by M. Berthelot. Observations in mines after explosions have been said to indicate that the rocks of which the walls are constituted possess a specific property by virtue of which they retain carbon monoxide in their pores for a longer period than other gases. From some experiments made to investigate this question, M. Berthelot finds that the volume of carbon monoxide absorbed by argillaceous rocks and given up by them, is sensibly identical with the volume of air absorbed and given up under the same conditions. Hence, rocks impregnated with carbon monoxide owing to an explosion do not retain it because of any specific action peculiar to this gas.—On acetylene condensed by the silent discharge, by the same author. An examination of the result of the condensation of acetylene by means of the silent discharge appears to indicate that it differs in character from that obtained by the influence of heat on the same compound.—Spark spectrum of gadolinium, by M. Lecoq de Boisbaudran. The author gives the wave-lengths of the lines, bands, and flutings characteristic of the spectrum of gadolinium.—On the atomic weight of terbium metals, by the same author. The value found from two experiments was 159·48.—On a new safety-lamp for use in mines, by M. Charles Pollak. The lamp is an incandescent one. It weighs about 1800 grammes, and will give a light equal to 0·7 or 0·8 of a candle-power for twelve hours.—Observations of Comets Coggia and Denning (*b* and *c* 1890), made with the great equatorial of Bordeaux Observatory, by MM. G. Rayet, L. Picart, and Courty. Observations for position were made on July 27 and 29 and on August 6 in the case of the former comet, and on September 14 and 15 in the case of the latter.—Thermo-electric researches, by MM. Chassagny and H. Abraham. The authors find, from some experiments, that the variation in the electromotive force produced by heating the poles of a copper-iron couple is practically constant between 0° and 100° C. It is therefore possible that thermo-electric elements may serve as standards of electromotive force better than electro-chemical cells. The same results were found with couples two months old as with those only two days old.—On a fungus of the Mucedinean group, by M. Raphael Blanchard.—On the properties of the principal natural colouring-matters of yellow silk, and their similarity to those of carrotin, by M. Raphael Dubois. Evidence is adduced to show that raw yellow silk owes its colour to the presence of a substance analogous to the colouring-matter recently extracted from the *Diaptomus denticornis*, by M. Blanchard, and considered as a carrotin of animal origin.—The identity in the structure of lightning and discharges from an induction machine, by M. E. L. Trouvelot.

STOCKHOLM.

Royal Academy of Sciences, September 17.—On the discovery of cerium minerals and columbite, and on the occurrence of microlite, by Baron Nordenskiöld.—On the discovery of pinakiolite, trimerite, and centrolite, by G. Flink, communicated by Prof. Brögger.—On inclosures of dissimilar rocks in some Scandinavian diabases, by Herr H. Bäckström.—On maxima and minima by double integrals, by Dr. O. Kobb.—On a generalization of the Bernoullian functions, and their connection with the generalized series of Riemann, by Dr. A. Jonquière, of Basel.—Some formulæ of Bierens de Haan, by Dr. Lindman.—Études de la distribution spectrale de l'absorption dans le spectre infra-rouge, by Dr. K. Ångström.—On phenyl-totyl and benzylen-diamin, by Dr. Söderbaum and Prof. Widman.—Derivatives of ortho-amido-benzyl-alcohol, iii., by Dr. Söderbaum.—Researches on the conductibility of the caloric in porous humid bodies, by Herr S. A. Andrée, C.E.—On the new edition of the collected works of Galileo, by Dr. G. Eneström.

AMSTERDAM.

Royal Academy of Sciences, September 27.—Prof. van de Sande Bakhuysen in the chair.—Prof. Schoute dealt with some general theorems relating to directly similar plane figures.—Prof. Hubrecht described phases in the early development of the shrew's placenta, and called attention to the fact that, whereas in the hedgehog the uterine epithelium disappears—the subepithelial stroma forming the maternal contribution to the placenta—in the shrew, on the contrary, this contribution is directly derived from the epithelium of the uterus.

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THURSDAY, OCTOBER 16, 1890.

ANALYTICAL MECHANICS.

A Treatise on Analytical Mechanics. By Bartholomew Price, M.A., F.R.S., F.R.A.S., Sedleian Professor of Natural Philosophy, Oxford. Vol. II. Dynamics of a Material System. Second Edition. (Oxford: Clarendon Press, 1889.)

A SECOND title-page describes the present work as volume iv. of "A Treatise on Infinitesimal Calculus," so that Prof. Bartholomew Price's well-known four volumes may be taken to represent the curriculum of the Infinitesimal Calculus and its applications for the mathematical student at Oxford.

To one accustomed to the style of the text-books in use at Cambridge, the contrast is very striking; the Oxford student is much to be envied for the leisurely and luxuriant way in which the subject is here presented, which follows on the lines of Lagrange and Laplace, and utilizes all the resources of analysis. A student who has been through the present work will be prepared to appreciate the purely geometrical form in which the Newtonian methods, insisted upon at Cambridge, would present some of the theorems in a more fundamental and incisive form; but to our mind the Cambridge system is inferior, which ostensibly insists on the purely geometrical methods before allowing the student to make use of the power of analysis.

Although Newton claims to be one of the inventors of this Calculus, and must have employed its methods in the discovery of his theorems, yet he carefully covered up all traces of the analytical scaffolding, and exhibited a theorem in the "Principia," like a Greek temple, in pure geometrical form.

His influence on his successors was too great when they attempted to follow in the same lines, with the consequence that our insular school of mathematics lagged hopelessly in rear of Continental progress.

Although prescribed as the text-book at Cambridge, the "Principia" is not studied in the original Latin, as Newton wrote it, from one end to the other; but the student makes use of commentaries and selections, which, in accordance with the regulations, he professes to appreciate and apply, before knowing even by sight the supposed mystifying symbols of dy/dx and $\int ydx$.

We might as well send out our soldiers armed with muzzling loading guns, or even bows and arrows, to meet a continental army equipped with the most recent inventions of magazine rifles and breech-loading artillery.

Thus the late R. A. Proctor could write that, although a wrangler, he knew nothing of the Differential Calculus till some time afterwards, when he had to pick it up of himself; however, by a recent regulation, only passed a few weeks ago, a most stupendous change has been made in the Mathematical Tripos, by prescribing a certain very elementary course of Analytical Geometry and the Calculus in the First Three Days.

At Cambridge the large number of candidates for mathematical honours acts as a check to change; and as the same papers have to serve for such widely different classes as the wranglers and the junior optimes, it may happen that a candidate who merely writes out book-work will beat a better mathematician who is tempted to try the difficult questions.

The number of students in mathematics at Oxford is much smaller, and the standard for honours is higher; so that we can take this treatise on Infinitesimal Calculus and contrast it with the extracts from Newton's "Principia," to illustrate the relative standards.

Under the enthusiastic influence of a Sylvester we may see the mathematical school at Oxford the first in this country, as it was two hundred years ago, in the days of Wren, Wallis, Keill, and the founders of the Royal Society, which had its origin in Oxford.

At the outset of the Dynamics of a Material System in space, it is necessary to discuss a number of theorems in solid geometry on the distribution of principal axes and the associated theorems of confocal quadrics (chapter i.); also the kinematics of a rigid body, involving the composition and revolution of angular velocities, and the transformation of co-ordinate axes (chapter ii.).

The author could simplify the distinction between the two systems of rectangular axes by adopting Maxwell's comparison with the screw, right-handed or left-handed. All specifications of rotation as clock-wise, or counter-clock-wise, are ambiguous; because the direction changes as we pass from one side to the other of the clock face. Standing at dusk about a quarter of a mile from a wind-mill, nearly in the plane of the sails, it is possible by a slight mental effort to change the apparent direction of rotation, and back again, as often as we please.

The author does not permit himself the use of the elliptic functions; or else he would have found the wonderful chapter i., t. ii., of Halphen's "Fonctions Elliptiques" of great service in giving the representation of the cosines of the angles which a movable straight line or a movable set of three rectangular axes makes with three fixed rectangular axes. Much of the subsequent work on Euler's three angles, the integration of his equations of motion, and of the spherical pendulum, &c., could be completed and the integrations effected by the use of Halphen's formulas.

Dynamics proper is introduced in chapter iii., where D'Alembert's principle is employed to establish the equations of motion of a material system, with the subsequent corollaries of the independence of the motions of translation and rotation, and the principles of the conservation of momentum and energy.

It is more the fashion now to dispense with D'Alembert's principle, and to refer immediately to Newton's third law of motion; still, D'Alembert's principle, although a mere corollary, states the thing in such a way as to lead immediately to the formation of the six equations of motion; and by stating it in such a manner as to reduce all dynamical principles to a statical form—"the reversed effective forces and the impressed forces form a system in equilibrium, the internal cohesive forces (stresses) being in equilibrium among themselves"—it was formerly considered that a simplification was effected.

But Maxwell, in his "Matter and Motion," by considering Newton's third law of motion as merely the definition of a *stress*, has been able to restate all the theorems involved in D'Alembert's principle in a few simple sentences, and in a much more convincing form.

It is a pity that the term *vis viva* has been allowed to remain in this last edition, and that it was not entirely replaced by *kinetic energy*: the contrasted term *vis mortua* has been dead for a long time, and *vis viva* should have followed long ago.

The transformations of the dynamical equations into the Lagrangian and Hamiltonian forms are introduced at rather an early stage; and the subject is resumed in the last chapter x., on "Theoretical Dynamics," written by the late Prof. W. F. Donkin. These transformations are merely analytical illustrations of the change of independent variables, the form of the equations depending on whether we express the kinetic energy in terms of the generalized velocities or the generalized momenta.

A clear and expressive notation, somewhat in the style of that found necessary in Thermodynamics, would make these equations more intelligible and convincing; but in any case, the application to definite problems, especially where the geometrical constraints present any peculiarity, is so difficult and refined, that these equations are dangerous weapons to put into the hands of any but advanced students.

The principles of Least Action and of Least Constraint are also introduced here by the author; interesting verifications are thus afforded of well-known problems; but these principles again would not be employed for choice; and although the author pleads in their favour, we think it should not be forgotten that the principle of Least Action was employed to bolster up the Corpuscular Theory of Light.

Newton's principle of mechanical similitude, in the next section, is, however, of great practical importance, and we see its application in the constantly increasing size of our bridges, ships, and guns. In its particular application to naval architecture, a corollary goes by the name of Froude's law (also enunciated by Reech), which asserts that in similar vessels run at speeds proportional to the square root of the length or the sixth root of the displacements, the resistances are as the displacements; and thus the naval architect is able to infer, from the known performance of a ship or a model, what to expect on a different scale. When we make, in any two similar machines of the same material, the velocities in the ratio of the square root of the linear dimensions, we ensure in this manner that the stress per unit area in the material is the same, and thus the two machines are equally strong; so that this law of corresponding speed is most useful in the practical application of Newton's law of similitude.

A valuable section on Units, No. 9, points out that there are only two systems which need be considered: the British foot-pound-second (F.P.S.) system, and the metric centimetre-gramme-second (C.G.S.) system. The author's numbers for the conversion of one system into the other are not exactly according to the latest determinations; thus it is more accurate to make 1 metre = 39·37079 inches, and 1 foot = 30·4794 centimetres. The metre was originally designed so that the kilometre should be the centesimal minute of latitude, for use in navigation; but taking the sexagesimal minute of latitude as 6080 feet, the Admiralty standard, then the above figures make the length of the earth's quadrant 10,007 kilometres, instead of 10,000, as designed. It has been decided, however, for electrical purposes that 10^9 centimetres should be called a *quadrant*, although about 0·07 per cent. out.

Recent redeterminations of the weight of a metre cube of water, and of the volume of 10 gallons or 100 pounds of water, made with the greatest care, have revealed perceptible discrepancies with former estimates; so that the definition of the kilogramme as a decimetre cube of pure water at its maximum density must be considered a purely academic definition, and not sufficiently precise for legal purposes; the ultimate appeal being to the lump of platinum preserved in the Conservatoire des Arts et Métiers.

A Committee of the British Association is at present engaged in attempting to fill up the gaps in our dynamical terminology: the author introduces the *dyne* and *erg*, due to a former Committee, but not the *kine*, *spoud*, *bole*, and *barad*, recently settled upon as names for the C.G.S. units of velocity, acceleration, momentum, and pressure. The C.G.S. units are too minute for practical purposes, so that electricians now employ the *joule*, of 10^7 ergs, and the *watt* as the volt-ampere, or power doing one joule per second—units based really upon the commercial units of the metre and kilogramme, instead of the centimetre and the gramme. These microscopic units were adopted by the original Committee apparently merely to gratify the fad of making $W = sV$, instead of $1000sV$.

The astronomical unit of mass is defined in § 143; but if it is difficult to measure the volume of a kilogramme of water, the probable error in the determination of this astronomical unit of mass is immensely greater; so that to our mind this unit had better be discarded, and the gravitation constant introduced into the equations, using its provisional value, $10^{-8} \times 6{\cdot}48$ C.G.S. units (Everett, "Units and Physical Constants," § 72).

Chapter iv. discusses the equations of motion of a rigid body expressed in terms of angular velocities and their increments, &c. The author adopts various illustrative methods, but to our mind the simplest procedure is to establish the general equations, with the usual notation $\dot{h}_1 - h_2\theta_3 + h_3\theta_2 = L, \ldots$; and then Euler's equations, &c., follow as particular cases. By adding the terms due to the employment of a movable origin we obtain the form of the Hamiltonian equations required in the discussion of the motion of a body moving in a liquid; and here is a good opportunity for the introduction of Dr. Routh's principle of the Ignoration of Co-ordinates, required to complete the theory of the generalized equations of motion.

Prof. Price could make a very useful book for students of elementary mathematics by taking out and printing separately the part on uniplanar motion and its illustrative examples (chapter v.): the complication of the subject of rigid dynamics is more than doubled when we consider motion in three dimensions; but in two dimensions the subject is within the grasp of most students, who will thus acquire a good working knowledge sufficient for most purposes. At the outset the determination of simple moments of inertia is required, and this involves a knowledge of integration; so that a student, untrained in the Calculus, can make very little headway. It is a pity that the lack of the slight knowledge of integration required for this purpose prevents most of our students from going on to the real study of the pendulum, the motion of the

wheel and wheeled carriage, and of the ballistic pendulum. Prof. Price calls the inventor Captain Robins; but, according to the preface of his "Mathematical Tracts," Robins was of Quaker extraction (like so many other students and inventors of warlike instruments), and his only military employment was as chief engineer of the East India Company, in planning and carrying out their fortifications.

In chapter vi. the rotation of a rigid body about a fixed point is discussed, with applications to the three important problems of motion under no forces with Poinsot's geometrical representation, the motion of the top or gyrostat, and the precession and nutation of the earth's axis. These problems illustrate very strikingly the great increase in complication when we go from plane motion to motion in space. The figure of the herpolhode, on p. 251, shows points of inflexion; but, as the author mentions in § 295, these points of inflexion cannot exist in Poinsot's herpolhode. An elegant geometrical demonstration is given on p. 379 of Sylvester's extension of Poinsot's representation, where confocals to the momental ellipsoid are made to roll upon parallel planes; and now it is possible in certain corresponding herpolhodes for points of inflexion to make their appearance; the analytical and geometrical discussion of this problem has engaged the attention of de Sparre and Hess.

We mentioned at the outset that the author did not permit himself the use of elliptic functions; but apparently he could not resist the temptation of introducing them in the complete solution of Poinsot's motion, the simplicity and elegance of the representation being so great. In the separating case, when the modulus of the elliptic functions becomes unity, the introduction of the corresponding hyperbolic functions would have exhibited an analogous symmetry.

By considering the elliptic functions as defined by plane pendulum motion, some of the results in the motion of the top or gyrostat could have been exhibited by comparison with a plane pendulum; but it must be confessed that the simplicity is not maintained when we investigate the projection of the motion on a horizontal plane, without we introduce functions invented by Hermite, of a higher degree of complication.

In the discussion of precession and nutation, a simplification can be introduced by making use of the observed fact in determining the latitude, that the deviation of the axis of rotation from the axis of figure, although certainly existing, is quite inappreciable in the case of the earth; so that the axes of figure, of rotation, and of angular momentum may be taken as coincident. With this approximation the pole of the earth follows a point 90° in longitude behind the sun or moon with a certain velocity; and now the rest of the calculation of precession and nutation becomes a kinematical problem.

Chapter vii. discusses interesting and important problems of small oscillations and of bodies rolling on each other, *e.g.* of a billiard ball on the table; and chapter viii., on relative motion, is important as showing how far we are justified in applying our dynamical equations to the problems going on around us, considering that they take place on the surface of the earth, which is moving in a complicated manner in space. The corresponding elementary discussion in Maxwell's "Matter and Motion," on the ideas of relative motion, and the modification of the principles of dynamics to make them rigorous, is well worth attention at this point.

The deviation from the vertical of a body let fall down a deep mine, of a projectile from the vertical plane of fire, and the rotation of the plane of oscillation of Foucault's pendulum, are discussed as illustrations of the influence of the earth's rotation in modifying a dynamical question; but considering how slight a disturbing cause, such as a current of air, would be sufficient to mask the effect, we believe that these effects have not yet really been observed.

In Foucault's pendulum a very slight jockeying can make the thing go as we wish; while with artillery fire at long ranges the disturbing cause of deviation or drift quite overpowers any deviation due to the rotation of the earth. Theoretically, Foucault's pendulum, if set swinging in a plane through the rising moon, should continue to follow the moon; and roughly speaking, a shot fired at the rising moon should keep moving in the moving vertical plane through the moon, and would thus fall to one side of its original vertical plane of fire; in a range of twelve miles, and a time of flight of one minute, this deflection would, in the latitude of Shoeburyness, amount to about 71 yards, out of about 1000 yards observed average lateral deviation.

A few simple problems on the vibration of elastic threads and plates are given in chapter ix.; and chapter x., as already mentioned, is occupied by Prof. Donkin's contribution on Theoretical Dynamics.

Throughout the work good collections of illustrative examples are introduced, to test the student in his grasp of the principles given immediately before. If we might make a slight criticism, we should suggest the introduction of some arithmetical exercises on these problems, taken from examples in real life; for, as Sir William Thomson insists, it is from arithmetical applications that the student obtains a real grasp of dynamics; the examples given here only testing his algebraical and geometrical power.

In conclusion, we congratulate the student of mathematics at Oxford on the possession of such an admirable text-book, fully brought up to date in the latest developments.

A. G. GREENHILL.

ANNALS OF THE ROYAL BOTANIC GARDEN, CALCUTTA.

Annals of the Royal Botanic Garden, Calcutta. Vol. I. Appendix—(1) "Some New Species of Ficus from New Guinea," by George King, F.R.S., &c., Superintendent of the Royal Botanic Garden, Calcutta. (2) "On the Phenomena of Fertilization in *Ficus Roxburghii*, Wall," by D. D. Cunningham, F.L.S., &c., Surgeon-Major, Bengal Army. (Bengal Secretariat Press, 1889.)

ABOUT a dozen new species of Ficus are added here to Dr. King's valuable monograph of the figs of the "Indo-Malayan and Chinese countries," which occupies the whole of the first volume of the "Annals." It may be remembered that Dr. King proposed a modified classification of the species of *Ficus*, based upon characters indicating, in his view, the direction of evolution in the genus, beginning with a small group having pseudo-hermaphrodite flowers (*Palæomorphe*),

and ending with another small group (*Neomorphe*) remarkable for having di- or tri-androus male flowers, and the receptacles (fruit) borne in clusters, often very large, on the trunk and branches, sometimes at the very base of the trunk.

Curiously enough, although the other five groups or sections, into which King divides the genus *Ficus*, are all represented among the additional species from New Guinea, neither the oldest nor the newest is; but both are represented there by previously known species, and the *Neomorphe* by some of the most remarkable of the genus. Thus, imperfectly as the flora of New Guinea is known, there are indications of great age and variety. Noteworthy among the species figured in the present work is *Ficus hesperidiiformis*, King, belonging to the section *Urostigma*, which is characterized by having male, female, and "gall-flowers" intermixed in the same receptacles. *Ficus hesperidiiformis* resembles the familiar india-rubber tree, *F. elastica*, but the leaves are larger and the receptacles (fruit) very much larger; the ripe dry ones resembling small oranges, hence the specific name.

Dr. Cunningham's memoir on the fertilization of *Ficus Roxburghii* is an interesting and important contribution to the subject of reproduction, inasmuch as he arrives at some rather startling conclusions with regard to the plant in question.

The relations between certain insects, parasitic in the receptacles of the fig and caprifig, and in various other species of fig, and the fertilization of the flowers, has been investigated in recent times more especially by Dr. G. King, Mr. Fritz Mueller, and Count Solms; and particulars of their results, or conclusions, have been given from time to time in NATURE (vols. xxvii. p. 584, xxxvi. p. 242, and xxxix. p. 246). Nevertheless it may be well to repeat here some of the principal conditions and phenomena of the flowers of figs. In the first place it may be noted for unbotanical readers that the flowers of figs are very small and crowded all over the interior of the receptacle or fruit. Further, that the wall or substance of the receptacle is continuous and closed, except at the apex, where it is provided with a number of closely overlapping scales, rendering ingress, and egress, without eating its way, impossible to any but a very minute insect. I say, without eating its way, because much depends upon whether insects can reach the interior of the receptacles and at the same time carry pollen with them; and writers on the subject, so far as I am aware, have not considered the probabilities of the earlier visiting-insects thus opening a channel for those following. I have also, in another place, suggested the possibility of the scales at the mouth of the receptacle being loosened at the receptive period, and Mr. C. B. Clarke tells me that he has actually observed this to be the case.

The flowers are of four kinds—namely, male, female, neuter, and gall; and they are variously associated, or separated, in different species of *Ficus*. There are indeed five kinds of flowers if we include the pseudo-hermaphrodite flowers of the group *Palæomorphe*. In the cultivated fig (*Ficus Carica*), the flowers are almost invariably all female; and the male flowers of this species are borne by the "caprifig" of the south of Europe and Western Asia. Associated in the same receptacles with the male flowers, and covering the whole of the inside except a ring near the top, are the so-called gall-flowers. Structurally they are female, but instead of bearing seed they nourish the larva of an insect, and the perfect females of this insect are supposed to convey the pollen of the male flowers to the receptacles containing female flowers, the ovules of which are thereby fertilized. The presence of insects in figs seems to be general in the very numerous (500 perhaps) species spread all over the tropical regions of the earth; and the commonly accepted theory is that these insects, in return for the shelter and nourishment received, convey the pollen from the male to the female flowers, so that the association is mutually beneficial. At least this was the theory of Solms and Fritz Mueller. In the introduction to his monograph of the Asiatic figs, Dr. G. King says:—"The exact way in which the females are pollenized is a matter on which I cannot pretend to throw any light. I can only state the problem." Yet a little farther on he states that there can be no doubt that the insect developed in the gall-flowers in some way conveys the pollen of the males to the females in other receptacles, though he found it difficult to understand how this could be effected; and he informed the writer that he had never discovered the slightest evidence of the process, beyond the fact that seeds were formed.

At the instigation of Dr. King, Dr. Cunningham has thoroughly investigated the phenomena of fertilization in *Ficus Roxburghii*, and he arrives at the conclusion that pollen is never, or exceedingly rarely, conveyed to the female flowers, though good seed is abundantly matured.

Ficus Roxburghii is perfectly diœcious—that is, the two sexes are produced on different trees; and the fruit is borne in large clusters on the thicker branches and trunk often at the very base of the same, and extended on the ground.[1] The receptacles are similar in shape to those of the common fig, and from two to three inches in diameter, or sometimes nearly four inches; and the flowers are proportionately large, so that they are easily examined. It may be mentioned, too, that this species belongs to King's section or sub-genus *Neomorphe*, which, in our opinion, exhibits the latest stage in the evolution of the genus.

It would occupy too much space to follow Dr. Cunningham through his investigations, but it will suffice to give some extracts from his concluding remarks:—

"There can be little room for doubt that the phenomena indicate that, while the development of embryos in the female receptacles of the tree is essentially connected with the access of the insects to the receptacular cavity, it is yet normally independent of the introduction of pollen by their agency. The fact that the access of a single insect or of a pair of them only is sufficient to determine the development of ten or twelve thousand embryos, is in itself almost conclusive against the occurrence of any ordinary process of pollination. The obstacles through which a passage has to be forced ere the receptacular cavity is reached are of such nature and amount as to render it almost inconceivable that pollen should be introduced in sufficient quantity, and there is at the same time an absolute want of evidence to show that such introduction takes place. I have carefully examined very many receptacles at various periods shortly after access

[1] A photograph of a tree in fruit forms the frontispiece to the first volume of the "Annals of the Calcutta Garden."

of insects to the cavities, and have never been able to detect any evidence of general distribution of pollen over the stigmatic surface. Examination of individual flowers has given like results; in most cases it has been impossible to find any pollen within the receptacle or cavity, and in the few cases in which any was found it was represented by one or two shrivelled grains adherent to the corpses of insects. It must be borne in mind, too, that, if we accept the hypothesis that the development of the embryos is due to ordinary processes of pollination, we must assume not only that a single insect can convey many thousands of pollen-grains with it in spite of the excessive obstructions to access presented by the ostiolar plug, but that these grains are also methodically and economically distributed, for, unless each stigma were only allowed to appropriate a single grain, the amount introduced would have to be indefinitely multiplied.

"The most important evidence against the occurrence of pollination of any sort as a normal and essential event lies, however, in the fact that the embryo originates, as it does in undoubted cases of development, apart from pollination. The embryo, as a rule—for of course it is possible that pollination and normal evolution may occur in certain individual flowers—certainly arises as an outgrowth of the nucellar parenchyma, outside the embryo-sac, and not as the result of special evolution of any elements contained with in the latter. The embryo-sac up to the period of insect-access and of initial development of the embryo normally retains the characters of a simple uninucleate cell. There is no evidence of the formation of an oosphere, of synergidæ, or of antipodal cells within it, and it is only subsequent to commencing evolution of the embryo that the primary nucleus is replaced by a large number of secondary ones, which are apparently related to the elaboration of food material for the growing embryo, when it gains access to the cavity of the sac.

"But if this be so, if pollination be unnecessary, why should the access of insects be essential to the development of embryos? The phenomena presenting themselves in connection with the male flowers of gall-receptacles appear to afford a clue to answering this question. It is just as impossible for the male flowers to come to perfection—just as impossible for perfect pollen-grains to be developed without the access of insects to the gall-receptacles—as it is for embryos to be developed in female ones under parallel circumstances.

"The development of embryos in *F. Roxburghii*, then, appears normally to be an asexual process dependent on hypertrophic budding of a specialized portion of the nucellar parenchyma, and it appears not improbable that the phenomenon is not peculiar to the species, but is the rule in the case of other figs also. This, of course, requires further investigation; but in the only instance in which I have yet had time to examine the matter—in the case of *F. hispida*—there can be no doubt that it is so."

From the foregoing extracts it will be seen that Dr. Cunningham insists on two extraordinary phenomena—namely, the impossibility of the formation of pollen in the absence of insects, and the formation of embryos by budding outside of the embryo-sac instead of sexual development. As to the first, improbable as it may seem, I am assured by two or three independent observers, who have had opportunities of testing Dr. Cunningham's work, that they have arrived at the same conclusions. As to the second, the asexual formation of embryos is not so very rare an occurrence, according to Strasburger in his elucidation of polyembryony and the so-called parthenogenesis. Then as to the whole, the phenomena would seem to point to the extinction of sexuality. The points are that the development of both pollen and embryo is due to a stimulation of the tissues caused by the punctures of insects. Therefore Dr. Cunningham might with more propriety have entitled his paper "The Phenomena of Non-Fertilization." W. BOTTING HEMSLEY.

SYNONYMY OF THE POLYZOA.

A Synonymic Catalogue of the Recent Marine Bryozoa. By E. C. Jelly, F.R.M.S. (London: Dulau and Co., 1889.

THIS is a work for which all students of the Polyzoa (Bryozoa) should be grateful. It supplies an undoubted want, and will greatly facilitate the investigation of the large and interesting class with which it deals.

Synonymy is certainly not an attractive element of natural history study. Indeed, anything of less intrinsic interest cannot well be imagined, and yet it has a specific value in relation both to morphological and systematic work, and it is of the first importance that it should be carefully determined. A just and accurate synonymy is of course an essential condition of a sound nomenclature; it is a key to the actual state of knowledge, and an index to the sources in which it must be sought, which is invaluable to the student. It is also a safeguard against duplicate and delusive names.

Miss Jelly's "Synonymic Catalogue of the Recent Marine Bryozoa" fills a vacant place. There is not, so far as we know, any work which occupies the same ground. "It aims," as the author explains in her preface, "at bringing into view all the names of *published* recent Bryozoa, with as full a synonymy as may be possible."

The fossil forms belonging to recent species, and these only, are included in the synonymy. The work, which bears the marks of careful and conscientious labour, brings before the student within small compass the entire series of published Polyzoan forms belonging to the recent fauna; supplies him with a reference to the book in which each species was first described and with the name of the writer who first described it; tracks it, as it were, however disguised by variety of name, from author to author, and so in fact furnishes an index to the whole range of the systematic literature.

The value of such a guide to the student of the Polyzoa must be at once evident. It economizes his labour; it enables him to enter upon original investigation with a full knowledge of what has been already done, and it saves him from adding to the weariness and perplexity of those who may follow him by multiplying duplicate names.

Within the last few years large additions have been made tó the known species of Polyzoa, the diagnosis of which is distributed to a great extent through the biological journals of Europe and America. With all the care that he can exercise the student is continually liable to overlook some paper in some obscure periodical of which perhaps he has never heard, and, as a result, to add another name to the already burthensome synonymy. In point of fact, as a matter of convenience the synonymy of each class of any extent is worthy of separate and special treatment; and such a work as Miss Jelly has now supplied should be regarded as an essential part of the apparatus of the student who devotes himself to descriptive and systematic work.

We cannot but regret that Miss Jelly has abandoned

the name of the class which has been generally adopted in England, and which commemorates the remarkable researches of an unobtrusive but most able and original worker in the province of invertebrate zoology. We have no intention of reviving the controversy on this subject, in which indeed Miss Jelly takes no part, as she does not state the grounds on which she bases her decision. The *consensus* of Continental naturalists in favour of Ehrenberg's name has no doubt had much weight with her, but the appeal to numbers is hardly likely to satisfy those who have tried the case on its merits, and arrived at a different conclusion. Of course it would be satisfactory to secure uniformity of nomenclature, if it were possible; but those who have a strong conviction that J. V. Thompson was the earliest to appreciate and define the distinctive peculiarities of Polyzoan structure, and that his name was intended to apply not merely to the zooids of a colony, but to the type of organization which they represent, can hardly consent to be parties to an absolute rejection of his claim.

The value of such works as the present depends entirely on the care and minute accuracy with which they are compiled. Miss Jelly's "Catalogue" affords abundant proof that these qualities have not been wanting in her case. That she possesses a thorough command of the literature of her subject is shown by the fulness of the synonymy, and (very strikingly) by the explanatory notes appended to many of the species. The book supplies ample evidence of intelligent and enthusiastic interest in the subject, and patient industry in dealing with it.

As to the synonymy itself, many difficult questions arise in connection with it, which clearly cannot be discussed in a work which aims at being a guide to the recorded species, and not a critical treatise. In not a few cases of supposed synonymy we should feel compelled to dissent from the conclusions arrived at, or adopted from others. But on such points the student must satisfy himself.

We may add that the book is handsomely got up, and printed in a type which, so far as clearness is concerned, leaves nothing to be desired.

Miss Jelly is to be congratulated on the completion of a very onerous task, and on the valuable contribution which she has made to the working apparatus of the student.

OUR BOOK SHELF.

Zoologische Ergebnisse einer Reise in Niederlandsch Ost-Indien. Von Dr. Max Weber. Erstes Heft. (Leiden: Brill, 1890.)

The numerous books, memoirs, and pamphlets that have been published during the century dealing with the natural history of the Malay Archipelago have revealed to the world of science a fauna which is perhaps unrivalled for richness, variety, and general interest. But even now we are scarcely beyond the threshold of the investigation. The travels of von Rosenberg, Wallace, van Martens, Forbes, and others, and the painstaking observations and collections of many of the Dutch residents and *controleurs*, have given us a knowledge of the principal features of the ornithology and entomology of some of the more important islands, but there are still many regions of undoubted interest that have scarcely been explored at all, even by their nominal masters the Dutch. During the journey undertaken by Prof. Weber and his wife in 1888 and 1889 some of these little-known regions and islands were visited, and the results are now appearing in a series of memoirs prepared by eminent Dutch naturalists, under the editorship of the distinguished Professor of Zoology at Amsterdam.

Several interesting new forms are described in the first part; and no doubt many others will follow in the memoirs that are now in course of preparation, from Central Celebes, Flores, and the Saleyer Islands—regions that have hitherto scarcely been visited by naturalists. But the interest in Prof. Weber's results does not by any means lie exclusively in the fauna of the remote corners of the archipelago. By investigating the fresh-water fauna and the inconspicuous forms of terrestrial invertebrates of all the districts visited, he has opened to us a new chapter in the natural history of the archipelago. The memoirs on Spongillidæ by the editor, on Apterygota by Oudemans, and on the land Planaria by Loman, are most valuable and interesting contributions to our knowledge of the tropical species of these groups. Of more than special importance is the paper by the Professor and Madame Weber, "On some New Cases of Symbiosis." One of the most remarkable of those described is that of an alga belonging to the family Trentepohliaceæ symbiotic with a fresh-water sponge. This paper is in French, and contains an interesting discussion of the problems presented and the literature of the subject.

Two memoirs, in English, by Prof. Weber and by Dr. Jentinck, deal with the mammalia collected during the journey. It appears from these pages that the curious animal the sapiutan, *Anoa depressicornis*, is not confined to the northern peninsula of Celebes, as is usually stated to be the case, but may be found in favourable localities all over the island. In a long discussion on the habitat of the rare monkey *Macacus maurus*, we are told that it occurs on the Maros River and elsewhere in South Celebes, and that it should be considered to be "one more of the remarkable animals peculiar only to that island, with a continental character."

Every naturalist who reads this first part of Prof. Weber's results will look forward with interest to the publication of the memoirs that are still in course of preparation.

SYDNEY J. HICKSON.

Inorganic Chemistry, Theoretical and Practical. By William Jago, F.C.S., F.I.C. (London: Longmans, Green, and Co., 1890.)

The number of classes established according to the regulations of the Science and Art Department is now so considerable that publishers and authors alike are ready to specially cater for their needs. Messrs. Longmans, Green, and Co. have already an extensive series of "science manuals" written to meet the requirements of students taking the elementary stage of subjects as given in the Directory of the Department; and Mr. Jago's volume is one of a similar series that is in preparation to include the matter prescribed in the advanced syllabus of each subject. After the preface we are nowhere reminded of the particular aim of the book, or of the limitations under which the author has done his work; and it is worthy of note that the papers set by the Department examiners are not reprinted at the end. Leaving the particular meaning attached to the words elementary and advanced by the Science and Art Department, we may describe the volume before us as an elementary treatise on inorganic chemistry, of about 460 pages. It includes a consideration of the more common metals and their compounds, three pages concerning the periodic law, and six pages on the "causes which modify chemical action." Fluorine takes its natural place among the halogens as an isolated element, and other of the recent advances are duly noticed Some parts are very meagre, as, for example, the paragraphs

on acetylene, in which, by the way, there occurs the very objectionable expression "two volumes of carbon" in reference to the composition of the gas. The statement that "nitric acid is largely used in the manufacture of sulphuric acid" needs qualification, especially as we are told that "sulphuric acid is largely employed in the preparation of . . . nitric acid." But, on the whole, the manual is one that deserves recommendation, and will be valued by those for whose use it has been written.

We would suggest that, in the future editions that will doubtless be called for, "choke" or "after-damp" be not described as carbon dioxide, because the amount of carbon dioxide in it is very small compared with the nitrogen present; and we do not think that the examiners at South Kensington would harshly judge any student who corrected the current notion. And if the engraver of the illustrations had photographs of the apparatus supplied him, or, better still, if the blocks could be prepared mechanically from such photographs, the figures would have an appearance of genuineness which at present many of them lack. It is better to represent the apparatus used than the operation in progress, and then one avoids such unnecessary and unwise conventionalities as appear in the attempt to illustrate a brilliant combustion in a glass jar.

Arithmetical Chemistry. Part I. By C. J. Woodward, B Sc. New Edition. (London: Simpkin, Marshall, and Co., 1890.)

IN the study of chemistry there is a certain amount of arithmetical work which the elementary student must master as he progresses, and this used to be generally considered as the part in which the young pupil was most likely to fail. But now it is different, and we fear that there is rather a danger of too much stress being laid upon arithmetical exercises. In the volume before us the author proceeds by easy stages, explaining the various subjects dealt with in a sound and simple manner. We hope for the student's sake that it is intended for the teacher to select from the numerous exercises set. At the end of the volume there are "the whole of the questions in arithmetical chemistry and chemical philosophy," selected from the examination papers of five different examining bodies, for the years 1886 to 1889 inclusive. These will doubtless be useful to the teacher if used with discretion enough to prevent his students from imagining that chemistry is a branch of arithmetic.

Air-Analysis: with an Appendix on Illuminating Gas. By J. Alfred Wanklyn and W. J. Cooper. (London: Kegan Paul, Trench, Trübner, and Co., Ltd., 1890.)

THIS small volume of ninety pages is a practical guide to air-analysis, especially for sanitary purposes. The directions are plain, and multiplication of methods is avoided. Hempel's apparatus is employed. For the estimation of oxygen, nitric oxide is advised, and it is pointed out that as an excess of the gas is used it need not be pure. The authors state that, in their hands, this method has proved very accurate, and they give experimental results showing that it is to be preferred to alkaline pyrogallol or explosion with hydrogen. Directions for these latter methods are, however, included. The estimation of small quantities of carbonic oxide is performed by absorption in a cuprous chloride solution, with subsequent elimination and measurement of the gas. The analysis of coal-gas is dealt with in the appendix, and the volume concludes with some useful tables. As an addition to the treatises on special branches of analysis written by Mr. Wanklyn, either solely or jointly, this volume will be welcomed by analysts and students.

Fresh-water Aquaria: their Construction, Arrangement, and Management. By Rev. Gregory C. Bateman. (London: L. Upcott Gill, 1890.)

MR. BATEMAN says that he has always been fond of natural history, and that when he was a boy he looked forward with pleasure to the prospect of having an aquarium of his own. When this delight was experienced, he found that it had many drawbacks. These were due to the fact that he did not know how to manage his treasure. He bought or borrowed books on the subject, but was not able to obtain all the information he required. Then he tried to find out by experiment what he could not learn by reading; and as most of his attempts were in the end successful, he resolved that he would write such a book as he himself had wished for when he was making his "first blunders in aquarium matters." The present volume is the result of this decision, and there can be no doubt that it will be very cordially welcomed by many students who want just such information as the author has brought together. He writes simply, clearly, and practically, and no one who reads with moderate care what he has to say will find much difficulty in complying with the rules he lays down. He gives, also, interesting details as to the best water-plants and live stock to be kept, how and where they are to be obtained, and how they are to be maintained in health. The volume includes many illustrations.

Scenes and Stories of the North of Scotland. By John Sinclair. (Edinburgh: James Thin. London: Simpkin, Marshall, and Co. 1890.)

MR. SINCLAIR is an intelligent and lively writer, and has produced a book which may be read with pleasure by persons who have visited, or think of visiting, the scenes he describes. The work is not, in the strict sense, scientific; but it includes many passages which are, to a certain extent, of scientific interest. The subjects are: Loch Duich, Ross-shire; the Black Rock, Ross-shire; the Island of Lewis; Assynt, in Sutherland; the Caithness coast; the town of Thurso; and the Shetland Islands. Here is ample scope for fresh observation and bright description; and the author has generally made good use of his opportunities. It is to be regretted, however, that he did not, before writing of the Island of Lewis, make himself acquainted with what trustworthy archæologists have said about the great prehistoric monument at Callernish. "There is little doubt," he asserts, "that these standing stones are a monument of the ancient Druids." There is not a shred of evidence that the Druids had anything whatever to do either with these or with any other "standing stones."

LETTERS TO THE EDITOR.

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The Discharge of Electricity through Gases.

ON returning from abroad I find that Prof. J. J. Thomson has written to you to complain about a passage in my Bakerian Lecture, and I should like to say a few words in explanation. I am sorry if I have said anything that would seem unfair to Prof. Thomson, but I have re-read his paper, and confess that my difficulties have not been cleared away.

I shall be glad to be allowed to enter somewhat fully into the objection raised by Prof. Thomson to my remarks, as, independently of any personal question, this may help to clear up some disputed matters. The point at issue between Prof. Thomson and myself is, whether the Clausius-Williamson dissociation hypothesis forms an essential portion of the views on

the disruptive discharge which he expressed in his paper published in the *Philosophical Magazine* for June 1883. That is partly a matter of opinion, but Prof. Thomson has certainly led his readers to think that he considered the presence of free atoms before discharge as essential.

One of the principal difficulties of any theory of the disruptive discharge is the explanation of the relation between pressure and so called dielectric strength. This relation is one of the crucial points by means of which every theory must be judged. The two following passages will show how Prof. Thomson meets the difficulty:—

"Let us now apply these considerations to the case of the electric discharge. The disturbance to which the gas in an electric field is subjected makes the molecules break up sooner into atoms than they otherwise would do, and thus diminishes the ratio of the paired to the free times of the atoms of the gas; as the intensity of the electric field increases, the disturbance in some places may become so violent that in these regions the ratio of the paired to the free times approaches the value it has when the gas is about to be dissociated."

"Let us now consider what effect rarefying the gas would have upon its electrical strength. In a rare gas the mean distance between the molecules is greater than in a dense one; and if the temperature be the same in both cases, and consequently the mean velocity of the molecules the same, the ratio of the free to the paired time will be greater for the rare than for the dense gas, for the free atoms will, on an average, be longer in meeting with fresh partners. Thus the rare gas will be nearer the state in which it begins to suffer dissociation than the dense gas, and thus it will not require to be disturbed so violently as the dense gas in order to increase the ratio of the free to the paired time to its dissociation value; and thus the intensity of the field necessary to produce discharge would be less for the rare gas than for the denser one: in other words, the electric strength would diminish with the density, and this we know is the case."

It will be seen that the explanation entirely depends on the idea that free atoms exist already before discharge. In my Bakerian Lecture, I have pointed out that the existence of free ions seems inconsistent with experimental fact, and I add: "This seems to me to be fatal to J. J. Thomson's view of the disruptive discharge." This is the passage which Prof. Thomson says implies a misconception, but surely, assuming that I am right in my argument that no dissociation takes place before discharge, I am also right in saying that this is fatal to any theory which makes the relation between pressure and spark potential depend on such dissociation. It may be said, on the other side, that the idea of decomposition of molecules by the discharge, which forms so important a feature in the theory which I have explained in my Bakerian Lecture of 1884, occurs already in Prof. J. J. Thomson's paper of 1883; and that it is unfair, therefore, to condemn his views because they do not account for a feature of the discharge which has never been satisfactorily explained. A few remarks are therefore necessary to explain the relationship between Prof. Thomson's paper of 1883 and my own of the succeeding year.

The hypothesis that the discharge of electricity in gases is similar to that in electrolytes, and that each atom of a gas such as nitrogen or hydrogen carries a permanent charge, seems so obvious that it must have occurred to many who have thought about the matter; but no attempt has, until recently, been made to develop the hypothesis so as to account for the complicated phenomena of the discharge.

The credit of being the first to have done so undoubtedly belongs to Giese, who has explained by means of it a number of observed facts concerning the behaviour of flames, and of the gases rising from flames, but he has not until quite lately considered any other part of the subject. J. J. Thomson, in 1883, published his paper "On the Theory of the Electric Discharge in Gases." The hypothesis of atomic charges is not mentioned therein, and the author does not discuss the question whether or not a current of electricity in a gas consists of a diffusion of charged atoms or not. So carefully are these matters excluded, that it is difficult to avoid the conclusion that it was done intentionally, in order that the investigation may be more general, and independent of any particular theory which might in future be established.

"As the most general assumption," the electric field is supposed to consist of "a distribution of velocity in the medium whose vortex motion constitutes the atoms of the gas; the disturbance due to this distribution of velocity will cause the molecules to break up sooner than they otherwise would do." The way in which the decomposition of molecules is connected with the spark must be judged from the passages quoted above, and from those quoted by Prof. Thomson in his letter to you. The general terms in which the whole paper is expressed may increase its scientific value, but it affords no help to those who wish to form any more definite notion what an electric current through a gas consists of. If in future it should be shown that a current of electricity does *not* consist of a diffusion of charged atoms, Prof. Thomson's reasoning may still apply. I do not know whether such general considerations may be fitly described as "a theory."

After having for a number of years attempted to trace out the consequences of the electrolytic hypothesis, and discovered some method by means of which it could be definitely tested, I presented to the Royal Society, in 1884, a paper which forms the subject of the Bakerian Lecture for that year. In that paper I referred to Prof. J. J. Thomson's of the previous year, but unfortunately I was not then aware of Giese's work, and that gentleman has undoubtedly some right to complain of the way in which his researches have, till recently, been neglected by myself and others. I have never claimed that my hypothesis of atomic charges was in any way original; but I have always maintained that that hypothesis, by itself alone, does not explain much. My recent Bakerian Lecture shows sufficiently clearly that we must form much more definite notions regarding the phenomena of dissociation and the interaction between chemical and electrical forces before we can say that we have a complete theory of the electric discharge, and those who will overcome successfully all the remaining difficulties will have done much more than those who started the idea. The work of Hittorf, E. Wiedemann, Hallwachs, Warburg, Elster and Geitel, and others, has thrown so much light on many points, that the final decision as to the truth or otherwise of the theory under discussion cannot be long deferred. I have ventured to call it the "theory of electrolytic convection," which, I should say, was a sufficiently neutral and distinctive name.

ARTHUR SCHUSTER.

A Suggestion respecting the Syllabus of the Science and Art Department.

WILL you permit me to call attention to the following considerations which have occurred to myself and several of my colleagues as the result of some years' teaching experience?

(1) The syllabus of Subject VIII. (Sound, Light, and Heat), as it at present stands, is very extensive—too much so, indeed, for the majority of students to grasp in one session. This fact is tacitly acknowledged by the Department, as a student is permitted to obtain a first class in either stage by taking two only out of the three subjects.

(2) Sound and light are pretty closely related, both being forms of wave motion, and the general ideas involved in their study very similar. But between these two subjects on the one hand, and heat on the other, this connection is small, existing, indeed (so far as the Department's syllabus extends), only in the comparatively unimportant section of radiant heat. The considerations involved in dealing with specific and latent heat and with heat as a form of energy are of an utterly different character from those presented in sound or light.

(3) The syllabus of Subject VI. (Theoretical Mechanics) is also too extensive for most students to grasp in one session, including, as it does, four subjects, viz. Statics, Dynamics, Hydrostatics, and Pneumatics. And although the Department does not officially state that a student can obtain a first class with two only of these subjects, yet the papers are always arranged to admit of his doing so by taking statics and dynamics only. Thus, in the elementary stage, a candidate may answer only seven questions out of twelve, and in the advanced, eight out of twelve, and yet in each case *nine* of the twelve are confined to statics and dynamics.

The result is that teachers and students pay but scant attention to hydrostatics and pneumatics.

(4) A large number of students take physics and mechanics simply as accessories to engineering and the applied sciences. Now, to such students a knowledge of *heat* is most essential, while sound and light are quite useless. Again, though heat has but slight connection with sound or light, it has a very strong connection with hydrostatics and pneumatics. By far the most important thermal phenomena are those presented by liquids and gases, and moreover it is precisely these that an engineering

student requires to know. For how can the action of the steam-engine be properly understood without a knowledge of the principles of fluid and gaseous pressure, and of the relation between heat and work? Yet under the present arrangement the former of these constitutes a neglected part of Subject VI., while the latter comes under Subject VIII., and is associated with matter totally irrelevant.

All these considerations seem to point to the desirability of a change in the official syllabus somewhat as follows:—

(*a*) To cut out Heat from Subject VIII., making the latter consist of Sound and Light only.

(*b*) To cut out Hydrostatics and Pneumatics from Subject VI., making the latter consist of Statics and Dynamics only.

(*c*) To combine Heat, Hydrostatics, and Pneumatics into a new subject having its appropriate number. These three could then be more effectively studied than under the present system, and there would be ample matter therein to form one of the courses from September to May. The syllabus of the new subject would naturally include all the points specified by the Department as necessary preliminaries to the study of Steam (*vide* Steam syllabus, Subject XXII.), and would thus supply a specific want to all engineering students.

On the whole, it is respectfully submitted to the authorities of the Department, and others interested in the education of the people, that the proposed alteration would conduce to a more thorough and systematic study of all the subjects referred to, and be attended with benefit to students both of physics and mechanics.

VOLO LEGES MUTARI.

On Last-place Errors in Vlacq.

M. M. F. LEFORT, in his account of the great Cadastre tables, contained in the fourth volume of the *Annales de l'Observatoire Impérial de Paris*, gives a list of errors in Adrian Vlacq's ten-place table of logarithms. As this one by Vlacq, or its copy by Georg Vega, is the only complete table of ten-place logarithms yet in existence, we naturally desire to make it thoroughly accurate, and therefore proceed to correct it by aid of this new information.

M. Lefort tells us that Prony, in his instructions, was expressly enjoined "not only to compute tables which shall leave nothing to be desired as to exactitude, but to make them the most vast and imposing monument of calculation that had ever been made or even conceived," and, adds M. Lefort, "this programme, so widely sketched, has been faithfully carried out." Yet, on the very same page, we are told "that Prony fixed the general limit of precision for his logarithmic tables at 12 decimals"; this although the original work by Henry Briggs had been carried to 14 places.

Thus it seems that the Cadastre tables cannot be trusted to determine the absolute accuracy of those of Vlacq whenever the figures to be rejected are between the limits 4900 and 5100, and that in no case can they serve to check the final figures in Briggs.

Having scrupulously examined, by help of my fifteen-place table, all the corrections given by M. Lefort, I here give the results, in order that the possessors of Briggs, Vlacq, or Vega may make note of them.

Among 282 last-place corrections given, I find seven to be erroneous, the logarithms in Vlacq and in Vega being right. In order to make doubly sure, I have also used my 28-place table, and here give the exact figures from the 8th to the 20th place—

Number.		Logarithm.
26188	...	322 49959 00920
29163	...	978 49968 31667
30499	...	999 50010 73882
31735	...	026 49975 27403
34182	...	883 50038 92375
34358	...	753 50011 99957
60096	...	662 49998 09339

From this we see that the Cadastre tables are inadequate to the thorough checking of ten-place logarithms; in the case of the last of these miscorrections, even the fifteen-place table is barely sufficient, and needs to be fortified by an extended calculation.

Among the 275 remaining errors, five have been imported from Briggs, and I have therefore examined them to greater length; the logarithms to the 20th place are—

Number.		Logarithm.
7559	...	453 41468 90981
8006	...	857 69086 31797
8009	...	936 63054 38960
10033	...	122 46398 29224
99926	...	031 14867 68936

Thus there are left 270 errors to be charged against Vlacq; of these no less than 96 are within the limits of inaccuracy allowed by Prony.

Near the end of the list there occurs a group of 21 (from the number 98336 to 98367) which seem to have resulted from some single running error. Now this part of the table was copied from Briggs, and we should expect these errors there; but, on turning to the original work, we find that none of his logarithms differs by more than unit in the 14th place from that of the fifteen-place table, and thus the source of the errors in Vlacq becomes mysterious.

The most feasible explanation is that the errors had been observed and corrected while the sheet was at press, and that thus all the copies of Briggs are not alike. It is probable that the very copy used by Vlacq may be preserved in some one of the libraries in the Netherlands; in such case, an inspection would set the matter at rest.

EDWARD SANG.

September 27.

On the Soaring of Birds.

IN answer to Mr. C. O. Bartrum's objections in NATURE of September 4 (p. 457), I beg to refer to an article in the *Skand. Archiv. für Physiologie*, ii. 2, in which I have given a detailed account of the weighty reasons which have led me to suppose that soaring birds are able to undertake successive alterations of direction with very little loss of *vis viva*. This loss is of the same kind as that caused by the resistance of the air to the rectilinear translation.

There is, however, one fact which, in the article in the *Skand. Archiv.*, I have thought it superfluous to point out—namely, that the manœuvre of the bird is the same, and the loss of energy thereby equally the same, whether the bird turns in a calm or in a uniform wind. If Mr. Bartrum has been led to another opinion, it may be that he has not quite made out how these turnings are executed.

MAGNUS BLIX.

Lund, Sverige, October 10.

Earthquake Tremors.

IF those of your readers who are interested in this subject will turn to p. 84 of the "Report on the East Anglian Earthquake of April 2, 1884," by R. Meldola and W. White (Essex Field Club special memoirs, vol. i.), they will see that at Wivenhoe a man who felt the shock of the earth movement found to his own satisfaction, by careful measurement and calculation, that the vertical displacement where he stood amounted to no less than *six feet*. How it was that any building in Wivenhoe remained standing after so tremendous an upheaval the observer did not appear to think worth considering.

ALFRED P. WIRE.

THE PROPERTIES OF LIQUID CHLORINE.

ALTHOUGH chlorine was shown by Faraday so long ago as the year 1823 to be one of the more easily condensable gases, yet, no doubt owing in a large measure to its very disagreeable nature, comparatively little has hitherto been known concerning its properties when in a liquefied state. In view of the fact that chlorine is now stored in the liquid state for the use of manufacturing chemists in a similar manner to carbon dioxide, sulphur dioxide, and ammonia, it is imperative that something more definite should be known as to the relations of liquefied chlorine to temperature and pressure. Consequently, a very complete investigation of the subject has been made by Dr. Knietsch at the request of the directors of the "Badischen Anilin- und Sodafabrik," of Ludwigshafen; and his results, of which the following is a brief account, are published in an interesting communi-

cation to the current number of *Liebig's Annalen* (Band 259, Heft 1, p. 100).

The work includes the determination of the vapour-tension of liquid chlorine at temperatures from $-88°$ C. to $146°$ C. (its critical point), a complete examination of its behaviour near the critical point and the determination of its specific gravity and coefficient of expansion for a range of temperature between $-80°$ and $+80°$.

Liquid chlorine generally appears to possess a yellow colour. When, however, the colour of a long column is examined, it is found to have a distinctly orange tint. The absorption spectrum does not exhibit any characteristic bands, but the blue and violet portions of the spectrum are completely absorbed, the transmitted spectrum thus consisting of the red, orange, yellow, and green.

Vapour-Tension of Liquid Chlorine below its Boiling-Point.

The apparatus used for this determination consisted of a kind of distilling flask, whose side tube was connected by means of a piece of strong-walled caoutchouc tubing with a wide manometer tube. The flask was about half filled with liquid chlorine, and was immersed in a bath also containing liquid chlorine whose temperature could at the same time be kept equal throughout, and be very finely regulated by means of a current of air driven in through a tube passing to near the bottom of the bath.

In commencing a series of determinations the chlorine in the flask was first made to boil, thereby driving out the air remaining in the apparatus. The neck was then closed by means of a caoutchouc stopper well coated with glycerine, and the open end of the manometer was allowed to dip into a vessel containing concentrated sulphuric acid. As the flask became cooled by immersing it in the cold chlorine in the bath, sulphuric acid was drawn into the manometer until it attained a height of 3–5 cm., when the caoutchouc connection was momentarily pinched while the open end of the manometer was transferred to the mercury trough. The small column of sulphuric acid thus standing above the mercury column effectually protected it from the corroding action of the chlorine. The bath was then cooled gradually, and a series of readings taken of the temperature of the bath, by means of an alcohol thermometer, and of the position of the meniscus of the mercury in the manometer. The small column of sulphuric acid was of course calculated to its equivalent height of mercury, and added to the measured height of the mercurial column. By careful use of the current of dry air the liquid chlorine of the bath was found capable of being reduced in temperature as low as $-60°$ C. The lower temperatures, down to $-88°$, were attained by mixing more or less solid carbon dioxide with the chlorine. The results obtained are given in the table at the end.

Determination of the Pressure of Liquid Chlorine from its Boiling-Point to 40° C.

The data at present existing upon this subject are very meagre and conflicting. Davy and Faraday found the pressure at 15° C. to be 4 atmospheres, whilst Niemann gives the pressure at 0° C. as 6 atmospheres, and at 12°·5 C. as 8 atmospheres. As this is a most important point in regard to the storage of liquid chlorine in metallic bottles, great pains have been taken to arrive at unimpeachable results, and as the most certain method of measuring the pressure a high column of quicksilver was employed. The apparatus consisted of a U-tube, one limb of which was narrower than the other, and prolonged upwards to a height of over 8 metres. The other and wider limb was joined at the top by means of a capillary tube to a cup, serving the purpose of a funnel for introducing the liquid chlorine. In commencing an experiment, a convenient quantity of mercury was first poured in so as to stand in the wider limb at about a quarter its height. A column of sulphuric acid was then introduced into the wider limb so as to protect the mercury, and finally the liquid chlorine was introduced through the funnel by a process of alternately warming and cooling; the cooling was effected by pouring a little liquid chlorine over a piece of cotton wrapped round the limb and evaporating it by a strong current of air. When the limb was quite full, the chlorine occupying the capillary tube was evaporated by the warmth of a small blowpipe flame, and the capillary fused up. The apparatus was then immersed, until the wider limb was covered, in a bath of liquid sulphur dioxide for temperatures up to 0°, in ice for the determination at 0°, and in water agitated by a current of air, and either cooled by ice or warmed by a small flame for temperatures up to 40°. For the comparatively higher of these temperatures it was of course necessary to pour mercury into the longer limb so as to prevent the mercury in the wider limb being driven round the bend. Complete results are given at the end, but it may be remarked in passing that the pressure at 0° is 3·66 atmospheres; and at 15°, 5·75 atmospheres.

Determination of the Pressure at Higher Temperatures.

For these yet more dangerous and difficult experiments a metal apparatus was employed, similar in principle to that just described, except that the pressures were measured by a metal gauge manometer, which had previously been completely tested and its readings verified. It was found important in these experiments not to employ too much chlorine, as owing to the immense coefficient of expansion the whole space might become full of liquid, and further heating would cause the generation of dangerously high pressures. For temperatures up to 100° a water-bath was employed, and for the higher temperatures up to the critical point 146° an oil-bath, both kept in circulation by a rapid current of air. The pressure at the critical temperature of 146° C. was found to be as high as 93·5 atmospheres.

Critical Point of Liquid Chlorine.

The critical point was determined in a separate experiment, and some very interesting results were obtained, the yellowish green colour of chlorine perhaps assisting in rendering the appearance of what has sometimes been termed the fourth state of matter between the liquid and the gaseous more distinct than usual. A hard glass tube of 8 mm. diameter was about one-third filled with redistilled dry liquid chlorine and sealed. A small thermometer, whose readings commenced at 140°, was attached to it by platinum wire, and the whole very slowly heated in a bath of vaseline. The observations were made with the naked eye, the observer being protected from any possible explosion by a thick glass plate. At 140° extremely small bubbles began to be developed throughout the mass of liquid. At 144° the hitherto sharp meniscus began to disappear, and at 145° the presence of a liquid was only evident by the more intense yellow colour and higher refractive power of the lower portion of the tube. At 146° the contents of the tube were homogeneous throughout, the critical point being attained, and the liquid converted into gas. On allowing the tube to cool slowly, the condensation always commenced below 146°, with the formation of a cloud and a fine rain of minute yellow spheres of liquid chlorine. The rain was generally apparent throughout the whole of the upper portion of the tube. Sometimes, however, the liquid meniscus again appeared without any previous manifestation of precipitation.

Specific Gravity and Expansion of Liquid Chlorine.

It is a curious fact that many gases when compressed to the state of liquid expand enormously when heated as compared with ordinary liquids, the amount of expansion sometimes exceeding that of the gas itself. Liquid chlorine is no

exception to this rule, and it was absolutely essential that its rate of expansion should be thoroughly investigated, in order that storage bottles should not be filled to a dangerous extent. For temperatures up to 36° C. a closed dilatometer of glass was employed, the long cylindrical bulb of 60 c.c. capacity and part of the stem being filled with liquid chlorine, and the remainder of the stem with chlorine gas. The whole apparatus was immersed in a long cylindrical bath. For the lowest temperature, of — 80°, the bath was filled with solid carbon dioxide. For the determination of the specific gravity at the boiling-point of chlorine, a bath of boiling liquid chlorine itself was employed, no less than three kilograms being required. Between the boiling-point and 0° the substance used in the bath was liquid sulphur dioxide. For the determination at zero powdered ice was employed, and for the higher temperatures a water-bath kept in motion by an air current. It was not possible to proceed higher than 36° with this apparatus, on account of the danger of explosion. The higher determinations were made by means of a hydrometer suspended in liquid chlorine enclosed in a tube of hard glass which was immersed in a glass water-bath heated to the required temperature.

It will be seen from the following table that liquid chlorine is indeed a very expansible substance. The coefficient of expansion at 80° is already 0·00346, nearly equal to that of gaseous chlorine, and is rapidly increasing, so that before the critical temperature of 146° is attained, the coefficient of expansion will be considerably higher than that of the gas.

Following is a table showing the pressure, specific gravity, and coefficient of expansion of liquid chlorine for every 5° of temperature from — 80° C., calculated from the formulæ derived from the experimental data obtained.

Temperature.	Pressure.	Specific gravity.	Mean coefficient of expansion.
– 102° C.	Solid (Olzewski).	—	
– 88	37·5 mm. Hg.	—	
– 85	45·0 ,,	—	
– 80	62·5 ,,	1·6602	
– 75	88·0 ,,	1·6490	
– 70	118 ,,	1·6382	
– 65	159 ,,	1·6273	
– 60	210 ,,	1·6167	
– 55	275 ,,	1·6055	0·001409
– 50	350 ,,	1·5945	
– 45	445 ,,	1·5830	
– 40	560 ,,	1·5720	
– 35	705 ,,	1·5589	
– 33·6	760 ,,	1·5575	
– 30	1·20 atmospheres	1·5485	
– 25	1·50 ,,	1·5358	
– 20	1·84 ,,	1·5230	
– 15	2·23 ,,	1·5100	0·001793
– 10	2·63 ,,	1·4965	
– 5	3·14 ,,	1·4830	
0	3·66 ,,	1·4690	
+ 5	4·25 ,,	1·4548	0·001978
+ 10	4·95 ,,	1·4405	
+ 15	5·75 ,,	1·4273	0·002030
+ 20	6·62 ,,	1·4118	
+ 25	7·63 ,,	1·3984	0·002190
+ 30	8·75 ,,	1·3815	
+ 35	9·95 ,,	1·3683	0·002260
+ 40	11·50 ,,	1·3510	
+ 50	14·70 ,,	1·3170	0·002690
+ 60	18·60 ,,	1·2830	
+ 70	23·00 ,,	1·2430	0·003460
+ 80	28·40 ,,	1·2000	
+ 90	34·50 ,,		
+ 100	41·70 ,,		
+ 110	50·80 ,,		
+ 120	60·40 ,,		
+ 130	71·60 ,,		
+ 146	93·50 ,,	Critical point.	

An interesting result, which is not noticed by Dr. Knietsch in his paper, is obtained on calculating the specific volume of chlorine from the determination of specific gravity at the boiling-point, — 33°·6. On dividing the atomic weight 35·5 by 1·5575, the specific gravity at the boiling-point, the number 22·8 is obtained for the atomic or specific volume of chlorine, a number practically identical with that derived by calculation from the numerous determinations of the specific volume of compounds containing chlorine.

In this respect chlorine resembles bromine and the compound radicles NO_2 and CN, which were shown by Prof. Thorpe (Journ. Chem. Soc., 1880, 382) to occupy the same volume in the free state as in combination.

A. E. Tutton.

ELECTRICAL STORMS ON PIKE'S PEAK.

The "Annals of the Astronomical Observatory of Harvard College," vol. xxii., contains the meteorological observations made at the summit of Pike's Peak, Colorado, at a height of 14,134 feet above sea-level. It is not remarkable that such an elevated station should be celebrated for its electrical storms, and the observers from 1874 to 1888 have recorded many interesting details in the journals respecting their physical and physiological actions.

The manifestation of atmospheric electricity by induced effects is often very strongly marked. During the passage of electrified clouds over the summit of the peak the well-known singing and buzzing noises described as an adjunct of St. Elmo's fire were heard to proceed from the telegraph wires, the exposed instruments, the instrument shelter, and the house. The sound is said to be very similar to the buzzing of bees and crackling of burning evergreens. At times the hair of the observers became upright and strongly repellent, and the same peculiar noise proceeded from it.

Some very remarkable effects are recorded on August 18, 1877:—"During the evening the most curiously beautiful phenomena ever seen by the observer were witnessed, in company with the assistant and four visitors. Mention has been made in journal of May 25 and July 13 of a peculiar 'singing' or rather 'sizzing' noise on the wire, but on these occasions it occurred in the day-time. To-night it was heard again, but the line for an eighth of a mile was distinctly outlined in brilliant light, which was thrown out from the wire in beautiful scintillations. Near us we could observe these little jets of flame very plainly. They were invariably in the shape of a quadrant, and the rays concentrated at the surface of the line in a small mass about the size of a currant, which had a bluish tinge. These little quadrants of light were constantly jumping from one point to another of the line—now pointing in one direction and again in another. There was no heat to the light, and when the wire was touched only the slightest tingling sensation was felt. Not only was the wire outlined in this manner, but every exposed metallic point and surface was similarly tipped or covered. The anemometer cups appeared as four balls of fire revolving slowly round a common centre: the wind-vane was outlined with the same phosphorescent light, and one of the visitors was very much alarmed by sparks which were plainly visible in his hair, though none appeared in the others'. At the time of the phenomenon snow was falling, and it has been previously noticed that the 'singing' noise is never heard except when the atmosphere is very damp, and rain, hail, or snow is falling."

These displays were described with the same minuteness on June 7, 1882. It was then noticed that when the finger was passed along the line the little jets of flame were successively puffed out, to be instantly relighted in the rear. An observer also found that when he approached

one of the places from which the buzzing sound proceeded it would cease, but would recommence again as soon as he withdrew two or three feet distant.

It is recorded that the "observer, on placing his hands close over the revolving cups of the anemometer where the electrical excitement was abundant, did not discover the slightest sensation of heat, but his hands instantly became aflame. On raising them and spreading his fingers, each of them became tipped with one or more cones of light nearly three inches in length. The flames issued from his fingers with a rushing noise, similar to that produced by blowing briskly against the end of the finger when placed lightly against the lips, and accompanied by a crackling sound. There was a feeling as of a current of vapour escaping, with a slight tingling sensation. The wristband of his woollen shirt, as soon as it became dampened, formed a fiery ring around his arm, while his moustache was electrified so as to make a veritable lantern of his face. The phenomenon was preceded by lightning and thunder, and was accompanied by a dense driving snow, and disappeared with the cessation of snow."

A few instances are given of convulsive muscular contraction caused by discharges. Thus, on June 23, 1887, whilst an observer was examining the iron joints around the station, from which the above-described hissing noise was proceeding, a strong electrical manifestation was felt by a twitching of the muscles of the face and hands. A violent "return shock" was experienced by the observer, who, on June 16, 1876, "whilst sitting on a rock, saw a blinding flash of lightning dart from a cloud seemingly not more than five hundred feet away, and heard a quick deafening report, and at the same time received a shock that jerked his extremities together as though by a most violent convulsion, and left lightning sensations in them for a quarter of an hour afterwards."

Among other effects previously noticed at considerable elevations above sea-level we find that on one occasion an observer felt a pain as if from a slight burn on both temples directly under the brass buttons of his cap; when he put his hands to the spots, there was a sharp crack, and all pain disappeared. A peculiar burning sensation was also often felt on the face and hands, and the scalp appeared to be pricked with hundreds of red-hot needles. A more intense action is recorded on June 9, 1882, when an observer was "raised off his feet by the action of the electricity passing through the top of his hat. Instantly snatching the hat from his head, he observed a beam of light as thick as a lead pencil, which seemed to pass through the hat, projecting about an inch on either side and remaining visible for several seconds. The top of his hat was at least two inches from his head when this fiery lance pierced him. . . . He experienced a peculiar burning or stinging sensation of the scalp for several hours afterwards."

The telegraph wires and the buildings were struck by lightning on several occasions. When a flash struck the telegraph wire on July 19, 1884, for a moment the line resembled a belt of fire, and vibrated violently for some minutes after the discharge. Frequent discharges have also been observed between the ground wire and the rocks on which it rested.

On August 12, 1879, it is recorded:—"At 5.40 p.m. a bolt of lightning went through the arrester with the report of a rifle, throwing a ball of fire across the room against the stove and tin sheathing. At 6.35 p.m. the lightning struck the wire and building at the north end where the wires come through the window and arrester, with a crash equal to any 40-pounder. It burned every one of the four wires coming in at the window into small pieces, throwing them with great force in every direction, and filled the room with smoke from the burning gutta-percha insulation. The window-sash was splintered on the outside, one pane of glass broken, and another coated with melted copper. The anemometer wires were also burnt up, and the dial burned and blown to pieces." Barometer bulbs were cracked by lightning on August 21, 1881; and on August 15, 1886, it is recorded:—"Station struck by lightning at 6.45 p.m.; shattered the west window of the dining-room, breaking four panes of glass and shivering the casing, leaving an opening between the casing and the wall; also slightly damaged the building in several places, and set fire to some articles in the storehouse, and burned several holes in the side of a tin bucket, allowing the water in it to escape."

Again, on September 7, 1883, we read:—"Lightning struck the anemometer cups, burning a round hole about half an inch in diameter in one of them. The contact spring in the dial was badly bent, and the point of contact was considerably damaged by melting. When the insulated wire passed over a nail in the side of the house, the head of the nail was melted and the wire burnt off. Inside the window, at a bend in the wire, electricity passed off into the sill, setting some paper on fire. The paper covering the battery box was ignited. Three window lights were broken. A tourist in the dining-room was badly stunned. Observer in passing from dining-room to office was severely stunned by what seemed and felt like a blow on the head. One hand swelled rather badly. The report in the house was double, and sounded like striking red-hot iron upon which cold water had just been thrown."

Some interesting observations of hail-stones are also given. The stones are said to vary in size from peas to pigeons' eggs, and many of them were conical in shape. Sometimes they consisted of soft white snow throughout, without any nucleus, and at other times they were so hard as to require a heavy blow to break them. When this was the case, the broken hail-stones presented a stratified structure, with a centre of clear ice, and concentric rings of solid and spongy ice, with an outer covering of soft snow. It is noted that in all hail-storms the fall of hail entirely ceased for about half a minute following a heavy electric discharge; after this interval, however, the fall was considerably heavier than before.

The following observations, made on October 12, 1877, have an important bearing on the subject of hail formation:—"The rotatory movement of the hail cloud could be plainly seen, and with every violent flash of lightning the passing cloud would grow perceptibly darker, indicating increased condensation. The hail formed by this cloud must have fallen about three miles below, for the wood-packers reported quite solid hail at timber line, and none above. This verifies the theory that a hail cloud can be transported laterally several miles while the ice stones are forming."

The constant crackling of hail when it reaches the earth is also referred to, and rocks are said to give forth a peculiar chattering noise, as if they were shaken by subterranean convulsions, during the occurrence of heavy hail-storms.

These instances of inductive actions manifested during thunderstorms, electrical discharges, and their relation to hailstorms might be considerably multiplied. They confirm previous observations in an intense manner, and should be of some assistance to the student of meteorological phenomena.

R. A. GREGORY.

NOTES.

THE ordinary general meeting of the Institution of Mechanical Engineers will be held on Wednesday evening, October 29, and Thursday evening, October 30, at 25 Great George Street, Westminster, by permission of the Council of the Institution of Civil Engineers. The chair will be taken at half-past seven p.m. on each evening, by the President, Mr. Joseph Tomlinson. The following papers will be read and discussed, as far as time permits:—On tube-frame goods waggons

of light weight and large capacity, and their effect upon the working expenses of railways, by Mr. M. R. Jefferds, of London (communicated through Mr. Henry J. Marten). In connection with this paper the members are invited to inspect one of the waggons which will be on view at any time during daylight on October 29 and 30, at the Victoria passenger station of the London, Chatham, and Dover Railway, where it will be standing in the siding behind the main arrival platform, by permission of the railway company. On milling cutters, by Mr. George Addy, of Sheffield. On the mechanical treatment of moulding sand, by Mr. Walter Bagshaw, of Batley.

THE annual general meeting of the Mineralogical Society will be held in the apartments of the Geological Society, Bnrlington House, Piccadilly, on Tuesday, November 11, at 8 p.m.

AN Exhibition, for the most part national, will be held at Lyons in 1892. With regard to electricity it will be international.

M. FLAHAULT, the eminent algologist and Professor of Botany at Montpellier, has been sent by the Minister of Public Instruction in France on a mission to Sweden, Norway, and Denmark, for the purpose of endeavouring to establish permanent relations between the Scandinavian Universities and the centres of higher instruction in France.

THE municipality of Verona gave a cordial reception to the Italian Botanical Society, which held its third annual Congress in that city from September 1-8. Prof. Arcangeli was elected President of the Society in the place of Prof. Caruel.

THE Vienna Academy commissioned Dr. G. Bukowski to make some geological investigations in Western Asia Minor at the beginning of the present year. After leaving the Khonas-Dagh, Dr. Bukowski made an excursion from Denisli to Tshoekelez-Dagh, the district lying to the north of the Tshuruksu Valley; from thence he proceeded over the Tshukur Pass to Jorengume, and over the Davas Ovassi table-land to the foot of the Baba-Dagh. At the end of June his researches were interrupted by ill-health.

THE Geologists' Association will open its winter session with a *conversazione*, which will be held in the Library of University College, Gower Street, on Friday, November 7. Many objects of geological interest will be exhibited on the occasion.

MR. JOHN HANCOCK, the well-known naturalist, died at his residence at Newcastle-on-Tyne on Saturday, at the age of 84. Mr. Hancock was an admirable observer of bird-life, and the Museum of the Natural History Society at Newcastle has profited largely by his knowledge and enthusiasm as an ornithologist.

THE death is announced, at the age of seventy-six, of Dr. Wenzel Leopold Gruber, the eminent Professor of Anatomy at the University of St. Petersburg.

THE first series of lectures provided by the Sunday Lecture Society begins on Sunday afternoon, October 19, in St. George's Hall, Langham Place, at 4 p.m., when Prof. Silvanus P. Thompson will lecture on "Waves of Light," with illustrations and experiments. Lectures will subsequently be given by Dr. B. W. Richardson, Mr. A. Elley Finch, Dr. Andrew Wilson, Mr. Willmott Dixon, Mr. Arthur Nicols, and Sir A. C. Lyall.

A SCIENTIFIC expedition, under the auspices of the Field Naturalists' Club of Victoria, will start on or about November 15 for the Eastern Islands. Its work will occupy from ten to fourteen days. The expedition will be divided into two parties, one of which will land on Deal Island, the other on Flinders Island. A sub-committee appointed to make the necessary arrangements has reported that while there is little information with regard to Deal Island, the utility of visiting one of the Kent group, which are small, lies in the possible opportunity of determining the limits of the strictly Tasmanian and Australian fauna and flora, since the islands lie considerably nearer the Victorian than the Tasmanian coast. The greater number of members will probably proceed to Flinders Island. They will, in all probability, be able to visit Barren Island, which lies close to the southern portion of Flinders Island. Two varieties of wallaby, waterfowl, and game of various kinds appear to be plentiful, and the nature of the country seems favourable for the pursuit of different branches of natural history.

THE October number of the *Kew Bulletin* begins with a paper on an edible fungus of New Zealand (*Hirneola polytricha*, Montagne). In order that the value of this fungus as an article of food might be tested, a supply of it was recently obtained for Kew. A portion of this supply was submitted for analysis to Prof. Church, F.R.S., and a note by him on the subject is printed in the *Bulletin*. Other subjects dealt with are Mexican Fibre or Istle, a forest plague in Bavaria (*Liparis Monacha*), okro fibre (*Hibiscus esculentus*, L.), cocoa-nut butter, and soil and cultivation in Yoruba-land.

IN the one hundred and third Annual Report of the Royal Botanic Garden, Calcutta, Dr. King says that the attention of the staff during the past year was devoted chiefly to the maintenance, in as high a state of efficiency as possible, of the various departments of the garden. Special attention was given to the herbarium, and a considerable number of new species were described. The sum of 1000 rupees having been granted in order that specimens might be obtained in Burma and Assam, Dr. King was enabled to do more than usual in these provinces. Under a recent order of the Government of India this exploration will be extended. An official document relating to Dr. King's Report, and issued by order of the Lieutenant-Governor of Bengal, contains the following passage:—"The control of Indian botanical operations has been centralized in the Calcutta Gardens, and the Superintendent has been appointed Director of the Botanical Survey of India. The grants promised by the Administrations of Burma and Assam will enable collections to be made on a larger scale and more continuously. As this work will constitute a separate Department, it has been ordered that in future years a separate Report should be submitted on the subject."

THE twenty-eighth Annual Report of the Government cinchona plantation and factory in British Sikkim, by Dr. King, has been issued. At the end of the financial year 1889-90 the plantation consisted of 4,682,401 trees of various ages, and of a nursery stock amounting to 264,000 seedlings. The crop collected during the year amounted to 304,705 pounds. The products of the factory were $1833\frac{1}{8}$ pounds of sulphate of quinine, and 6578 pounds of febrifuge. The whole of the quinine and the greater part of the febrifuge were manufactured by the new fusel-oil process; and, as the arrangements for working this process were quite completed during the year, the old acid and alkali method of manufacture has now been definitively abandoned. An additional year's experience of the fusel-oil process confirms Dr. King's previously expressed opinion of its complete success. The quinine turned out by it is of excellent appearance and great purity, in the latter respect comparing favourably with most of the brands of the drug of European manufacture.

THE National Association for the Promotion of Technical and Secondary Education have issued some valuable "notes" on the working of the Technical Instruction Act; and a series of "suggestions" to County Councils and other local authorities on the use of the new fund allocated to County Councils for the purposes of technical and secondary education.

The greater part of the new number of the *Mineralogical Magazine* consists of a most careful and interesting paper, by Mr. L. Fletcher, on the Mexican meteorites, with especial regard to the supposed occurrence of wide-spread meteoritic showers. The number includes also the following papers, all of which are short:—A visit to the calcite quarry in Iceland, by J. L. Hoskyns-Abrahall; sanguinite, a new mineral, and Krennerite, by H. A. Miers; notes on Bowenite, or pseudo-jade from Afghanistan, by Major-General C. A. McMahon; on the relations between the gliding planes and the solution planes of augite, by Prof. J. W. Judd.

It is reported from India that Mr. John Eliott, Meteorological Reporter to the Government, starts this month on tour, and will first visit Quetta and then go down the Indus to Kurrachee and Bombay, and finally make his way to Calcutta.

We learn from *La Nature* that on September 21 Marseilles was visited by a severe thunderstorm accompanied by torrential rain and hail. The storm began about 6 a.m., and lasted $2\frac{1}{2}$ hours. Everything that was in front of the shops was carried away, and the port was filled with *débris* of all kinds. Many of the hailstones were of the size of walnuts and even of fowl's eggs; several places were struck by lightning, and many animals were drowned. Such atmospheric disturbances are said to be very rare at Marseilles.

The Agricultural Department of Bohemia has published, in a quarto volume of 138 pages, the results of the rainfall observations made in that country during the year 1889, in continuation of the work formerly undertaken by the Hydrographic Committee, under the direction of Dr. Studnička. The stations now number 707; the rainfall is measured at 6 a.m. daily, and the amount set down to the previous day. For a large number of stations the daily rainfall is entered in the tables. The yearly results are shown upon a map, by means of reference numbers to the tables and curves for each 100 mm., and the various watersheds are also shown by red outlines. The tables show the number of wet days, and the maximum daily falls at each station. This rainfall service is now one of the most complete in Europe.

The Annual Report of the Director of the Royal Alfred Observatory, Mauritius, for the year 1888 shows that the temperature of the air was $0^{\circ}\cdot5$ below the average of the last fourteen years, and below the average in every month except August and November. The greatest rainfall in one day was 4·5 inches, on March 19, on which day 1·3 inch fell in fifty minutes. The island has not been visited by a hurricane since March 21, 1879, but the Observatory continues its useful work of examining the logs of ships traversing the Indian Ocean, and synoptic charts have been prepared for eighteen days on which tropical cyclones were experienced. The upper clouds, when visible, generally travelled from the westward. The number of unusual sky glows was less than in 1887, but were observed in all months except September, October and December. There seems to be some connection between mortality from fever and rainfall; the maximum mortality occurs about two months after the maximum rainfall, and the minimum mortality about two months after the minimum rainfall. The report contains monthly rainfall values for eight stations, and results of the meteorological observations at Seychelles and Rodrigues.

Four interesting phenological maps of Finland appear in the *Meteorologische Zeitschrift* for September. In these, Dr. Ihne shows the date of flowering of *Ribes rubrum*, *Prunus padus*, *Syringa vulgaris*, and *Sorbus Aucuparia* in different parts of the country, by a series of zones, embracing each five days. *Ribes* and *Prunus* begin to flower earlier than the two others, and, accordingly, the zone for a given date is further north in the case of the former; their maps also present more zones. The isophanes (or lines of the same date of flowering), bordering the zones, are approximately parallel to the parallels of latitude. The regions from June 9 to 20 have more regular boundaries than those from May 26 to June 4, more equable weather having then set in. The presence of ice, and its cooling effect on water (even after melting), and so on wind, delays the time of flowering. Thus it is that islands and the land north-west of Lake Ladoga, &c., show retardation. The unequal breadth of the zones is remarkable; Dr. Ihne supposes the cause to lie in an irregular progression of the wave of heat, due to the arrangement of land and water.

A report from Nicaragua states that an earthquake occurred at Granada on September 30. No damage was done, nor did any volcanic eruption of the Mombacho take place.

A gigantic pendulum has been suspended from the centre of the second platform of the Eiffel Tower. It consists of a bronze wire, one hundred and fifteen metres long, with a steel globe weighing ninety kilogrammes at the end. The object is to demonstrate visibly the motion of the earth.

In the course of archæological explorations lately carried on in the Crimea, Prof. Vasselovski found painted human bones in two graves—six skeletons in one grave, and one in another. Prof. Grempler, of Breslau, is of opinion that these graves belonged to the original inhabitants of the Crimea, the Cimmerians of Herodotus. They laid their dead on elevated spots, so that the birds might consume the flesh. When quite bleached, the skeletons were painted with some mineral pigment. Several graves containing such painted skeletons have been found in Central Asia. Only three had been previously found in the Crimea.

We learn from the *Botanical Gazette* that the Cornell University Experiment Station is making a large and important collection of cultivated plants; collectors being sent to leading nurseries, and botanists employed in many parts of the country to collect the cultivated plants.

The rich algological herbarium collected by the late Prof. F. Hauck, of Trieste, has been purchased by Mme. Weber van der Bosse, of Amsterdam.

The *Victorian Naturalist* learns from Mr. Tisdall that the English foxglove has established itself on the slopes of the Stringer's Creek Valley, near Walhalla. Last season in some parts the banks were purple with them.

Nautilus shells are being picked up by fortunate hunters at Portland, Victoria. The *Portland Guardian* says the search after the shells is very keen, and that before daylight numbers of enthusiasts visit the beaches ready to prosecute their searches as soon as the morning breaks.

The Museum Committee of the Leicester Town Council, in their twelfth Report, just issued, are able to give a most satisfactory account of the institution under their charge. The building of the Town Museum has lately undergone extensive repairs, and many important additions have been made to the various departments. We may note that a very ingenious method for the exhibition of coins is in use. The pulling of a lever rotates a frame—containing cards in which the coins are inserted—in such a manner that the obverse and reverse, with a full description of each coin, are shown at the will of the observer. This method has been devised by Mr. Montagu Browne, the Curator.

A valuable paper, by Mr. E. Wilson, on fossil types in the Bristol Museum, has been reprinted from the *Geological Magazine* for August and September 1890. The Bristol Museum, it seems, contains 186 distinct fossil forms; and many of them "possess for the student of British palæontology a very high interest, not only on account of the remarkable nature of the fossils themselves, but also from the fact of their having been described by some of the most distinguished of palæontologists."

MESSRS. CASSELL AND CO. have issued Part 24 of their "New Popular Educator." It includes a coloured map of France.

FOUR new parts of the "Encyklopædie der Naturwissenschaften" (Breslau, E. Trewendt) have been issued. In Parts 58 and 59 of the second Abtheilung some important contributions are made to the dictionary of chemistry included in this great work. Parts 5 and 6 of the third Abtheilung contain portions of a hand-book of physics.

THE preliminary surveys for the projected Onega-White Sea Canal have been completed. The British Vice-Consul at Archangel in his last Report says that the following facts have been established. The level of the White Sea is about 15 feet higher than that of the Lake Onega; and the length of the proposed canal would be 219 versts, of which 129 versts are a natural waterway. The proposed measurements of the canal are—breadth, 63 feet; at the locks 112 feet; and along its other portions the proposed depth is 10 feet. The cost is estimated at about 7,500,000r. (£800,000), not including the expenses incurred in the construction of a port at a point on the coast of the White Sea. With the construction of the canal it is expected that the cost of transport of goods from St. Petersburg to Archangel will be diminished from 1r. per poud to 40c. The canal will afford every facility for the transport of fish from the plentiful fishing-grounds of the White Sea to St. Petersburg, and also for the transport of the mining products of Olonets. It will also be of great strategical importance in connecting St. Petersburg and Cronstadt with the White Sea. There can be no doubt, the Vice-Consul thinks, that, considering the unlimited supply of timber in the province of Olonets, and the enterprising character of the population, shipbuilding will be carried on on a large scale when the canal is constructed.

IF we were to judge by statistics alone, we should be forced to conclude that the present system of granting rewards for the destruction of wild animals in India has had little or no effect in diminishing their numbers or in decreasing the mortality caused by them. This conclusion, however, would not be in accordance with facts. The methods according to which the statistics are collected have been so much improved that no induction can safely be made from the figures available. This is pointed out in a recent Report of the Revenue Department of the Government of Madras. The Report continues:—"The experience of almost every District officer who has been some years in the country would be that the number of destructive wild animals had largely decreased with the advance of cultivation and the progress of railways, and the evidence of natives would probably be the same. There are parts of the country still where, owing to the existence of forest and difficulty of access, wild animals of prey continue to exist in large numbers, and it is the case that, owing to various causes, Europeans at all events do less now in the way of killing large game than formerly was the case. They have less time to spare from their official duties, and less money to spend. It can hardly, however, be doubted that, owing to the existence of the system of granting rewards for animals slain, native shikaris are encouraged to maintain a profession which otherwise probably they would give up from want of support, and for this reason, if for no other, the Board would not wish to see at present any change made in the system of granting rewards. It may be hoped that the construction of the East Coast Railway, and the branch from it through the heart of the Vizagapatam district to the Central Provinces, will tend in a great measure to reduce the number of wild animals in the districts where they now do very considerable damage. Cultivation and population in tracts now given up to jungle and grass will increase largely, and the need of wood for the railways will lead probably to the destruction of large areas of jungles, which now exist in tracts which should be devoted to agriculture."

NATURALISTS will read with interest a paper in *Humboldt* for September, in which Prof. Forel, of Zürich, gives the results of a visit he lately paid to Tunis and Eastern Algeria, chiefly to observe the ants there. Looking from a ship at the dreary grey wastes, and the large date-palm oasis of Gabes, one fancies all animal life must be concentrated under the palms. But really there is very little of it there, and hardly anything singular; while the sand of the desert contains, round each of the poor, small, sparse plants, a host of beetles and other insects, many of them with striking adaptations and peculiarities. Some live on excrement of camels, asses, &c., some on the plants, and some prey on other animals, big and small. In one ant-hill he found that several ants had a small brown object clinging to the lower part of an antenna; in some cases, one on either antenna. On examination, this fell off, and was found to be a small beetle, which evidently clings there as guest; it has tufts of hair, which are probably licked by the ant. The host did not seem to trouble itself about this little creature, which, by its odd post, is enabled to accompany the ant in its wanderings and changes of abode. Prof. Forel remarks on the peaceful character of the ants in that region; with few exceptions they avoid fighting, and only one ant was found capable of piercing the human skin.

THE phenomenon of globular lightning was imitated by M. Planté, it will be remembered, with his secondary batteries. It has been recently shown by Herr von Lepel (*Met. Zeits.*) that this can also be done with so-called statical electricity, obtained from an influence-machine. Two thin brass-wire points from the poles of a powerful machine being held at a certain distance from the opposite sides of an insulated plate of mica, ebonite, glass, or the like, there appear small red luminous balls, which move about, now quickly, now slowly, and are sometimes still. Even better effects were had with a glass or paper disk which had been sprayed with paraffin. Small particles of liquid or dust seem to be the carriers of the light. A slight air-current makes the spherules disappear with hissing noise. These spherules, the author remarks, are phenomena of weak tension; an increase of the tension gives a rose spark-discharge. Various interesting analogies with globular lightning are traced.

IN a long series of articles a native Japanese paper gives some interesting figures about the students of Tokio. There are 107,312 students in the whole Empire in the various colleges and other high schools (primary schools and ordinary middle schools excepted). Of this number, 38,114 represent students prosecuting their studies in the capital—that is to say, about 40 per cent. of the whole number are congregated in Tokio. Among the 38,114 students, 6,899 are domiciled in Tokio, so that the number of those coming from other localities is 31,215. The amounts which individual students spend vary from seven or eight dollars to about fifteen dollars per month. Taking the average, it may be assumed that each student spends ten dollars a month, or 120 dollars a year. Thus the total amount of money annually disbursed by these lads is a little over 3,700,000 dollars. In other words, money aggregating over three millions and a half is being yearly drawn from the provinces to the capital through this channel. The provinces receive little in return, for few of the students ever go back to their homes, their sole ambition being to remain in the capital, and there rise to eminence in some walk of life.

THE British Consul at St. Jago de Cuba, in his latest Report, refers to the disease in the cocoa-nut plantations there, and the result of the investigations into the pest made by the Academy of Sciences of Havanna. Their Report attributed the disease to a microscopic fungus of the genus *Uredo*, and stated that the

only remedy was to cut down and burn all the trees attacked. Dr. Galves, however, endeavoured at the time to convince the Academy that this was an error, and that the disease proceeded from an hemipterous insect of the genus *Coccus*, which he classified as *Diaspis vandalicus*, and afterwards Dr. Valdés Dominguez, of Baracoa, confirmed this opinion. Finally, at the end of last year, Dr. Carlos de la Torre, a member of the Academy, set all doubt at rest, and proved that the *Uredo* referred to by the Commission did not even exist in the cocoa-nut trees, and that the small stains which had been mistaken for it were normal to the plant, and existed both in the healthy and attacked trees, and that the real cause was the *Diaspis vandalicus* of Galves, together with three other species of *Coccus*. The first symptom of the disease is the appearance on the under side of the leaflets of the fronds, of small white stains, almost imperceptible. These soon attain the size of a pepper-corn, and impart a general white colour to the leaflets, which change, later on, to yellow, and finally dry up.

The additions to the Zoological Society's Gardens during the past week include two Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented by Captain C. Crawford-Caffier, R.N.; an African Civet Cat (*Viverra civetta*), a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Lieut.-Colonel W. Gordon Pachett, W.I.R.; a Serval (*Felis serval*) from West Africa, presented by Mr. J. H. Cheetham, F.Z.S.; two Long-fronted Gerbilles (*Gerbillus longifrons* ♂ ♀) from Western Asia, presented by Mrs. F. A. Kitchener; two Blackcaps (*Sylvia atricapilla*), a Garden Warbler (*Sylvia hortensis*), British, presented by Mr. J. Young, F.Z.S.; three Passerine Parrots (*Psittacula passerina*) from Brazil, presented by Mr. Arthur Robottom; a Barnard's Parrakeet (*Platycercus barnardi*) from Australia, presented by Mrs. E. M. Temple; a Golden Eagle (*Aquila chrysaëtus*) from Morocco, presented by Mr. Charles A. Payton; a Snowy Egret (*Ardea candidissima*) from America, presented by Mr. H. H. Sharland; a Herring Gull (*Larus argentatus*), three Lesser Black-backed Gulls (*Larus fuscus*), British, presented by the Hon. J. S. Gathorne Hardy, M.P., F.Z.S.; two Purple Porphyrios (*Porphyrio cæruleus*) from Sicily, presented by Mr. J. I. S. Whitaker; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mrs. Wanklyn; two North African Jackals (*Canis anthus*) from North Africa, deposited; two Philantomba Antelopes (*Cephalophus maxwelli* ♂ ♀) from South Africa, three Passerine Parrots (*Psittacula passerina*) from Brazil, a Lucian's Parrakeet (*Palæornis luciania* ♂) from China, purchased; six Esquimaux Dogs (*Canis familiaris* var. 4 ♂ 2 ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on October 16 = 23h. 41m. 28s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4940	—	—	23 15 9	+ 7 28
(2) G.C. 4964	—	Bluish-green.	23 20 36	+41 47
(3) 280 Schj.	8	Very red.	23 55 39	+59 44
(4) ψ_1 Aquarii	4·5	Whitish-yellow.	23 9 34	− 9 45
(5) ψ_3 Aquarii	5·5	White.	23 12 43	−10 16
(6) V Tauri	Var.	Reddish.	4 45 40	+17 21
(7) R Tauri	Var.	Very red.	4 22 16	+ 9 55
(8) R Lyræ	Var.	Red.	18 52 0	+43 48

Remarks.

(1) The spectrum of this nebula has not yet been recorded. It is described as "considerably bright; pretty small; round; pretty suddenly brighter in the middle."

(2) This remarkable planetary nebula was stated by Lassell to be bi-annular, consisting of a nucleus and two rings, whilst Lord Rosse observed a spiral structure. Herschel described it as "a very remarkable planetary nebula; very bright; pretty small; round; blue." The spectrum is also a remarkable one, consisting of the three ordinary lines of the nebulæ, and, in addition, a line near wave-length 470, which was first seen by Dr. Huggins. This line occurs only in this nebula, with the exception of the Orion nebula, where it has been recorded by Mr. Taylor, and is also feebly impressed in Mr. Lockyer's photograph of the spectrum. It is, however, far brighter in the planetary nebula than in the Orion nebula. It has been suggested that the line is really the blue band of carbon under such conditions that most of the light is concentrated at about wave-length 470, as is sometimes the case in the laboratory. If this be so, it ought not to be so sharply defined as the other lines, and an observation should be made with reference to this point. Other lines, especially near the carbon flutings in the green, should also be looked for.

(3) The spectrum of this star, according to the observations of Dunér, is undoubtedly a banded one, but whether it is one of Group II. or Group VI. is doubtful. The strongest band is in the red, and this is very little degraded. In the green there is another band, which is wider but not so strong. With less certainty other bands were detected in the red and yellow-green, and another "very vaguely" in the blue. The spectrum is well worth further investigation, as we are likely to learn most by a study of the apparent departures from the regular types.

(4) A star of the solar type, with fine lines (Konkoly). The spectrum should be further examined as to whether the temperature is increasing (Group III.) or decreasing (Group V.). The fineness of the lines tend to show that it is the latter.

(5) A star of Group IV. (Konkoly).

(6) The spectrum of this variable, according to Gore's Catalogue, has yet to be determined, and the forthcoming maximum of October 19 may therefore be taken advantage of. The period is about 169 days, and the magnitude ranges from 8·3–9·0 at maximum to $<$ 12·8 at minimum.

(7) This variable of Group II. will reach a maximum about October 21. The period is about 326 days, and the variation from 7·4–9·0 at maximum to $<$ 13 at minimum. Bright lines and carbon flutings should be looked for.

(8) This well-known variable of Group II. will reach a maximum about October 24. The range is small (4·3–4·6) and the period short (46 days), two conditions which appear to go together, exactly as is demanded by the collision theory of this kind of variability. Further, if that explanation be correct, it is not likely that bright lines will appear at maximum, and this may be made a test observation. A. Fowler.

Theory of Solar Radiation.—Mr. W. Goff has written a pamphlet in which he propounds a theory of the sun's radiation of heat. It is well known that geologists and physicists demand a much longer duration of the sun's past activity than the present estimate of the expenditure of heat would allow, supposing that there have been no unknown means of supply. Dr. Croll, in "Stellar Evolution," brings forward evidence in support of the longer periods. To account for the great disparity that exists between the results arrived at from different points of view, he assumed that the primitive nebulous mass possessed a store of energy derived from the impact of two large cold bodies moving with enormous velocities. Mr. Goff also thinks that the grounds upon which geologists and biologists found their conclusions are more certain and trustworthy than those of the physicist. He does not, however, supplement gravitational energy by energy derived from other sources, in order to account for the sun's outlay, but shows that the methods adopted for arriving at values of the amount are at fault. In his words:—"Radiant energy is a very different thing from absorbed heat, and I have endeavoured to demonstrate that its value must be considerably less. Also I have shown that the current estimates of the sun's annual loss of heat are founded entirely on an absorbed heat basis. They must, consequently, if my arguments are correct, be far in excess of what his expenditure actually is." The distinction between radiant energy and absorbed heat is clearly indicated, and it is evident that, unless the value of each is the same, the present determinations of the sun's emission of heat must be incorrect.

The Satellites of Saturn.—The micrometer measures of the satellites of Saturn made by Dr. Hermann Struve with the 30-inch Pulkova equatorial, has led to some interesting and im-

portant results. In a recent communication (*Astronomische Nachrichten*, No. 2983), the orbits of Mimas and Enceladus, and their relation to those of the other satellites, are considered. The orbit of Mimas has an eccentricity of 0·016, and an inclination of 1° 26′. The retrograde movement of the nodes, is about 1° per day, and is accompanied by a direct movement of the perisaturnium point, which is almost equal to it (− 365° and + 371° per year). The comparison of the Pulkova observations with those made at Washington (1882–86) indicates an acceleration of the mean motion of Mimas, which corresponds to a retardation in the mean movement of Tethys. Dr. Struve shows that the changes in the elements and mean motions of the two satellites may increase indefinitely, or vary between certain limits. The latter explanation is proved to be the correct one, and a discussion of the observations of Sir W. Herschel, Lassell, Marth, Newcomb, Asaph Hall, &c., leads to the conclusion that the conjunctions of Enceladus and Dione occur at the perisaturnium of the former satellite or nearly so, whilst those of Mimas and Tethys oscillate 45° about the point midway between the ascending nodes of their orbits on Saturn's equator, and perform this libration in about sixty-eight years.

The masses of Dione and Tethys inferred by Dr. Struve from the libration are respectively seven and eleven times smaller than those deduced from photometric comparisons with Titan. The result for Mimas is twenty-two times smaller than that furnished by photometry. It appears necessary to admit, therefore, that in the system of Saturn, as in that of Jupiter, either the intrinsic brilliancy of the satellites increases, or their density decreases, as the planet is approached. A knowledge of the masses of the four above-named satellites, determined photometrically and found by Dr. Struve, allows those of Enceladus and Rhea to be estimated with some probability. The following are the calculated and the hypothetical values in terms of the mass of Saturn: Mimas, 1/11,500,000; Enceladus, 1/4,000,000; Tethys, 1/767,000; Dione, 1/528,000; Rhea, 1/200,000; Titan, 1/4700. By adopting the above hypothetical values of the masses of Enceladus and Rhea, the observed and calculated values of the secular motions of the nodes and apses are found to agree in a very satisfactory manner. The spheroidal constant of Saturn has been determined as 0·0258, which differs considerably from the value 0·0223 assumed in a previous paper (*Astronomische Nachrichten*, No. 2946). This alteration obviates the necessity of giving the ring-system a sensible mass in the calculations.

A New Comet (*d* 1890).—A faint comet was discovered by Mr. E. E. Barnard, of the Lick Observatory, on the 6th inst. It was then situated in Capricornus.

ANTARCTIC EXPLORATION.

THE following address, on "The Objects of Antarctic Exploration," was delivered at the annual meeting of the Bankers' Institute of Australasia, at Melbourne, on Wednesday, August 27, by Mr. G. S. Griffiths, F.G.S., F.R.G.S., His Excellency the Earl of Hopetoun being in the chair.

Mr. Griffiths said,—My experience, during the four years which have elapsed since this project was first mooted in Melbourne, is that any reference to the subject is sure to be met with the query, *Cui bono?* What good can it do? What benefit can come from it? What is the object to be served by such an expedition?

In setting myself to the task of answering these questions, let me observe that it would indeed be strange if an unexplored region, 8,000,000 square miles in area—twice the size of Europe—and grouped around the axis of rotation and the magnetic pole, could fail to yield to investigators some novel and valuable information. But when we notice that the circle is engirdled without by peculiar physical conditions which must be correlated to special physical conditions within, speculation is exchanged for a confident belief that an adequate reward must await the skilled explorer. The expected additions to the geography of the region are, of all the knowledge that is to be sought for there, the least valuable. Where so many of the physical features of the country—the hills, the valleys, and the drainage lines—have been buried beneath the snow of ages, a naked outline, a bare skeleton of a map, is the utmost that can be delineated. Still, even such knowledge as this has a distinct value, and as it can be acquired by the explorers as they proceed about their more important researches, its relatively small value ought not to be admitted as a complete objection to any enterprise which has other objects of importance. Our present acquaintance with the geography of the region is excessively limited. Ross just viewed the coasts of Victoria Land, between 163° E. and 160° W. long.; he trod its barren strand twice, but on each occasion for a few minutes only. From the adjacent gulf he measured the heights of its volcanoes, and from its offing he sketched the walls of its icy barrier. Wilkes traced on our map a shore-line from 97° E. to 167° E. long., and he backed it up with a range of mountains, but he landed nowhere. Subsequently Ross sailed over the site assigned to part of this land, and hove his lead 600 fathoms deep where Wilkes had drawn a mountain. He tells us that the weather was so very clear, that had high land been within 70 miles of that position he must have seen it ("Ross's Voyage," 1278). More recently Nares, in the *Challenger*, tested another part of Wilkes's coast-line, and with a like result; and these circumstances throw doubts upon the value of his reported discoveries. D'Urville subsequently followed a bold shore for a distance of about 300 miles from 136° E. to 142° E. long.; whilst in 67° S. lat., and between 45° E. and 60° E. long., are Enderby's and Kemp's lands. Again, there is land to the south of the Horn, which trends from 45° to 75° S. lat.. These few discontinuous coast-lines comprise all our scanty knowledge of the Antarctic land. It will be seen from these facts that the principal geographical problem awaiting solution in these regions is the interconnection of these scattered shores. The question is, Do they constitute parts of a continent, or are they, like the coasts of Greenland, portions of an archipelago, smothered under an overload of frozen snow, which conceals their insularity? Ross inclined to the latter view, and he believed that a wide channel leading towards the Pole existed between North Cape and the Balleny Islands ("Ross's Voyage," 1221). This view was also held by the late Sir Wyville Thomson. A series of careful observations upon the local currents might throw some light upon these questions. Ross notes several such in his log. Off Possession Island a current, running southward, took the ships to windward (*ibid.*, 1195). Off Coulman Island another drifted them in the same direction, at the rate of eighteen miles a day (*ibid.*, 1204). A three-quarter knot northerly current was felt off the barrier, and may have issued from beneath some part of it. Such isolated observations are of little value, but they were multiplied, and were the currents correlated with the winds experienced, the information thus obtained might enable us to detect the existence of straits, even where the channels themselves are masked by ice-barriers.

Finally, it is calculated that the centre of the polar ice-cap must be three miles, and may be twelve miles, deep, and that, the material of this ice mountain being viscous, its base must spread out under the crushing pressure of the weight of its centre. The extrusive movement thus set up is supposed to thrust the ice cliffs off the land at the rate of a quarter of a mile per annum. These are some of the geographical questions which await settlement.

In the geology of this region we have another subject replete with interest. The lofty volcanoes of Victoria Land must present peculiar features. Nowhere else do fire and frost divide the sway so completely. Ross saw Erebus belching out lava and ashes over the snow and ice which coated its flanks. This circumstance leads us to speculate on the strata that would result from the alternate fall of snow and ashes during long periods and under a low temperature. Volcanoes are built up, as contradistinguished from other mountains, which result from elevation or erosion. They consist of *débris* piled round a vent. Lava and ashes surround the crater in alternate layers. But in this polar region the snowfall must be taken into account as well as the ash deposit and the lava-flow. It may be thought that any volcanic ejecta would speedily melt the snow upon which they fell, but this does not by any means necessarily follow. Volcanic ash, the most widespread and most abundant material ejected, falls comparatively cold, cakes, and then forms one of the most effective non-conductors known. When such a layer, a few inches thick, is spread over snow, even molten lava may flow over it without melting the snow beneath. This may seem to be incredible, but it has been observed to occur. In 1828, Lyell saw on the flanks of Etna a glacier sealed up under a crust of lava. Now, the Antarctic is the region of thick-ribbed ice. All exposed surfaces are quickly covered with snow. Snow-falls, fish-falls, and lava-flows must have been heaping themselves up around the craters during unknown ages. What has

been the result? Has the viscosity of the ice been modified by the intercalation of beds of rigid lava and of hard-set ash? Does the growing mass tend to pile up or to settle down and spread out? Is the ice wasted by evaporation, or does the ash-layer preserve it against this mode of dissipation? These interesting questions can be studied round the South Pole, and perhaps nowhere else so well.

Another question of interest, as bearing upon the location of the great Antarctic continent, which it is now certain existed in the Secondary period of geologists, is the nature of the rocks upon which the lowest of these lava-beds rest. If they can be discovered, and if they then be found to be sedimentary rocks such as slates and sandstones, or plutonic rocks such as granite, they will at once afford us some data to go upon, for the surface exposure of granite signifies that the locality has been part of a continental land sufficiently long for the weathering and removal of the many thousands of feet of sedimentary rocks which of necessity overlie crystalline rocks during their genesis; whilst the presence of sedimentary rocks implies the sometime proximity of a continent from the surfaces of which alone these sediments, as rain-wash, could have been derived.

As ancient slate rocks have already been discovered in the ice-clad South Georgias, and as the drag-nets of the *Erebus* and the *Challenger* have brought up from the beds of these icy seas fragments of sandstones, slates, and granite, as well as the typical blue mud which invariably fringes continental land, there is every reason to expect that such strata will be found.

Wherever the state of the snow will permit, the polar mountains should be searched for basaltic dykes, in the hope that masses of specular iron and nickel might be found, similar to those discovered by Nordenskiöld, at Ovifak, in North Greenland. The interest taken in these metallic masses arises from the fact that they alone, of all the rocks of the earth, resemble those masses of extra-terrestrial origin which we know as meteorites. Such bodies of unoxidized metal are unknown elsewhere in the mass, and why they are peculiar to the Arctic it is hard to say. Should similar masses be found within the Antarctic, a fresh stimulus would be given to speculation. Geologists would have to consider whether the oxidized strata of the earth's crust thin out at the poles; whether in such a case the thinning is due to severe local erosion, or to the protection against oxygen afforded to the surface of the polar regions by their ice-caps, or to what other cause. Such discoveries would add something to our knowledge of the materials of the interior of our globe and their relation to those of meteorites.

Still looking for fresh knowledge in the same direction, a series of pendulum observations should be taken at points as near as possible to the Pole. Within the Arctic circle the pendulum makes about 240 more vibrations per day than it does at the equator. The vibrations increase in number there because the force of gravity at the earth's surface is more intense in that area, and this again is believed to be due to the oblateness of that part of the earth's figure, but it might be caused by the bodily approach to the surface at the poles of the masses of dense ultra-basic rocks just referred to. Thus, pendulum experiments may reveal to us the earth's figure, and a series of such observations, recorded from such a vast and untried area, must yield important data for the physicist to work up. We should probably learn from such investigations whether the earth's figure is as much flattened at the Antarctic as it is known to be at the Arctic.

We now know that in the past the North Polar regions have enjoyed a temperate climate more than once. Abundant seams of Palæozoic coal, large deposits of fossiliferous Jurassic rocks, and extensive Eocene beds, containing the remains of evergreen and deciduous trees and flowering plants, occur far within the Arctic circle. This circumstance leads us to wonder whether the corresponding southern latitudes have ever experienced similar climatic vicissitudes. Conclusive evidence on this point it is difficult to get, but competent biologists who have examined the floras and faunas of South Africa and Australia, of New Zealand, South America, and the isolated islets of the Southern Ocean, find features which absolutely involve the existence of an extensive Antarctic land—a land which must have been clothed with a varied vegetation, and have been alive with beasts, birds, and insects. As it also had had its fresh-water fishes, it must have had its rivers flowing and not frost-bound, and in those circumstances we again see indications of a modified Antarctic climate. Let us briefly consider some of the evidence for the existence of this continent. We are told by Prof. Hutton, of Christchurch, that 44 per cent. of the New Zealand flora is of Antarctic origin. The Auckland, Campbell, and Macquarie Islands all support Antarctic plants, some of which appear never to have reached New Zealand. New Zealand and South America have three flowering plants in common, also two fresh-water fishes, five seaweeds, three marine crustaceans, one marine mollusk, and one marine fish. Similarly New Zealand and Africa have certain common forms, and the floras and faunas of the Kerguelen, the Crozets, and the Marion Islands are almost identical, although in each case the islands are very small, and very isolated from each other and from the rest of the world. Tristan d'Acunha has 58 species of marine Mollusca, of which number 13 are also found in South America, six or seven in New Zealand, and four in South Africa (Hutton's "Origin of New Zealand Flora and Fauna"). Temperate South America has 74 genera of plants in common with New Zealand, and 11 of its species are identical (Wallace's "Island Life"). Penguins of the genus Eudyptes are common to South America and Australia (Wallace, "Dist. of Animals," 1399). Three groups of fresh-water fishes are entirely confined to these two regions. Aphritis, a fresh-water genus, has one species in Tasmania and two in Patagonia. Another small group of fishes known as the Haplochitonidæ inhabit Tierra del Fuegia, the Falklands, and South Australia, and are not found elsewhere, while the genus Galaxias is confined to South Temperate America, New Zealand, and Australia. Yet the lands which have these plants and animals in common are so widely separated from each other that they could not now possibly interchange their inhabitants. Certainly towards the equator they approach each other rather more, but even this fact fails to account for the present distribution, for, as Wallace has pointed out, "the heat-loving Reptilia afford hardly any indications of close affinity between the two regions" of South America and Australia, "whilst the cold-enduring Amphibia and fresh-water fishes offer them in abundance" (Wallace, "Dist. of Animals," 1400). Thus we see that to the north interchange is prohibited by tropical heat, while it is barred to the south by a nearly shoreless circumpolar sea. Yet there must have been some means of intercommunication in the past, and it appears certain that it took the shape of a common fatherland for the various common forms from which they spread to the northern hemisphere. As this fatherland must have been accessible from all these scattered southern lands, its size and its disposition must have been such as would serve the emigrants either as a bridge or as a series of stepping-stones. It must have been either a continent or an archipelago.

But a further and a peculiar interest attaches to this lost continent. Those who have any acquaintance with geology know that the placental Mammalia—that is, animals which are classed with such higher forms of life as apes, cats, dogs, bears, horses, and oxen—appear very abruptly with the incoming of the Tertiary period. Now, judging by analogy, it is not likely that these creatures can have been developed out of Mesozoic forms with anything like the suddenness of their apparent entrance upon the scene. For such changes they must have required a long time, and an extensive region of the earth, and it is probable that each of them had a lengthy series of progenitors, which ultimately linked it back to lower forms.

Why, then, it is constantly asked, if this was the sequence of creation, do these missing links never turn up? In reply to this query, it was suggested by Huxley that they may have been developed in some lost continent, the boundaries of which were gradually shifted by the slow elevation of the sea margin on one side and its simultaneous slow depression upon the other, so that there has always been in existence a large dry area with its live stock. This dry spot, with its fauna and flora, like a great raft or Noah's Ark, moved with great slowness in whatever direction the great earth-undulation travelled. But to-day this area, with its fossil evidences, is a sea-bottom; and Huxley supposes that the continent, which once occupied a part of the Pacific Ocean, is now represented by Asia.

This movement of land-surface-translation eastwards eventually created a connection between this land and Africa and Europe, and if when this happened the Mammalia spread rapidly over these countries, this circumstance would account for the abruptness of their appearance there.

Now, Mr. Blanford, the President of the Geological Society of London, in his annual address, recently delivered, advances matters a stage further, for he tells us that a growing acquaintance with the biology of the world leads naturalists to a belief that the placental Mammalia, and other of the higher forms of

terrestrial life, originated during the Mesozoic period, still further to the southwards—that is to say, in the lost Antarctic continent, for the traces of which we desire to seek.

But it almost necessarily follows that wherever the Mammalia were developed there also man had his birthplace, and if these speculations should prove to have been well founded we may have to shift the location of the Garden of Eden from the northern to the southern hemisphere.

I need hardly suggest to you that possibilities such as these must add greatly to our interest in the recovery of any traces of this mysterious region. This land appears to have sunk beneath the seas after the close of the Mesozoic. Now, the submergence of any mass of land will disturb the climatic equilibrium of that region, and the disappearance of an Antarctic continent would prove extremely potent in varying the climate of this hemisphere. For to-day the sun's rays fall on the South Polar regions to small purpose. The unstable sea absorbs the heat, and in wide and comparatively warm streams it carries off the caloric to the northern hemisphere to raise its temperature at the expense of ours. But when extensive land received those same heat rays, its rigid surfaces, so to speak, tethered their caloric in this hemisphere, and thus when there was no mobile current to steal northwards with it, warmth could accumulate and modify the climate.

Under the influences of such changes the icy mantle would be slowly rolled back towards the South Pole, and thus many plants and animals were able to live and multiply in latitudes that to-day are barren. What has undoubtedly occurred in the extreme north is equally possible in the extreme south. But if it did occur—if South Polar lands, now ice-bound, were then as prolific of life as Disco and Spitzbergen once were—then, like Spitzbergen and Disco, the unsubmerged remnants of this continent may still retain organic evidences of the fact in the shape of fossil-bearing beds, and the discovery of such deposits would confirm or confute such speculations as these. The key to the geological problem lies within the Antarctic circle, and to find it would be to recover some of the past history of the southern hemisphere. There is no reason to despair of discovering such evidence, as Dr. M'Cormack, in his account of Ross's voyage, records that portions of Victoria Land were free from snow, and therefore available for investigation; besides which their surface may still support some living forms, for they cannot be colder or bleaker than the peaks which rise out of the continental ice of North Greenland, and these, long held to be sterile, have recently disclosed the existence upon them of a rich though humble flora.

We have now to consider some important meteorological questions. If we look at the distribution of the atmosphere around the globe we shall see that it is spread unequally. It forms a stratum which is deeper within the tropics than about the poles and over the northern than over the southern hemisphere, so that the barometer normals fall more as we approach the Antarctic than they do when we near the Arctic. Maury, taking the known isobars as his guide, has calculated that the mean pressure at the North Pole is 29·1, but that it is only 28 at the South (Maury's "Meteorology," 259). In other words, the Antarctic circle is permanently much barer of atmosphere than any other part of the globe. Again, if we consult a wind chart we shall see that both poles are marked as calm areas. Each is the dead centre of a perpetual wind vortex, but the South Polar indraught is the stronger. Polarward winds blow across the 45th degree of north latitude for 189 days in the year, but across the 45th degree of south latitude for 209 days. And while they are drawn in to the North Pole from over a disk-shaped area 5500 miles in diameter, the South Polar indraught is felt throughout an area of 7000 miles across. Lastly, the winds which circulate about the South Pole are more heavily charged with moisture than are the winds of corresponding parts of the other hemisphere. Now, the extreme degree in which these three conditions—of a perpetual grand cyclone, a moist atmosphere, and a low barometer—co-operate without the Antarctic, ought to produce, within it, an exceptional meteorological state, and the point to be determined is what that condition may be. Maury maintained that the conjunction will make the climate of the South Polar area milder than that of the north. His theory is that the saturated winds being drawn up to great heights within the Antarctic must then be eased of their moisture, and that simultaneously they must disengage vast quantities of latent heat; and it is because more heat must be liberated in this manner in the South Polar regions than in the north that he infers a less severe climate for the Antarctic. He estimates that the resultant relative differences between the two polar climates will be greater than that between a Canadian and an English winter (Maury's "Meteorology," p. 466). Ross reports that the South Polar summer is rather colder than that of the north, but still the southern winter may be less extreme, and so the mean temperature may be higher. If we examine the weather reports logged by Antarctic voyagers, instead of the temperature merely, the advantage still seems to rest with the south. In the first place, when the voyager enters the Antarctic, he sails out of a tempestuous zone into one of calms. To demonstrate the truth of this statement, I have made an abstract of Ross's log for the two months of January and February 1841, which he spent within the Antarctic circle. To enable everyone to understand it, it may be well to explain that the wind force is registered in figures from 0, which stands for a dead calm, up to 12, which represents a hurricane. I find that during these 60 days it never once blew with the force 8—that is, a fresh gale; only twice did it blow force 7, and then only for half a day each time. Force 5 to 6—fresh to strong breezes—is logged on 21 days. Force 1 to 3—that is, gentle breezes—prevailed on 34 days. The mean wind force registered under the entire 60 days was 3·43—that is, only a four to five knot breeze. On 38 days, blue sky was logged. They never had a single fog, and on 11 days only was it even misty. On the other hand, snow fell almost every second day. We find such entries as these—"beautifully clear weather," and "atmosphere so extraordinarily clear that Mount Herschel, distant 90 miles, looked only 30 miles distant." And again, "land seen 120 miles distant, sky beautifully clear." Nor was this season exceptional, so far as we can tell, for Dr. M'Cormack, of the *Erebus*, in the third year of the voyage, and after they had left the Antarctic for the third and last time, enters in his diary the following remark. He says: "It is a curious thing that we have always met with the finest weather within the Antarctic circle; clear, cloudless sky, bright sun, light wind, and a long swell" (M'Cormack's "Antarctic Voyage," vol. i. p. 345). It would seem as if the stormy westerlies, so familiar to all Australian visitors, had given to the whole southern hemisphere a name for bad weather, which, as yet at least, has not been earned by the South Polar regions. It is probable, too, that the almost continuous gloom and fog of the Arctic (Scoresby's "Arctic Regions," pp. 97 and 137) July and August have prejudiced seamen against the Antarctic summer. The true character of the climate of this region is one of the problems awaiting solution. Whatever its nature may be, the area is so large and so near to us that its meteorology must have a dominant influence on the climate of Australia, and on this fact the value of a knowledge of the weather of these parts must rest.

To turn to another branch of science, there are several questions relating to the earth's magnetism which require for their solution long-maintained and continuous observations within the Antarctic circle. The mean or permanent distribution of the world's magnetism is believed to depend upon causes acting in the interior of the earth, while the periodic variations of the needle probably arise from the superficial and subordinate currents produced by the daily and yearly variations in the temperature of the earth's surface. Other variations occur at irregular intervals, and these are supposed to be due to atmospheric electricity. All these different currents are excessively frequent and powerful about the poles, and a sufficient series of observations might enable physicists to differentiate the various kinds of currents, and to trace them to their several sources, whether internal, superficial, or meteoric. To do this properly at least one land observatory should be established for a period. In it the variation, dip, and intensity of the magnetic currents, as well as the momentary fluctuations, of these elements, would all be recorded. Fixed term days would be agreed on with the observatories of Australia, of the Cape, America, and Europe, and during these terms a concerted continuous watch would be kept up all round the globe to determine which vibrations were local and which general.

The present exact position of the principal south magnetic pole has also to be fixed, and data to be obtained from which to calculate the rate of changes in the future, and the same may be said of the foci of magnetic intensity and their movements. In relation to this part of the subject, Captain Creak recently reported to the British Association his conclusions in the following terms. He says:—"Great advantage to the science of

terrestrial magnetism would be derived from a new magnetic survey of the southern hemisphere extending from the parallel of 40° S. as far towards the geographical pole as possible."

Intimately connected with terrestrial magnetism are the phenomena of auroras. Their nature is very obscure, but quite recently a distinct advance has been made towards discovering some of the laws which regulate them. Thanks to the labours of Dr. Sophus Tromholt, who has spent a year within the Arctic circle studying them, we now know that their movements are not as eccentric as they have hitherto appeared to be. He tells us that the Aurora Borealis, with its crown of many lights, encircles the Pole obliquely, and that it has its lower edge suspended above the earth at a height of from 50 to 100 miles, the mean of 18 trigonometrical measurements, taken with a base line of 50 miles, being 75 miles. The aurora forms a ring round the Pole which changes its latitude four times a year. At the equinoxes it attains its greatest distance from the Pole, and at midsummer and midwinter it approaches it most closely, and it has a zone of maximum intensity which is placed obliquely between the parallels of 60° and 70° N. The length of its meridional excursion varies from year to year, decreasing and increasing through tolerably regular periods, and reaching a maximum about every eleven years, when, also, its appearance simultaneously attains to its greatest brilliancy. Again, it has its regular yearly and daily movements or periods. At the winter solstice it reaches its maximum annual intensity, and it has its daily maximum at from 8 p.m. and 2 a.m., according to the latitude. Thus at Prague, in lat. 50° N., the lights appear at about 8.45 p.m.; at Upsala, lat. 60° N., at 9.30 p.m.; at Bossekop, 70° N., at 1.30 a.m. Now, while these data may be true for the northern hemisphere, it remains to be proved how far they apply to the southern. Indeed, seeing that the atmosphere of the latter region is moister and shallower than that of the former, it is probable that the phenomena would be modified. A systematic observation of the Aurora Australis at a number of stations in high latitudes is therefore desirable.

Whether or not there is any connection between auroral exhibitions and the weather is a disputed point. Tromholt believes that such a relationship is probable ("Under the Rays," 1283). He says that, "however clear the sky, it always became overcast immediately after a vivid exhibition, and it generally cleared again as quickly" ("Under the Rays," 1235). Payer declares that brilliant auroras were generally succeeded by bad weather ("Voyage of Tegelhoff," 1324), but that those which had a low altitude and little mobility appeared to precede calms. Ross remarks of a particular display "that it was followed by a fall of snow, as usual" ("Ross's Voyage," 1312). Scoresby appears to have formed the opinion that there is a relationship indicated by his experience. It is, therefore, allowable to regard the ultimate establishment of some connection between these two phenomena as a possible contingency. If, then, we look at the eleven-year cycle of auroral intensity from the meteorological point of view, it assumes a new interest, for these periods may coincide with the cycles of wet and dry seasons, which some meteorologists have deduced from the records of our Australian climate, and the culmination of the one might be related to some equivalent change in the other. For if a solitary auroral display be followed by a lowered sky, surely a period of continuous auroras might give rise to a period of continuous cloudy weather, with rain and snow. Fritz considers that he has established this eleven-year cycle upon the strength of auroral records extending from 1583 to 1874, and his deductions have been verified by others.

In January 1886 we had a wide-spread and heavy rainfall, and also an auroral display seen only at Hobart, but which was sufficiently powerful to totally suspend communication over all the telegraph lines situated between Tasmania and the China coast. This sensitiveness upon the part of the electric currents to auroral excitation is not novel, for long experience on the telegraph wires of Scandinavia has shown that there is such a delicate sympathy between them that the electric wires there manifest the same daily and yearly periods of activity as those that mark the auroras. The current that reveals itself in fire in the higher regions of the atmosphere is precisely the same current that plagues the operator in his office. Therefore, in the records of these troublesome earth-currents, now being accumulated at the Observatory by Mr. Ellery, we are collecting valuable data, which may possibly enable the physicist to count the unseen auroras of the Antarctic, to calculate their periods of activity and lethargy, and, again, to check these with our seasons. But it need hardly be said that the observations, which may be made in the higher latitudes and directly under the rays of the Aurora Australis, will have the greater value, because it is only near the zone of maximum auroral intensity that the phenomena are manifested in all their aspects. In this periodicity of the southern aurora I have named the last scientific problem to which I had to direct your attention, and I would point out that if its determination should give to us any clue to the changes in the Australian seasons which would enable us to forecast their mutations in any degree, it would give to us, in conducting those great interests of the country which depend for their success upon the annual rainfall, an advantage which would be worth, many times over, all the cost of the expeditions necessary to establish it.

Finally, there is a commercial object to be served by Antarctic exploration, and it is to be found in the establishment of a whaling trade between this region and Australia. The price of whalebone has now risen to the large sum of £2000 a ton, which adds greatly to the possibilities of securing to the whalers a profitable return. Sir James Ross and his officers have left it on record that the whale of commerce was seen by them in these seas, beyond the possibility of a mistake. They have stated that the animals were large, and very tame, and that they could have been caught in large numbers. Within the last few years whales have been getting very scarce in the Arctic, and in consequence of this two of the most successful of the whaling masters of the present day, Captains David and John Gray, of Peterhead, Scotland, have devoted some labour to collecting all the data relating to this question, and they have consulted such survivors of Ross's expedition as are still available. They have published the results of their investigations in a pamphlet, in which they urge the establishment of the fishery strongly, and they state their conclusions in the following words. They say:—"We think it is established beyond doubt that whales of a species similar to the right or Greenland whale, found in high northern latitudes, exist in great numbers in the Antarctic seas, and that the establishment of a whale fishery within that area would be attended with successful and profitable results." It is not necessary for me to add anything to the opinion of such experts in the business. All I desire to say is that if such a fishery were created, with its head-quarters in Melbourne, it would probably be a material addition to our prosperity, and it would soon increase our population by causing the families of the hardy seamen who would man the fleet to remove from their homes in Shetland and Orkney and the Scotch coasts, and settle here.

In conclusion, I venture to submit that I have been able to point to good and substantial objects, both scientific and commercial, to justify a renewal of Antarctic research, and I feel assured that nothing could bring to us greater distinction in the eyes of the whole civilized world than such an expedition, judiciously planned and skilfully carried out.

QUARTZ FIBRES.[1]

BEFORE I enter upon the subject upon which I have to address you, I wish to point out that, quite apart from any deficiency on my part which will be only too apparent in the course of the evening, it is my intention to commit two faults which may well be considered unpardonable. In the first place, I shall speak entirely about my own experiments, even though I know that the iteration of the first personal pronoun for the space of one hour is apt to be as monotonous to an audience as it is wanting in taste on the part of a lecturer. In the second place, I am going almost to depend upon the motions of a spot of light to illustrate the actions which I shall have to describe, in spite of the fact that it is impossible for an audience to get up any enthusiasm when watching the wandering motion of a spot of light the result of the manipulation of a mystery-box, of which it is impossible to see the inside. These, however, are faults which are the immediate consequence of the nature of my subject.

Physicists deal very largely with the measurement of extremely minute forces, which it is of the utmost importance that they should be able to measure accurately. Now forces may be considered under two aspects. It may be that the force which is developed and which has to be measured is a twist, in which case the twisting force may be applied to the end of a wire directly, when the amount through which that wire is twisted is a measure of the twisting force. Or the force may be a direct pull

[1] Lecture delivered by Prof. C. Vernon Boys, F.R.S., on September 8, 1890, at the Leeds meeting of the British Association.

or a push, which may also be measured by the twist of a wire if it is applied to the end of a lever or arm carried by the wire.

Now supposing that the force—whether of the nature of a twist or of a pull, it does not matter which—is too small to produce an appreciable twist in the wire, it is obvious that a finer wire must be employed, but it is not obvious how much more easily a fine wire is twisted than a coarse one. If the fine wire is one-tenth of the diameter of the coarse one, we must multiply ten by itself four times over in order to find how much more easily twisted it is, and thus obtain the enormous number 10,000; it is 10,000 times more easily twisted than the coarse one. Thus there is an enormous advantage in increasing the minuteness of the wire by means of which feeble twisting or pulling forces are measured. But if the delicacy of the research is such that even the finest wire which can be made is still too stiff, then, even though with such wire, which is somewhere about the thousandth of an inch in diameter, forces as small as the millionth part of the weight of a single grain can be detected with certainty, the wire is of no use; and as wire cannot be made finer, some other material must be used. Spun glass is fine and strong, and is still more easily twisted than the finest wire, but it possesses a property somewhat analogous to putty. When it has been twisted and then let go, it does not come back to its old place, so that though it is much more largely twisted than wire by the application of a force, it is not possible with accuracy to measure that force. There is, or rather I should say there was, no material that could be used as a torsion thread finer than spun glass; and therefore physicists use instead a fibre almost free from torsion. A single thread of silk as spun by the silkworm is taken and split down the middle, for it is really double, and one half only is used. This is far finer than spun glass, and being softer in texture, it is much more easily twisted. Silk is ten thousand times more easily twisted than spun glass. So easily twisted is silk that in the majority of instruments the stiffness of the silk is either of no consequence at all, or at any rate it only produces but the slightest disturbing effect. Now if it is necessary to push the investigation further still by the continued increase in the delicacy of the apparatus, silk itself begins to prevent any progress. Silk has a certain stiffness, but if that were always the same it would not matter; but then it possesses that putty-like character of spun glass, but in a far higher degree; it is affected by every variation of temperature and moisture, and any really delicate measures are out of the question when silk is used as the suspending fibre.

This, I believe, is a fairly accurate account of the state of the case three years ago. At that time I was improving, or attempting to improve, a certain class of apparatus of which I shall have more to say presently, and I was met by the difficulty that a greater degree of delicacy was required than was possible with existing torsion threads. Silk would have entirely prevented me from reaching the degree of delicacy and certainty in this instrument that I hope to show this evening that I have attained.

Being then in this difficulty, I was by good fortune and necessity led to devise a process which I propose at once to show you. I shall not describe the preliminary experiments, but simply describe the process as it stands. There is a small cross-bow held in a vice, and a little arrow made of straw with a needle point, and I have here a fragment of rock crystal which has been melted and drawn into a rod. It requires a temperature greater than that developed in any furnace to melt this material so that it may be drawn out. If the arrow, which also carries a piece of the quartz rod, is placed in the bow, and if both pieces are heated up to the melting-point and joined together, and then the arrow is shot, a fibre of quartz is drawn—that is to say, it is drawn if there is not an accident.

The arrow has flown, and there is now a fibre, not very fine this time, which I shall hand to our President. At the same time I can pass him a piece of much finer fibre, made this afternoon, which shows (and this is a proof of its fineness) all the brilliant colours of the spider line when the sun shines upon it, but with a degree of magnificence and splendour which has never been seen on any natural object.

The main features of these fibres are these. You can make them as fine as you please; you can make them of very considerable length; you can make pieces 40 or 50 feet long, without the slightest trouble, at almost every shot. Even though of that great length, they are very uniform in diameter from end to end, or, at any rate, the variation is small and perfectly regular. The strength of the fibre is, I think I may safely say, something astonishing. Fibres such as I have in use at the present time in an instrument behind me are stronger than ordinary bar steel: they carry from 60 to 80 tons to the square inch. That is one of their most important features, for this reason—that on account of their enormous strength you can make use of very much finer fibres than would be possible if they were not so strong; and I have already explained the importance of the fineness of the fibre when delicacy is of the first importance.

As to the diameter of these fibres, I have said they can be made as fine as you please. I shall not trouble you with a large number of figures, but one or two may probably be interesting to those who are in the habit of using philosophical apparatus. In the first place, a fibre a great deal finer than a single fibre of silk—that is, one five-thousandth of an inch in diameter—will carry an apparatus more than thirty grains in weight. I have in one of the pieces of apparatus which I shall use presently a fibre the fifteen-thousandth of an inch in diameter. That is, so fine that if you were to take a hundred of them and twist them into a bundle you would produce a compound cable of the thickness of a single silkworm's thread. I do not mean the silk used for sewing that is wound on a reel, because that is composed of an enormous number of silk threads; but a single silkworm's thread as it is wound from the cocoon, and that fibre is at the present time carrying a mirror the movements of which will presently be visible in all parts of this large room.

But that is by no means the limit of the degree of fineness which can be reached. A fibre the fifteen-thousandth of an inch in thickness is quite a strong and conspicuous object. You may go on making them until you cannot see them with the naked eye. You may go on following them with the microscope until you cannot see them with the microscope—that is to say, you cannot find their end—they gradually go out. The ends are so fine that it is impossible ever to see them in any microscope that can be constructed, not because the microscopes are bad, but because of the nature of light. But that is a point upon which I shall not say more this evening. It has been estimated that probably the ends of some of these are as fine as the millionth part of an inch—I do not care whether they are or whether they are not, because they can never be seen and never be used—but certainly the hundred-thousandth of an inch is by no means beyond the limit which can be obtained. As these large numbers of hundreds of thousands and millions are figures which it is impossible for anybody thoroughly to realize, I may for the purpose of illustration say that, if we were to take a piece of quartz about as big as a walnut, and if we could draw the whole of that into a thread one hundred-thousandth of an inch in diameter—threads which can certainly be produced—there would be enough to go round the world about six or seven times.

These quartz fibres, on account of their fineness, are eminently capable of measuring minute forces—that is to say, they would be capable if they were free from that putty-like quality which I have described as making spun glass useless. Now, experiments made both in this country and in Australia show that to a most extraordinary degree they are perfectly free from that one fault of spun glass.

The number of useful properties of quartz that has been melted is so great that I can merely take, in a more or less disjointed way, one or two; and I propose, in the first place, to say something which, I think, may be especially interesting to chemists, and, perhaps, to our President. I should like to ask experimental chemists what they would think of a material which could be drawn into tubes, blown into bulbs, joined together in the same way that glass is joined, drawn out, attached to a Sprengel pump, sealed off with a Sprengel vacuum, which would be transparent, which would be less acted upon than glass by corrosive chemicals, and which, finally, at the point at which platinum is as fluid as water, would still retain its form. Here is such a tube with a bulb blown at the end. I have found that it is possible to make tubes (though it cannot be done in the ordinary way, as with glass) and to blow bulbs with quartz, and that they have this advantage which glass does not possess—namely, that it is almost impossible to crack them by the sudden application of heat.

Then there is another property which quartz fibres and rods possess which I shall be able to show only imperfectly—namely, the power of insulating anything charged with electricity under conditions under which in general insulation is impossible. You now see upon the screen an electroscope, the leaves of which were charged at noon, and they are still divergent, but not to a very great extent, because they have suffered from un-

avoidable shaking during the day. The point to which I specially wish to refer is this. In electroscopes and all electrostatic apparatus one puts in a dish of sulphuric acid, which is an abomination, in order to keep the atmosphere dry. I have in this electroscope such a dish, but it is filled with water in order to keep the atmosphere moist. Experiments carefully made, using the same box—everything the same—except that in one case the insulating stem was made of quartz, and in the second case it was made of the best flint glass, well washed, of the same shape and size, show that, if the atmosphere is perfectly dry, the electricity escapes from both at the same rate; but that, if the atmosphere is perfectly moist, the electricity escapes from the leaves insulated by the clean-washed flint glass only too quickly; whereas, from the leaves insulated by the quartz, the rate is identically the same as it was in either case when the atmosphere was perfectly dry.

I have said that these fibres are uniform in diameter, and fine and smooth and strong, and that they glisten with all the colours of the spider web, but that they are far more brilliant. It was naturally rather a curious point to note what a spider would do if by any chance she should find herself on such a web, and now that I am dealing with live and wild animals which cannot possibly be trained the conditions are such as to render the success of an experiment entirely a matter of chance. However, I propose to make use of the spider as a test of the very great smoothness and slipperiness of one of these fibres. There are here three little spiders which have been good enough, since they came to Leeds, to spin upon these little wooden frames their perfect and beautiful geometrical webs. I have succeeded in placing one of these frames in the lantern without disturbing the spider, which you can now see waiting upon her web. I must now, without disturbing the peace of mind of the spider, carry her to a web of quartz; and therefore it is necessary that the spider should be fortunate enough to catch a fly. Now, instead of bringing a fly I will make an ordinary tuning-fork buzz against the web. She immediately pounces upon the imaginary fly, and thus I can without frightening her place her upon the quartz fibre. Unfortunately this spider has slipped and has got away, but with another I am more successful. I intended to show that the small and common garden spider could not climb the quartz fibre, but for some reason this spider is able to get up with difficulty; however I shall not spend any more time upon this experiment.

I shall now at once speak about the instrument which actually led me to the invention of the process for making quartz fibres. This, which I have called a radio-micrometer, is an instrument of very great delicacy for measuring radiant heat from such a thing as a candle, a fire, the sun, or anything else which radiates heat through space.

The radio-micrometer which I wish to show this evening is resting upon a solid and steady beam, and as usual its index is a spot of light upon the scale. You see that that spot of light is almost perfectly steady. Now the heat that I propose to measure, or rather the influence of which I intend to show you, is the heat which is being radiated from a candle fixed in the front of the upper gallery some 70 or 80 feet from the instrument; and in order that you may be sure that the indication of the instrument is due to the heat from the candle, and not to any manipulation of the apparatus on the beam, I shall perform the experiment as follows. None of the apparatus at this end of the room will be touched or moved in any way; but by a string I shall simply pull the candle along a slide up to a stop, at which position it will shine upon the sensitive part of the radio-micrometer. Instantly the spot of light darts along the scale for a distance of ten feet, and then after leaving the scale it comes to rest upon the face of the balcony five or six seconds after it began to move. Now if the candle is allowed to move back through about a foot, you will see that the instrument will cool down at once—it is at present suffering from the heat which falls upon it from the distant candle; but it will cool down at once, and the index will go back to its old place. It is very nearly at its old place now. I will now let the candle shine upon it again. The index at once goes on to the balcony as before, and now that the candle is moved away again, the index has assumed its old place upon the scale.

That really shows that we have here the means of measuring heat with a degree of delicacy, and also with a degree of certainty, ease, and quickness, which has never yet been equalled. It is probable that the measure which I have given of the degree of delicacy that I have reached in my astronomical apparatus—namely, that the heat of a candle more than two miles away can certainly be felt—will not seem so absurd now that you have seen this less perfect apparatus at work, as it does to people whose experience is limited by the thermopile or their senses.

You can now see the spot of light; it is perfectly quiet in its old place. I wish to show you that this instrument is unlike those which are ordinarily used for this purpose. All the heat, the very considerable heat, due to this electric arc lamp, is actually falling on the instrument, but not upon its sensitive surface, and there is no indication. There are a large number of people in the room—it does not feel the heat from them. Stray heat which it is not meant to feel—which is not in the line along which it can see, or feel—has no influence upon it. When the candle was moved to the place to which it was looking, it felt the heat, and you saw the movement of the index. What is perhaps more important than all is, that it is an instrument which does not even feel the influence of a magnet. I have here a magnet, and on waving the magnet about near the instrument there is no movement of the index at all; it does not dance up and down the scale, as it certainly would do in the case of a galvanometer, because this magnet would affect a galvanometer at the other end of the room. We have then a degree of sensibility which is certainly not easily developed in any other way. I must except, however, the instrument which Prof. Langley of America has recently brought to a great state of perfection. I am unable to state, from want of information, whether his instrument is as sensitive as the one I have just shown, but whether it is or is not as sensitive it certainly cannot compare with this in its freedom from the disturbing effects of stray heat falling upon it, or of the magnetic or thermo-electric disturbances which give so much trouble where the galvanometer is employed.

Now this apparatus I was recently using in some astronomical experiments on the heat of the moon and the stars. As these experiments could only be made with an instrument such as this, possessing extreme sensibility and freedom from extraneous disturbances, and as this instrument is both the cause of the discovery and the first result of the application of quartz fibres, I have thought it well to repeat a typical experiment upon the moon's heat, but, like Peter Quince, I am in this difficulty. As he said, "There is two hard things, that is to bring the moonlight into a chamber." In fact, at the present time the moon has not risen, and if it had we should not be much better off. Peter Quince proposed that they should in case of moonlight failing have a lanthorn and a bunch of thorns. That no doubt was sufficient for the conversation of Pyramus and Thisbe, but that would not do for the purpose of showing the variation of radiation from point to point upon the moon's surface, and as that is the experiment which I now wish to show—an experiment which this instrument enables one to make with the greatest ease and certainty—it is necessary to have something better than a lanthorn and a bunch of thorns. Therefore I have been obliged, as the moon is not available, to bring a moon. Now this moon is a real moon; it is not a representation; it is not a slide; it is a real moon, and it is made by taking an egg-shell and painting it white. That egg-shell is now placed upon a stand, and is illuminated by the sun—that is, an electric light—and in order that the moon may be visible the room must be darkened. The moon is now shining in the sky. An image of the moon is cast by means of a concave mirror upon a translucent screen. There is in addition another mirror which throws a small image of the same moon upon the radio-micrometer. There is one more thing to explain. There is upon the screen a black spot which represents the sensitive surface of the radio-micrometer. That bears the same proportion to the moon which you see on the screen as the sensitive surface of the radio-micrometer bears to the image of the moon that is cast upon it. Now the two mirrors are arranged to move by clockwork, so as to make the two images travel at proportional rates. The moon is travelling with the dark edge foremost, and now that the terminator of the moon has come upon the sensitive surface, the heat is felt and the deflection of the instrument is the result. Now as the moon is gradually travelling through the sky, the radiation is slowly and steadily increasing, because the radiation from the moon gets greater and greater, as the point at which the sun is shining vertically—that is, a point at right angles with the terminator—is approached; it is here a maximum, and then it falls back, and as soon as the moon has gone off the instrument, you will see the index fall back almost suddenly. But there is something more. This moon in one respect is better than the other moon. At the present time it represents the moon nineteen days old, a moon, that is to say, which is waning, and which goes through the sky

with its dark edge foremost. The clockwork will now bring the moon back again, and convert the nineteen-day moon into a nine-day moon, one in which the bright edge goes forward. What I want you to notice, and it will be perfectly evident, is this, that the spot of light will now go up the scale suddenly, will then rise to a maximum position, and will then fall slowly until the terminator is reached, which proves that in the former case the slow rise and sudden fall, or the present sudden rise and slow fall was not a peculiarity of the instrument, but was due to the fact that the different points of the moon radiated in the manner which I have stated. There is one point which, as the moon has now left the instrument, I should like to show; that is, that it is a real moon and not a mere slide. That is shown by gradually moving the sun round. Now it is at right angles to the line of view, and we have got the half moon. As it goes round, the moon continues waning, appearing more like a new moon, and at last we have an eclipse of the sun, which may be annular if the proportions of the apparatus are properly arranged.

I wish now to make a few statements as to the delicacy of apparatus that can be made with the help of quartz fibres. I would wish you most distinctly to understand that it is not sufficient to go into a shop and buy apparatus as it is now made, replace the silk by quartz, and to suppose you can get a degree of delicacy such as I have shown you. That is not sufficient. If you take out the silk and put in a quartz fibre the apparatus will be much improved, and you can then increase its delicacy. You will then escape the troubles due to silk; but one after the other a new series of disturbances will appear, and anything like ultimate, extreme and minute accuracy will still seem out of the question. Now it has been my business to eliminate one by one these disturbing influences. I will not weary you with a description of them all, and the methods by which they may be certainly provided against. These disturbing causes, which at the present time with instruments carrying a silk fibre are not even known to exist, or if known to exist, are practically of no consequence whatever, come one by one into prominence, when you attempt to push the delicacy of your apparatus to the extent that I have reached in the home-made apparatus which I have here this evening. I do not propose to give more than one illustration, and as this is one which I found out by accident, and which at the time very much annoyed me, I imagine that it may be of interest to explain the circumstances under which this was observed.

In the experiments I made on the heat of the moon and the stars it was necessary to determine to what degree of delicacy the apparatus could be brought—that is to say, to determine what deflection would be produced by a known and familiar source of radiation. For this purpose the source of heat that I used was a common candle, placed sufficiently far off to produce a convenient deflection. I began by placing the candle about 100 yards away, but I was obliged to place the candle at a distance of 250 yards. At that distance I could not conveniently at night turn the shutter on and off with a string. Therefore I adopted the more simple and practical plan of asking my niece to stand at the top of the hill and to pull the string when I gave the signal. The signal was nothing more nor less than my saying the word "on" or "off," so that without moving I could observe the deflection due to the heat of the candle at that distance. Those were the circumstances, but when I shouted "on," before the sound could have reached my niece at the top of the hill, the spot of light had been driven violently off the scale. This seemed as if, as I suspected at the time, one of my little eight-legged friends had got inside the apparatus, and feeling the trembling due to the sound, struck forward, as the diadema spider is known to do, and tried to catch the thing that was flying by. But further experiments showed that this was not the case. It happened that the sound of my voice was just that to which the telescope tube would respond. It echoed to that note, the instrument felt the vibration of the air, and that was the result.

In order to show that an instrument will feel the motion in the air under the influence of sound, I have arranged an experiment of the simplest possible character. I should say that the first instrument of this kind was made many years ago by Lord Rayleigh; but I feel sure that even he would not be prepared for the delicacy to which apparatus on this principle can be brought. It simply depends upon this familiar and well-known fact. A card or a leaf allowed to drop through the air does not fall the way of the least resistance—that is, edgeways—but it turns into the position of greatest resistance, and falls broadside on, or it overshoots the mark, and so gets up a spin.

Supposing you take a little mirror suspended at an angle of 45° to the direction of the waves of sound, the instant sound-waves proceed to travel, that mirror turns so as to get into such a position as to obstruct them. The mirror that I have for this purpose weighs about the twentieth part of a grain, and the fibre on which it is suspended is about the fifteen-thousandth part of an inch in diameter. The mirror is so small and light that the moment of inertia is a two-hundredth part of that which people ordinarily call the minute and delicate needle of the Thomson mirror-galvanometer. With a fibre only a few inches long, there is no difficulty in getting a period of oscillation of ten or eleven seconds. When the light from the lamp is reflected and falls upon the scale, as it will be in a minute, then a movement of the light from one of those great divisions to the next—that is, a movement of three inches—will correspond to a twisting force such as would be produced by pulling the end of a lever an inch long with a force of a thousand-millionth part of the weight of a grain. It would be easy to observe a movement ten or a hundred times less. My difficulty now is that it is impossible to speak and at the same time to keep that spot at rest, because the instrument is arranged to respond to a certain note. This is not the predominating note of my voice, but since the voice, like all other noises as distinguished from pure musical sounds, consists of a great number of notes, every now and then the note to which the instrument is tuned is sure to be sounded, and then it will respond. Therefore, while I am speaking it is impossible to keep the spot of light at rest. However, in order to show that the instrument does respond to certain notes, even if feeble, with a degree of energy and suddenness which I believe would never be expected, I shall with these small organ pipes sound three notes. But I must explain beforehand what I am going to do, as the sound of my voice will spoil the experiment. I shall, standing as far away as I can get from the instrument, first sound a note that is too high; I shall then sound a note that is too low; and then I shall sound the note to which the instrument is tuned. I must ask everyone during this experiment to be as quiet as possible, as the faintest sound of the right sort will interfere with the success of the experiment. The first two notes sounded loudly produced no result, while the moment the right note was heard the light went violently off the scale and travelled round the room. When this little organ pipe was blown at the farthest end of the room this afternoon, it drove the light off the scale, almost as violently as it did just now.

[The Cavendish experiment of observing the attraction due to gravitation between masses of lead was then explained; and the actual experiment, performed with apparatus no larger than a galvanometer, in which the attracting masses were two pounds and fifteen grains respectively, in which the beam was only about five-eighths of an inch long, and in which the total force was less than one ten-millionth of the weight of a grain, was then shown. The actual deflection on the scale was rather more than ten feet, and eighty seconds were required for the single oscillation. With this apparatus forces two thousand times as small could be observed, though the fibre is, in comparison with others that were made use of, exceedingly coarse. Forces equivalent to one million-millionth of the weight of a grain were stated to be within the reach of a manageable quartz fibre.]

Now that I have shown all that my limited time has permitted me, I wish finally to answer a question which is frequently put to me, and which possibly some in the room may have asked themselves. The question may be put broadly in this form: "These fibres no doubt are very fine, and very wonderful, but are they of any practical use?" This is a question which I find it difficult to answer, because I do not clearly know what is meant by "practical use." If by "a thing of practical use" you mean something which is good to eat or to drink, or if you mean something which we may employ to protect ourselves from the extremes of heat or cold or moisture, or if you mean—and this is a point which those who have studied biology will perhaps appreciate more than others—something which may be made use of for the purpose of personal adornment; if that is what you mean by "practical use," then with the exception of the possibility of being able to weave garments of an extraordinary degree of fineness, softness, and transparency, quartz fibres are of no "practical use." But if you mean something which will enable a large and distinguished body of men to do that which is most important to them more perfectly than has been possible hitherto—I allude of course to the experimental philosopher and his experimental work, which after all has laid the foundations upon which so much that is called practical

actually is built—if this is what you mean, then I hope that the few experiments which I have been able to show this evening are sufficient to prove that quartz fibres are of some practical use; and they have served this additional purpose—with what success I am unable to say—they have provided a subject for an evening lecture of the British Association.

SOCIETIES AND ACADEMIES.

London.

Entomological Society, October 1.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The Rev. Dr. Walker exhibited, and read notes on, a long and varied series of *Crymodes exulis*, collected in June and July last in Iceland. In reply to a question by Lord Walsingham as to whether all the forms referred by Dr. Walker to *Crymodes exulis* had been identified as belonging to that species, Mr. Kirby said the species was a very variable one, and that several forms had been described from Labrador and Greenland. Mr. South stated he believed that most of the forms had been described by Dr. Staudinger.—Dr. D. Sharp, F.R.S., exhibited a specimen of *Ornithomyia avicularia*, L., taken near Dartford, to which there were firmly adhering—apparently by their mandibles—several specimens of a mallophagous insect. He also exhibited specimens of fragile Diptera, Neuroptera, and Lepidoptera, to show that the terminal segments in both sexes might be dissected off and mounted separately without the structures suffering from shrivelling or distortion.—Mr. G. F. Hampson exhibited a series of *Erebia melas*, taken in July last, in the Austrian Alps (Dolomites), by Mrs. Nicholls. Captain Elwes observed that this species was abundant in the Pyrenees, but he had never been able to obtain specimens from any other part of Europe; and that it had been left to an English lady to first take a species of *Erebia* new to these Alps. He added that the species only frequented very steep and stony slopes on the mountains, so that its capture was attended with difficulty.—Mr. McLachlan, F.R.S., exhibited specimens of an extraordinary Neuropterous larva found by Mr. B. G. Nevinson in tombs at Cairo. He said that this larva had been assigned to the genus *Nemoptera* by Schaum, and Roux had previously described and figured it as an abnormal apterous hexapod under the name of *Necrophilus arenarius*. Mr. Nevinson supplemented these remarks with an account of his capture of the specimens in the Egyptian tombs.—Mr. G. T. Baker exhibited species of the genus *Boarmia* from Madeira; and also melanic varieties of *Gracilaria syringella* from the neighbourhood of Birmingham.—Mr. W. F. H. Blandford exhibited and remarked on specimens of *Dermestes vulpinus*, a wood-boring beetle, which had been doing much damage to the roofs of certain soap-works in the neighbourhood of London.—Mr. R. W. Lloyd exhibited a specimen of *Carabus catenulatus*, in which the femur of the right foreleg was curiously dilated and toothed.—The Rev. C. F. Thornewill exhibited a black variety of the male of *Argynnis aglaia*, taken by himself in July last on Cannock Chase; also a number of living larvæ of a species of *Eupithecia* feeding on the flower-heads of *Tanacetum vulgare*. He expressed some doubt as to the identity of the species, but the general opinion was that the larvæ were those of *Eupithecia absynthiata*.—Mr. H. Goss exhibited, for Mr. G. Bryant, a variety of the larva of *Trichiura cratægi*.—Mr. C. G. Barrett exhibited a specimen of *Plusia moneta*, Fabr., a species new to Britain, taken at Reading in July last. Mr. Goss stated that the first specimen of this species had been taken at Dover last June, and was now in the collection of Mr. Sydney Webb, of that town. Mr. Kirby said that Mynheer Snellen had reported this species as being unusually common in Holland a few years ago.—Mr. W. Dannatt exhibited a variety of *Papilio hectorides* from Paraguay. Mr. O. Salvin, F.R.S., said he had seen this form before.—Mr. C. J. Gahan exhibited a curious little larva-like creature, found in a mountain stream in Ceylon, and observed that there was some doubt as to its true position in the animal kingdom. It was made up of six distinct segments, each of which bore a single pair of laterally directed processes or unjointed appendages. Mr. Hampson remarked that the appendages were very suggestive of the parapodia of certain chætopod worms. Lord Walsingham and Mr. McLachlan expressed an opinion that the animal was of myriopodous affinities, and was not the larva of an insect.—Mr. Baker read a paper entitled "Notes on the genitalia of a gynandromorphous *Eronia hippia*."

Paris.

Academy of Sciences, October 6.—M. Duchartre in the chair.—On the determination of integrals of certain equations from partial derivatives of the second order, by M. Émile Picard.—On the balls of fire or electric globes of the St. Claude tornado, according to the report of M. Cadenat, by M. H. Faye. Prof. Cadenat, of the St. Claude College, has brought forward a number of testimonies as to the appearance of many balls of fire during the storm of August 19. It is a remarkable fact that the United States tornadoes are rarely accompanied by globular lightning discharges like those observed during the recent storms of Dreux or St. Claude. The cause may be that American tornadoes have been most frequently observed in broad daylight, whilst in France those of August 18 and 19 appeared towards the evening.—On the movement of Foucault's pendulum, by M. de Sparre. The author establishes the complete formulæ for the movement of Foucault's pendulum in air, and shows that the resistance of the air has an indirect influence on the velocity of rotation of the plane of oscillation, both diminishing the amplitude of the vibrations, and causing deformations in the curve described.—Some theorems on similar plane figures, by M. P. H. Schoute.—On a new method for testing urea, by M. M. P. Miquel.—Destruction of the tubercular virus by the products of the evaporation of certain substances, such as a mixture of alcohol and different essences, on spongy platinum, by M. Onimus.—On the fecundation of *Hydatina senta*, Ehr., by M. Maupas.—Experiments on the cultivation of wheat in a sterile siliceous soil, by M. Pagnoul. The experiments show that phosphates, especially in the soluble form, play an important *rôle* in the production of wheat; in fact, the suppression of phosphoric acid retarded the maturity of plants about ten days. The richness of the grain in nitrogenous matters increases with the proportion of nitrogen at the disposal of the plant. It it is found to decrease to 8 or 9 per cent in plants grown in soil containing no nitrogen, and reaches as much as 20 per cent—that is, much above the average—in those grown in soils in which the assimilated nitrogen was greater than that of the most fertile soils.—Observations of the part played by fluor in mineralogical syntheses, by M. Stanislas Meunier. The author finds that the introduction of fluorides renders the synthesis of labrador, nephelite, and leucite remarkably easy and rapid, and does away with the necessity for very high temperatures.

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THURSDAY, OCTOBER 23, 1890.

BRITISH FARM, FOREST, ORCHARD, AND GARDEN PESTS.

British Farm, Forest, Orchard, and Garden Pests. A Manual of Injurious Insects, with Methods of Prevention and Remedy for their Attacks to Food Crops, Forest Trees, and Fruit, to which is appended a Short Introduction to Entomology. Compiled by Eleanor E. Ormerod, F.R.Met.Soc., &c. Second Edition. (London: Simpkin, Marshall, Hamilton, Kent, & Co., 1890.)

THE first edition of "The Manual of Injurious Insects" was published in 1881, and was then justly considered by all entomologists to be the most important work upon economic entomology since Kirby and Spence wrote their famous "Introduction to Entomology," "combining," as John Curtis said, "truth, instruction, and amusement." It was undoubtedly also by far the most exhaustive account of insects destructive to agricultural and horticultural crops that had been produced since the appearance of the admirable "Farm Insects" of Curtis in 1860. The second edition of this useful "Manual of Injurious Insects" has been recently issued, and contains in addition to the vast stores of information concerning all manner of insects which attack farm and garden crops, the results of the devoted labours, keen research, and scientific observation of Miss Ormerod, during a period of nine years.

In point of volume and matter this last edition is nearly twice as large as the first. As regards interest, practical value, and science, it is likewise of much more importance, because it records the discovery of insects altogether new and undescribed in this country, as well as measures of prevention and remedial methods against these and many other insects, that have been prescribed and adopted within the past decade. It describes, in short, the advance which has been made in economic entomology in this period, in the knowledge of insects, of their life histories, and habits, and of means to protect the crops of cultivators against their ravages. And no one is better qualified to relate this progress than Miss Ormerod, who has herself contributed so greatly towards it.

Most of this new matter has been previously given for the edification of the public and the advantage of farmers in Miss Ormerod's "Annual Reports of Observations of Injurious Insects and Common Farm Pests." It is condensed in the new manual, and arranged under different headings, or parts. These are three, the same as in the first edition. Part I.—Food crops and insects that injure them. Part II.—Forest trees and insects that injure them. Part III.—Fruit crops and insects that injure them.

An Introduction to Entomology is given in this edition as an Appendix, while in the former it precedes the three parts, or divisions. It may be said of this, in passing, that it will be most useful to students of entomology, as it gives in concise terms the main points by which insects of various orders and species may be distinguished in each stage of their life histories. The classification of insects is plainly set forth so that beginners may see almost at a glance the primary division of insects into the two great tribes, *Mandibulata* and *Haustellata*, and the subdivision of the one into eight orders, and of the other into five orders, in accordance with the rational arrangement of Prof. Westwood.

Among the troublesome insects treated of in Part I. are several species of butterflies, moths, and flies which attack cabbages, as the large and small white cabbage butterfly, *Pieris brassicæ* and *Pieris rapæ*, the cabbage moth *Mamestra brassicæ*, the cabbage fly *anthomyia brassicæ*, and others more or less injurious to the brassica tribe. Complete histories are furnished of all these insects, and valuable means of prevention are advised and remedies suggested of a practical nature that can easily be adopted, both on a large scale suitable for farmers and market gardeners, as well as for gardeners and allotment holders.

There is an important monograph of the carrot-fly, *Psila rosæ*, which will be gratefully received by market gardeners and market garden farmers, as the fly has in the last few years been especially destructive, not only in England, but also in Scotland and Ireland. This attack is generally termed "rust," because the leaves of the carrots become yellowish, or rusty coloured, and the roots are covered with rusty patches. To one unacquainted with entomology and not having good eyesight, it is difficult to trace the cause of the disorder to the tiny maggots of this fly in the roots of the plants. Upon very careful examination, however, a diseased carrot will be found to be swarming with legless, slimy, yellowish maggots, not a quarter of an inch in length, many of which are found to be sticking half in and half out of the roots. Miss Ormerod says:—"The grubs may be found in winter as well as summer, and attack all parts of the carrot-root by gnawing galleries on the surface, or into the substance of the root; but whilst the roots are young, the grub appears generally to attack the lowest part." This is not always the case, for in some young carrots examined in July last, which were sent from Ireland, the crowns of the roots were as full of the maggots as the ends.

Under the head of prevention and remedies for this affection, it is stated—

"The following notes regarding carrot cultivation will be found to bear in various ways suitable to different circumstances of soil and climate on the main points of —1st, such preparation of the ground in autumn, or winter, as will ensure favourable conditions for a healthy, vigorous, and uninterrupted growth from the first sprouting of the seed; 2nd, thinning at such a stage of growth, in such circumstances of damp weather or with such watering or treatment after thinning as may least expose the plants to the attack of the carrot-fly which frequently occurs after this operation. Whether the fly is attracted by the scent of the bruised plants, or what brings it, is not clear, but it is very clear that, as it goes down into the ground to lay its eggs on or by the carrots, all operations which leave the soil unusually loose and open lay at the same time the carrot roots open to attack, and it will be observed that the various methods of treatment in regard to thinning bear upon the means of meeting this difficulty."

This is given as one instance of Miss Ormerod's powers of observation as to the habits of insects, which enable her to recommend suitable and effective remedies and methods against them. The practical conclusion in this

case is that carrot-growers should thin carrot plants early, and draw the soil as close as possible, and make the soil very firm around them immediately after thinning has taken place.

Again, "although the summer broods hatch in three or four weeks the maggots may be found in the roots during the winter, and they change to pupæ in the earth adjacent. It is therefore very desirable that all infested carrot-beds should be thoroughly cleared of roots in the autumn, and the ground well dug, or trenched, so that such maggots or pupæ as remain in the bed may be destroyed; some may escape, but the larger number will thus be buried too deeply to come up again or be thrown on the surface to the birds; and a dressing of gas-lime will be serviceable in destroying such of the maggots as are lying near the surface."

Remedies are prescribed for this attack in the shape of dressings with spirits of tar mixed with sand, and of paraffin oil and sand; also waterings with dilute soluble phenyl and paraffin oil, in the proportion of a pint of paraffin to two gallons of water.

Among the many insects that injure corn crops whose histories appear in this "Manual," is a group of flies, among which are the frit fly (*Oscinis frit*)—a minute fly, not the eighth of an inch long, whose attention seems to be confined to oats. The maggot coming from the egg laid by this "fly feeds in the heart of the young oat-plant a little above the ground-level and eats away the centre, so that the shoot above the eaten part is destroyed, and the damage that is going forward then becomes noticeable from the injured shoots turning brown and withering instead of continuing their growth."

The frit fly has been well known in France, Germany, and particularly in Sweden, where it attacks barley, but until 1888, when the attacks of the frit fly were very prevalent in Devon and Cornwall, not much was known of it in this country, although, as Miss Ormerod points out, "the presence of the *Oscinis vastator*, Curtis, which appears to be the same as *Oscinis frit*, was watched and recorded in 1844 by John Curtis in his 'Farm Insects.' In 1881 I was favoured by Mr. R. H. Meade, of Bradford, with the information that the *Oscinis frit* had been observed in the autumn of that year in swarms in an outbuilding, in the lofts of which a lot of newly-threshed barley had been stored, which points to the Swedish form being then present; but it was not until 1887 that I was able to watch this attack throughout its course, up to the development of this fly as a regular field attack."

Farmers now find another fly, the "gout" fly, or ribbon-footed fly, *Chlorops tæniopus*, to be a frequent enemy to wheat, rye, and barley plants. This, as shown in the "Manual," is most prevalent on barley, and is mentioned by Curtis as having done much harm, in 1841, in Surrey and Lancashire. Now it is found in most parts of the country, and is a striking instance of the general spread of insect pests within the last few years among cultivated crops of all kinds.

The action of this insect is thus described by Miss Ormerod:—"Whilst the plant is still young and the forming ear is wrapped in the sheathing leaves, the fly places her eggs either within these leaves or so that the maggot can make its way through them to the ear; there it usually eats away some parts of the lower portions of the ear, and then gnaws or, rather, tears a channel down one side of the stem to the uppermost knot, and beneath the leaves the maggot changes to a reddish chrysalis, from which the gout fly appears about harvest time."

It has been a moot point where this insect passed the winter in this country. In Germany, as Taschenberg states in his Praktische Insekten Kunde, the flies place their eggs on grasses and autumn sown corn, upon which hibernation takes place either in larval or pupal form. As reported by the Consulting Entomologist of the Royal Agricultural Society of England, pupæ of the chlorops were discovered in the main stems of wheat plants just above the ground, in England, in the early part of the spring of 1890 by Mr. Whitehead. The time when these were found and the evident injury caused to the plants proved that the insect had hibernated within their stems.

Another insect belonging to the group of corn flies is the corn saw fly, *Cephus pygmæus*, a very small insect which pierces the stem of wheat and barley plants "just below, or at one of the knots, and inserts there an egg, continuing this process successively to other stems until her egg supply is exhausted. The maggot, which hatches in about ten days, is about half an inch long, yellowish white, fleshy, with a horny, rusty-coloured head, and is peculiar in being footless, although the larva of a saw fly. It feeds on the inner substances, clearing its way sometimes through the knots, even through the topmost, and when nearly full grown comes down inside the stalk on which it has fed; and about harvest time, or a little before, it comes down to the ground level, where it gnaws a ring so neatly and cleanly round inside the stem that the straw readily falls with its own weight, or from a slight pressure of the wind, the severed stalk showing almost as smooth a fracture as if it had been separated by a knife. When the maggot has thus travelled down the stalk and nearly cut it through (so that nothing may prevent its escape presently as a fly) it goes down into the lowest part and spins itself a silken case in which it passes the winter."

The wheat-bulb fly (*Hylemia coarctata*), though only identified in 1882, has now become one of the pests to be dreaded by wheat growers. Curtis does not speak of it, and it was first distinguished in this country by Miss Ormerod. Taschenberg speaks of this fly as destructive in parts of Germany, and says there are two broods there. As this seems to be a new destroyer here, it is possible that it was brought from Germany with imported straw or produce of some kind.

In the "Manual" it is observed that the attacks of the maggots of the wheat-bulb fly and those of the frit fly are much alike, so far as the method of injury is concerned. But here Miss Ormerod's entomological knowledge and acute perception of the smallest distinctions serve to show how the different flies may be recognized. In the maggot or larva of the wheat-bulb fly "the tail segment projects, and ends in two squarish-ended teeth with flattened edges placed centrally, with one pointed tooth, and sometimes more, on the central square part. . . . The presence of these teeth and the *absence of* a little bunch of stalked spiracles near the head appear to me to be the simplest way of knowing the wheat-bulb from the frit maggots."

The Hessian fly, another member of the group of stem

flies, having first appeared in Great Britain in 1886, is graphically described by Miss Ormerod, who has done so much to familiarize agriculturists with the dreaded scourge, and to make them acquainted with preventive measures and remedies against it. This information, published from time to time, is concisely summarized, so that it may be said that, in the few pages devoted to this insect, all that is known about it is plainly set forth.

The least generally known facts connected with the Hessian fly, and those of the most scientific interest, relate to its parasites, which have been carefully studied by the authoress, who had the advantage of long consultations with Prof. Riley in 1887.

The importance of the various parasites of the Hessian fly in tending to keep it down in this country is great. By some it is believed to be desirable to rear them and take them to places that are badly infested, just as, recently, parasites were imported from Australia to destroy the *Icerya purchasi* in the Californian orange groves. It is certain that in this last summer the attack of the Hessian fly was immensely modified by the parasites, which were present in unusual numbers. In several instances where the pupæ of the Hessian fly were transferred to live cages, at least 70 per cent. proved to be parasitized by at least three different kinds of flies. Miss Ormerod and Prof. Riley agree that the parasites of the Hessian fly in Great Britain are of the same species as those found in Russia, and differ from those which infest the Hessian fly in America. Comparative lists of the American and Russian Pteromali are submitted, from which it is seen that they are of the same genus, but not of identical species. "The examination of our parasites," Miss Ormerod concludes, "pointed, therefore, very strongly to the probability of our Hessian fly attack having been imported to us from the east of Europe." And, further, it is suggested that it originated, not in straw imports, as it was first imagined, but in the pupa, or "flax-seed," condition in foul grain imports.

In Part II., devoted to the insects that injure forest-trees, among the principal offenders is shown to be the elm-bark beetle, *Scolytus destructor*, which makes the well known galleries between the bark and the wood, "mainly in the soft inner bark, but so as to leave a slight trace of the working on the surface of the tree." This beetle often causes serious injury to elms both in this country and on the Continent. It generally attacks trees, or the parts of trees that are inclined to disorder, or decay, or that have been previously attacked by beetles. To circumvent the operations of this insect, Miss Ormerod recommends that the rough bark should be scraped off, so that the larvæ are exposed to air, or driven out by the flow of sap from the inner lining of the bark. This was found to answer in France, where upwards of 2,000 trees were thus treated.

The ash-bark beetle, *Hylesinus fraxini*, injures ash trees in the same manner by making galleries beneath the bark, particularly in young trees. It is advised that the bark should be treated with a good coat of soft soap well rubbed into the affected parts of the trees.

Yet another boring beetle is given, known as *Hylurgus piniperda*, or pine beetle, injurious more on account of the harm the beetles cause by boring through the side of the tender shoots of young pine trees and eating their way for an inch or more along the pith, than from the galleries made by the larvæ in pine timber. As they often select dead or diseased trees for boring into for breeding purposes, felled trees should be at once removed and diseased branches or limbs of trees in infected woods should be cut off and carried away. Or traps may be set for the beetle by placing "young Scots pine tops, thinning off all the branches (which makes them convenient to handle) in the plantations or against the lower part of the standing trees." The beetles select these for laying their eggs upon, and they should be taken away and burned in June.

Another pine beetle, *Hylobius abietis*, is even more injurious to many of the coniferæ than the *Hylurgus*. It may be entrapped in the same manner, as it frequents forest clearings, that is, where fir trees, few or many, have recently been felled, and lays its eggs also on logs and stumps.

Against the attacks of many other insects troublesome to trees, such as the pine-bud moth, the pine-shoot moth, the pine saw fly, the spruce gall aphis, the larch aphis, the willow beetle, and the oak-tree roller moth, methods of prevention and remedies are prescribed. This part of the "Manual" cannot fail to be most instructive and useful to those in charge of woods and forests.

In Part III., treating of fruit-crops and insects that injure them, twenty-three different insects are fully described, and in all cases practical suggestions are made for preventing their onslaughts upon the fruit crops, and for diminishing the virulence of their attacks. These suggestions are most timely, as during the last few years the fruit crops of almost all descriptions have suffered much from insects. Not only have new kinds of insects arisen, but long-known foes have increased and multiplied to a terribly dangerous extent, so that whole districts have been cleared of fruit. For example, in the spring of each of the last three years hosts of caterpillars of several species have ruined the apple, plum, and damson crops in many parts of Kent, and in other fruit-producing counties.

Among the fruit pests that have recently sprung up are the white woolly scale, *Pulvinaria ribesii*, found last year upon currant bushes to a considerable extent. A figure of a currant twig covered with white cottony, or woolly matter, forming a covering for the eggs and young scales is appended, which conveys a good idea of the "almost overwhelming nature of the infestation and the serious amount of injury caused by it." This attack has been known in France for some time, and is mentioned by Signoret in his "Essai sur les Cochenilles." Miss Ormerod recommends applying limewash to the infested bushes with a brush, "the same process as whitewashing." Where remedies cannot be brought to bear, or fail, "it would be best to cut off and burn the infested branches, or to destroy and burn the infested bushes if it could be done without serious loss, and thus stamp out this newly-observed pest in time."

A fruit-tree boring beetle new in this country, but well known in Germany and America, from whence it was probably imported, was identified by Miss Ormerod in 1889 as the "Shot-borer," *Xyleborus dispar*. This was found in Lord Sudeley's fruit plantations in Gloucestershire, in the stem of a young plum tree into which it had

bored and killed the tree. Several trees were killed in the same manner. The great peculiarity in these insects is the disparity in size between the females and males, from which it is termed *dispar*. The female is about the eighth of an inch long, while the male is only about two-thirds of this length. The injury begins by a small hole like a shot-hole being bored in the side of the stem, from which a tunnel is made into the pith, and a branch tunnel running horizontally about half, or two-thirds, round the stem. Other tunnels are made straight up and down. These borings, and the destruction of the pith, soon serve to kill the branch. The only remedy appears to be to cut off and burn the infested limb, and "coating the trees with some wash or mixture, which will not hurt the bark but will prevent the beetle getting in or getting out. One application advised for trial is a thick coat of whitewash with some Paris green in it."

There is a detailed account of the winter moth, that arch enemy of apple, pear, and plum growers; this is particularly valuable, as it gives the latest experience of practical growers with respect to preventive and remedial measures. The most important of these is the careful banding of the trees in the autumn, before October, with grease and offensive compounds, to prevent the females from climbing up, and the use of arsenites (Paris green and London purple) for washing or syringing infested trees. These washes have been proved to be efficacious in America, where they are universally applied for many insect attacks. In this country, however, cultivators have hesitated to use them on account of their poisonous nature. Miss Ormerod plainly shows that they may be employed without danger and with vast benefit to the fruit grower. For plum trees, the proportion is 1 ounce of Paris green to 10 gallons of water, and for apple trees 1 ounce to 20 gallons. Testimony is given from various growers as to the efficacy of this wash, which from henceforth will, it is presumed, be adopted, as it seems to be the only one which will check the ravages of moths injurious to fruit trees. Full details concerning the use of these American remedies for insect attack are given, which must be most serviceable.

Want of space prevents allusion to many other insects described in this part of the work. It can only be said that they are clearly and minutely defined, and all that is known of their habits and of means to avert or to modify their mischief is set forth.

The "Manual" is replete with capital figures of the insects in all stages. Many of these are from drawings executed by Miss Ormerod, and many are the well-known accurate and inimitable designs of Curtis.

TORNADOES.

The Tornado. By H. A. Hazen, Assistant Professor of the United States Signal Office. (New York: Hodges, 1890.)

THIS is a book that will hardly enhance the reputation of its author. Despite his assurance (which of course will not be questioned) that he has endeavoured throughout to be absolutely unprejudiced, its apparent aim is not so much to set before the reader a concise description of tornado phenomena as to controvert the views put forward by Ferrel and others relative to their mechanical and physical constitution, and to substitute for these certain other speculations (we can scarcely call them a theory) which appear to the author to have the merit of greater probability. Prof. Hazen does not, indeed, restrict his condemnation to Ferrel's theory of tornadoes and thunder-storms. As a root-and-branch reformer, he finds himself in opposition to the majority of those who, during the last quarter of a century, have built up the fabric of modern meteorology, for, while he speaks with deference of "the epoch-making experiments of Mayer [*sic*] and Joule," he appears to regard as inapplicable to the movements of the atmosphere those laws of thermodynamics which are based on the results of Joule's labours. Were it the practice of scientific authors, in imitation of romance-writers, to head their chapters with quotations appropriate to the subject-matter, chapter v. of this treatise, more especially, might be fitly introduced with the well-known lines from "Faust":—

> "Ich bin der Geist, der stets verneint!
> Und das mit Recht! denn alles was entsteht
> Ist werth, dass es zu Grunde geht";

substituting, however, "*entstanden*" for the present tense of the verb.

Lest it should be thought that these remarks misrepresent or exaggerate the sweeping character of Prof. Hazen's "objections," we extract one or two passages from the chapter in question. On the generally-accepted view that work is performed by an ascending current of air, in pushing aside the atmosphere into which it expands, and that in saturated air the requisite energy is furnished, in part at least, by the condensation of vapour, he observes (p. 52),

"There is nothing in the science of meteorology, or possibly in any physical science, that has been developed from such a worthless origin as this theory of the liberation of energy on the condensation of moisture";

again (p. 54),

"All the reasoning regarding the diminution of temperature in dry and moist air, as we ascend in the atmosphere, is founded upon purely theoretical considerations. Every experiment, whether in the laboratory or in Nature, has proved that these theories, in their sum and substance, are false";

and again (p. 56),

"I am inclined to think that even Espy, with all his disadvantages, was too well informed to adopt such a doubtful and visionary idea as this of effective work performed in the free upper air."

The familiar lecture experiment illustrative of dynamic cooling, in which a cloud is produced in a receiver containing moist air by partially exhausting it with a few strokes of the air-pump, is interpreted in a novel manner consistently with the above opinions (p. 67):—

"The presence of haze or cloud is no evidence of saturated air, for such cloud has been produced in air having only 2 per cent. of moisture.[1] When air is pumped from the room, it has an enormous number of dust particles in it, and these give the appearance of a fog on sudden expansion."

After these samples of the author's opinions it will be scarcely necessary to notice, in detail, the other numerous

[1] Prof. Hazen does not give his authority for this statement, nor does he specify whether the expression is to be understood as 2 per cent. of saturation, or 2 per cent. by weight or volume. If the former, authentication seems desirable; if the latter, the fact is obviously irrelevant.

points on which Prof. Hazen's views are in dissonance with those of most other writers who have treated of tornadoes. Among others, he assures us (p. 57), that "the evidence for [their] gyrations is exceedingly contradictory, and the weight of evidence is overwhelmingly against them"; that (p. 52) it is impossible that warm south wind underruns that which is cooler from the north, "for the denser must always be beneath the lighter"; and (p. 59) that it seems impossible to ascribe the progressive movement of the tornado to the drift of the upper current, "because it moves with a velocity double that of the general storm." Those who are curious to see the further arguments by which these theses are supported must be referred to the work itself. We have yet to notice briefly the alternative views advocated by the author.

The late Dr. Percy used to relate that, in his early days, when the iron would not "come to nature" in the Staffordshire puddling furnaces, the workmen were accustomed to ascribe its perversity to the presence of sulphur in the charge. In still more remote times, the potentate in whose realm that element is supposed to be somewhat abundant, or his agents, would assuredly have been held responsible for what was amiss. But thirty years ago the march of science had brought in other ideas, and the approved explanation of any otherwise unaccountable difficulty of the kind was that electricity had something to do with it. Even at the present day this mysterious agency is the favourite resource of puzzled tyros in physical reasoning, but we should hardly have expected to find it seriously put forward in all its familiar vagueness by an author whose official designation is that quoted above from the title-page of his work. That such, however, is the case stands in evidence in the following extracts, which we give as fully as our space will admit of, lest we should fail to do their author justice:—

"It is very difficult to believe that electricity has nothing to do with our thunder-storms, and is merely a result and never a cause. . . . Our thunder-storms seem to show an enormous storehouse of electricity at five thousand or six thousand feet above the earth; at least electricity seems to be concentrated there over thousands of square miles during thunder-storm action. We are taught that electricity forms a sort of dual condition, or the electric field is a double one. May not this electric field draw on the sun for its energy? . . . Why may not the sun's electricity, oftentimes observed by its direct effect on our magnetic instruments, and, more often still, indirectly in our auroras, be intercepted by a peculiar condition of the atmosphere or of the earth below, and thus be concentrated in particular localities?"

This may, perhaps, appear somewhat vague as an alternative theory of storm generation; in one particular, however, viz. the accumulation in the storm-cloud of the enormous quantities of water precipitated in cloud-bursts, the *modus operandi* is more fully explained. In Prof. Hazen's opinion, it would seem that electricity performs a part in the atmosphere somewhat analogous to that of Clerk Maxwell's hypothetical demons, and which is described as follows:—

"Is it inconceivable that we have to deal here with a negative electric field, which draws to itself with great velocity particles of moisture from regions perhaps for one hundred miles about, when suddenly, upon a discharge of electricity, the potential upon the particles is diminished, and they unite in great abundance and form rain-drops?"

This remarkable speculation, it is considered, receives support from a novel experiment described as follows:—

"A Holz machine was run for fifteen minutes in a rather large room; and most careful measurements of the amount of moisture at the machine and at a point twenty feet away, before and after the machine was in action, showed an increase at the machine. When we consider that it was impossible to measure the moisture contents just at the plate of the machine, and also what an extremely slight charge could by any possibility enter the air from the machine, we can but be surprised that any effect at all was observed."

Without imitating King Charles the Second's scepticism in the matter of the fish's weight in water and out of water, but accepting Prof. Hazen's statement of the results as he gives them, we may still inquire whether the operators who worked the Holz machine continuously for fifteen minutes did not exhale a considerable quantity of water vapour in the neighbourhood of the machine. Perhaps they even perspired freely with their exertion. In any case the foundation seems hardly adequate for the superstructure.

Had this book appeared under a less known name, and were it not for the official position of the writer, we should scarcely have deemed it desirable to notice it at such length. A really searching, intelligent criticism of Ferrel's theory, by one who has exceptional advantages for ascertaining the facts of observation, would have been welcome; for, symmetrical as that theory is, it is still mainly deductive, and there are many points in it, and these not the least important, which still lack confirmation. But we cannot attach much weight to the objections raised by Prof. Hazen. They seem to betray a strange misconception of the physical processes which he condemns in such uncompromising terms; and where his arguments turn on the facts of observation, we must decline to accept his sweeping denials, in the face of the positive testimony of numerous, not incompetent, observers. In some cases, indeed, we might adduce our own personal experience of phenomena which are declared by him to be improbable or impossible. Such are, for instance, the superposition of dry northerly above warm and moist southerly currents, and the spiral movement of the air in dust whirls, which, on a miniature scale, represent that of the tornado.

Again, Prof. Hazen's argument that the rise of pressure beneath a thunder-storm is sometimes observed in storms that are rainless, and therefore cannot be due to the cooling of the air by the rain, or to its downward pressure as it falls, is rendered of little weight by the fact that this occurs only when the lowest strata of the atmosphere are very dry. In the recently published "Climates and Weather of India," it is stated that "a complete transition may be traced between [the rainless dust-storms of Upper India] and the north-westers of Bengal, which are accompanied by heavy rain." In the latter province "the dust-storm is. as a general rule, only the first stage of a north-wester." It is attended with a sudden rise of pressure, and "is followed by heavy rain and sometimes hail," and though the dust-storms of the former are occasionally, though perhaps rarely, quite rainless,

"the coolness of the wind and that of the atmosphere after the storm is over is hardly to be accounted for otherwise than by supposing that rain is always formed in the cloud overhead, but is re-evaporated before it reaches the earth."

There is nothing inconsistent in the existence of an excessive pressure at the ground surface beneath a thunder-storm and a diminished pressure in the vortex of the storm-cloud, but in ordinary thunder and hail storms this latter is restricted to the cloud-region. As the barograph traces of these storms show, the oscillations of pressure beneath them are very great, and there may and indeed must be still greater differences between the tornado vortex and the neighbouring region of precipitation. Indeed, the great velocity of the air-movement implies as much.

A part of Ferrel's theory which especially stands in need of confirmation is the assumption that, immediately prior to the formation of the vortex, the vertical distribution of temperature is such as to bring the atmosphere into a state of unstable equilibrium, and that a slight casual local disturbance of this equilibrium starts the vortical uprush. This is also his explanation of cyclone generation, and indeed it is that hitherto held by the majority of writers on the subject. On the other hand, it is generally considered that anticyclones are determined by the greater local density of the atmosphere, due to a low mean temperature of the air-column. The last of these assumptions, even in the case of winter anticyclones accompanied by very low temperatures at the ground surface, has now been conclusively disproved by Prof. Hann, of Vienna; and he has also shown very strong reasons for believing that the temperature conditions of extra-tropical cyclones are also incompatible with the prevailing view. It does not, of course, follow that those of tornadoes and hail-storms are equally so, but at least the assumed conditions require verification. This may, perhaps, be some day effected by our mountain observatories.

H. F. B.

OUR BOOK SHELF.

Inorganic Chemistry: the Chemistry of the Non-Metals. By J. Oakley Beuttler, M.A. (London: Relfe Brothers.)

NOW that there can be obtained a considerable variety of really good text-books of elementary chemistry suitable for all the usual needs of the present day, one is entitled to look for special features in any new manual. We fail to find any reason for the existence of the volume before us: wherein it differs from others that enjoy general recognition, it is incomplete and erroneous. It has neither index nor contents table, but this is quite a trivial matter when compared with the imperfections of the body of the work. On pp. 19 and 20 there are nine attempts at equations, none of which are correct, while many represent impossible or at least unknown reactions; and in the following paragraphs, on graphic notation, bonds, and radicles, there is a collection of statements that read like the imperfect recollections of a student who never understood the subject. A single atom of oxygen is shown with curiously shaped projections as an example of an element with an even number of bonds existing as a single "atom-molecule." It is stated emphatically that "the element having the greatest number of bonds is always printed in thick type," but we search in vain for thick type in any formula in the book. The statements that are intended to convey the facts of chemistry are vague, often misleading, and very rarely of a practical character. For an illustration of the style there is no need to go further than the chapter that treats of the first element, hydrogen. It states that "on throwing a piece of sodium into water the sodium combines with part of the hydrogen of the water to form caustic soda, liberating the other part of the hydrogen." The volume closes, as one would expect, with the questions set by various examining bodies during the last three or four years.

Anatomy, Descriptive and Surgical. By Henry Gray, F.R.S. Twelfth Edition. Edited by T. Pickering Pick. (London: Longmans, Green, and Co., 1890).

OF a solid text-book so well known as the present work it is hardly necessary to say more than that a new edition has appeared. The book has been carefully revised, and the editor has added considerably to its value by introducing sections on topographical anatomy, and amplifying those on surgical anatomy. Both of these classes of sections have been printed in smaller type, so that they may be disregarded by students who wish to confine their attention exclusively to the descriptive part of the subject. There are many new illustrations, some of which are original.

The Story of the Heavens. By Sir Robert Stawell Ball, LL.D. Fifteenth Thousand. (London: Cassell and Co., 1890.)

IT is, for many reasons, satisfactory that there should be a popular demand for a clear, brightly-written work on astronomy. Sir Robert Ball, however, ought hardly to be content with the issue of mere reprints of his book. It may be somewhat misleading to send forth in its original form, in 1890, an astronomical work first published in 1886.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Passage of Electricity through Gases.

IN my letter in NATURE of July 24 (p. 295) I objected to Prof. Schuster's statement that the fact that free atoms must turn a gas into a conductor of electricity was fatal to the theory of the electric discharge given by me in the *Philosophical Magazine* in 1883, and I maintained that the presence of free atoms in a gas free from electric strain was in no way essential to the theory given in that paper. I see no reason, after reading Prof. Schuster's letter in this week's NATURE, to change that opinion. Prof. Schuster bases his statement, not on my description of the theory itself, but on the explanation by it of the weakening of the electric strength produced by a diminution in the density of the gas. A reference to this explanation will show, however, that it really rests solely on the well-known fact that dissociation is assisted by diminution of pressure, and that the passage which Prof. Schuster quotes is merely an explanation of this property of dissociation from the point of view of the kinetic theory of gases; if this explanation is held to be inconsistent with the absence of free atoms from gas in a normal state, then any alteration in the explanation which might be made to meet this difficulty, though of primary importance in the kinetic theory of gases, is only of secondary importance for the theory of the electric discharge given in my paper, which I still maintain is not all bound up with the existence or non-existence of free atoms in gases not in the electric field. J. J. THOMSON.

Cambridge, October 18.

Changing the Apparent Direction of Rotation.

IN NATURE of October 16 (p. 585), a curious optical effect is incidentally mentioned. Standing near a windmill, and nearly

in the plane of the sails, "it is possible, by a slight mental effort, to change the apparent direction of rotation, and back again."

A similar effect I have often observed, but it seems in no way dependent on the will. Look, for say 30 seconds, steadily at the revolving disks of an anemometer; they will soon reverse their apparent direction, whether you wish it or not. Continue still to gaze, and that reversed direction will be changed back.

All whom I have asked to try this experiment felt the effect to be involuntary. The changes take place not gradually or confusedly, but distinctly and with decision. The fact is plain; the explanation not so simple. HERCULES MACDONNELL.

4 Roby Place, Kingstown.

Earthquake Tremors.

PERMIT me to say that Mr. John Perry, in his criticism (October 2, p. 545) of my "Method of observing the Phenomena of Earthquakes," has assumed that the phenomena observed were due to vertical displacement; whereas they were probably due to a swaying of the building in which the observations were made.

This assumption seems also to have been made in the case of the man mentioned by Mr. Wire in your last issue (p. 593).

Marine Villa, Shanklin, I.W., H. G. DIXON.
October 18.

A Ball of Fire.

AT about 12.5 last night I was going through the street at Milverton, and saw a bright light about south of me. I saw also a bright ball of fire appear through a break in the clouds proceeding with great rapidity, at about the height of 45°, in a direction which I estimate to be from south to north-north-east; it disappeared behind a church, and I saw nothing more. I am told this may be of interest, and therefore forward the account to you. CHARLES RANDOLPH.

Milverton, Somerset, October 17.

HYDRAZOIC ACID—A NEW GAS.

A NEW gaseous compound of nitrogen and hydrogen has been obtained by Dr. Theodore Curtius, the discoverer of amidogen, and its nature and properties were described by him in the Chemical Section during the recent scientific meetings at Bremen. The composition of the gas is HN_3, and its constitution $\mathrm{H{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$. It is, in fact, the hydrogen compound corresponding to the well-known diazobenzene imide of Griess, $\mathrm{C_6H_5N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$, the three nitrogen atoms being united in the form of a closed chain. The gas dissolves in water with great avidity, forming a solution which possesses strongly acid properties, and dissolves many metals, such as zinc, copper, and iron, with evolution of hydrogen gas and formation of nitrides, the metal taking the place of the liberated hydrogen. The derivation name of the gas, azoimide, is somewhat unfortunate in view of its strongly acid nature, and Prof. Curtius proposes the name "Stickstoffwasserstoffsäure." Perhaps the nearest English equivalent, open to the least objection, is hydrazoic acid—a name which will serve to recall the many analogies which this acid bears to hydrochloric and the other halogen acids.

In studying the reactions of his recently-discovered hydrazine (amidogen) hydrate, $N_2H_4 \cdot H_2O$, Dr. Curtius found that benzoylglycollic acid, $C_6H_5CO{-}O{-}CH_2COOH$, was decomposed by two molecules of hydrazine hydrate, with elimination of water and formation of benzoylhydrazine, $C_6H_5CO{-}NH{-}NH_2$, and hydrazine acetic acid, $NH_2{-}NH{-}CH_2COOH$, in accordance with the equation—

$$C_6H_5CO{-}O{-}CH_2COOH + 2N_2H_4 =$$
$$C_6H_5CO{-}NH{-}NH_2 + NH_2{-}NH{-}CH_2COOH + H_2O.$$

Under the influence of nitrous acid benzoylhydrazine forms a nitroso compound, $\mathrm{C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{NO}\\ \mathrm{NH_2}\end{matrix}\right.$, which spontaneously changes into benzoyl-azo-imide, $\mathrm{C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$, with elimination of water.

$$\mathrm{C_6H_5CO{-}NH{-}NH_2 + NO{-}OH = C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{NO}\\ \mathrm{NH_2}\end{matrix}\right. + \mathrm{H_2O}.$$

$$\mathrm{C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{NO}\\ \mathrm{NH_2}\end{matrix}\right. = \mathrm{C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right. + \mathrm{H_2O}.$$

Benzoyl-azo-imide decomposes, upon boiling with alkalies, with formation of benzoate of the alkali and the alkaline salt of the new acid.

$$\mathrm{C_6H_5CO{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right. + \mathrm{2NaOH}$$
$$= \mathrm{C_6H_5COONa + Na{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right. + \mathrm{H_2O}.$$

When this sodium nitride is warmed with sulphuric acid, hydrazoic acid, $\mathrm{H{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$, is liberated as a gas. The gas is decomposed by hot concentrated oil of vitriol; hence diluted acid requires to be employed, and the gas can thus only be collected in a moist state. HN_3 possesses a fearfully penetrating odour, producing violent catarrh, and dissolves in water with an avidity reminding one of hydrochloric acid. The solution also bears a surprising resemblance to aqueous hydrochloric acid; for, on distillation a concentrated acid first passes over, and afterwards a more dilute acid of constant composition. The aqueous solution possesses the odour of the free gas, and is strongly acid to litmus. With ammonia gas, hydrazoic acid gas forms dense white fumes of the ammonium salt, N_4H_4 or $\mathrm{NH_4{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$, a compound which is completely volatile below 100°, and which crystallizes, but not in crystals belonging to the cubic system, in this respect indicating its different constitution to ammonium chloride. The aqueous solution rapidly evolves hydrogen in contact with zinc, copper, iron, and many other metals, even when largely diluted. As in the case of hydrochloric acid, the silver and mercurous salts are insoluble in water, the others being generally readily soluble. As the acid possesses feebly reducing properties, solutions of many of its metallic salts, the copper salt for instance, yield precipitates upon boiling of compounds of the lower oxides of the metals. The barium salt, BaN_6, crystallizes from solution in large brilliant anhydrous crystals. With silver nitrate the aqueous solution of the acid or a soluble salt yields a precipitate closely resembling silver chloride in appearance. Silver nitride, $\mathrm{Ag{-}N}\left\langle\begin{matrix}\mathrm{N}\\ \|\\ \mathrm{N}\end{matrix}\right.$, does not, however, darken when exposed to light, and is further distinguished from silver chloride by its fearfully explosive properties. During the course of his description at Bremen, Prof. Curtius placed a quantity of this salt less than 0·001 gram in weight upon an iron plate, and then touched it with a heated glass rod. A sharp and loud detonation resulted, and the plate was considerably distorted. The mercurous salt, Hg_2N_6, is likewise very explosive. The metallic salts are readily converted into

ethereal salts by reacting upon them with the haloid ethers. The phenyl salt thus prepared, $C_6H_5N\left\langle\begin{matrix}N\\ \|\\ N\end{matrix}\right.$, is in every way identical with the diazobenzene imide, so long ago prepared by Griess.

A. E. TUTTON.

PROF. S. A. HILL.

THE last Indian mail of September brings us the sad news of the death of Prof. S. A. Hill, one of the best-known of that small band of scientific workers, to whom we owe our present knowledge of Indian meteorology. He has been struck down suddenly, in the full maturity of his powers, and in the prime of life, after a few days' illness which gave no reason to anticipate so fatal a result. The son of a clergyman in the north of Ireland, Mr. Hill, after studying in the London School of Mines, and taking the degree of Bachelor of Science in the London University, was appointed, in 1876, to the Professorship of Physical Science in the Muir College, Allahabad, and, shortly after his arrival in India, received the additional appointment of Meteorological Reporter to the Government of the North-West Provinces, in succession to Mr. John Eliot, now the head of the Meteorological Department of the Government of India. In these combined offices, Prof. Hill has laboured for nearly fifteen years. In such spare hours as he could dispose of amid the exacting duties of his educational appointment and the administrative work of his office, in a climate which is but little favourable to mental or physical exertion, he devoted himself assiduously to those original investigations which have made his name familiar to the meteorologists of Europe and America. On subjects dealing with questions of terrestrial physics, he published numerous papers of high value and much originality in the Indian Meteorological Memoirs, the Journal of the Asiatic Society of Bengal, the Austrian *Zeitschrift für Meteorologie*, and the *Meteorologische Zeitschrift*; and an elaborate memoir on some anomalies in the winds of Northern India, in the 178th volume of the Philosophical Transactions. In this memoir he boldly endeavoured to map out the distribution of atmospheric pressure over India, at a height of 10,000 feet above sea-level, and showed how this distribution, differing greatly from that at the earth's surface, explains much that is otherwise anomalous in the winds experienced at the lower level, and especially the dry land-winds which play so conspicuous, and occasionally disastrous, a *rôle* in the meteorology of India. To the pages of this journal he was also a not infrequent contributor.

Having regard to Prof. Hill's high powers and his single-minded devotion to the work, of whatever kind, that lay before him, it is somewhat sad to read the following passage in an obituary notice in the Allahabad *Pioneer*, evidently written by one who knew him well. It need hardly be said that the Government referred to is that of the North-West Provinces and Oudh; not that of India, nor of Bengal, the relations of which to their scientific officers are known to be of a very different character. The writer says:—"Many of our readers who will recall their late friend's clear and accurate mind, his knowledge and his powers of application, will feel with a sense of bitterness that men of his capacity are not meant for the service of a Government, which is not only always ready to pass them over for a joint-magistrate who has been unlucky in his promotion, but will maintain that the latter is the best man. Mr. Hill was, officially speaking, the most unfortunate man of an unfortunate service [the educational service of the North-West Provinces]; but, no doubt because he had a talisman always with him in his devotion to science, he was never embittered by his ill-luck. With none of the eccentricities of a disciple of science, but with all the modesty and virtue of that character, he will pass away from us respected by all, and much more than respected by all those who were privileged to know him with intimacy."

H. F. B.

JOHN HANCOCK.

AT the venerable age of eighty-four years this well-known British naturalist has passed away, and it would be an injustice to his memory not to recall in these pages the effect of his life-work on the zoology of this country. He seems to have inherited his natural history tastes from his father, who was in business in Newcastle in the early part of the century, but was apparently devoted to natural history pursuits; and, in company with other kindred spirits, was intent on working up the natural history of Newcastle and the immediate neighbourhood. Unfortunately the father died at the early age of forty-three, in September 1812, leaving a widow and six children, of whom the eldest was only eight years of age. Mrs. Hancock, however, carefully preserved the collections which her husband had formed, and it was doubtless due to her affectionate interest that three of her children—Albany, John, and Mary—pursued the study of natural history with such success. The subject of this notice, John Hancock, seems to have turned his attention to ornithology in particular, and as early as 1826 he commenced the study of the artistic mounting of animals, which, as Mr. Bowdler Sharpe has said, has made John Hancock's name a password wherever the art of taxidermy is mentioned. Those who remember the celebrated groups of mounted animals which Mr. Hancock sent to the Great Exhibition of 1851, will testify to the revulsion of feeling which his beautiful work created, and every real naturalist felt in his heart that in this way alone could art and nature be combined in a Museum, and the public properly instructed in a due realization of the beauty and symmetry of form which animals possess in nature—beauties which are not reproduced in a Museum gallery once in a hundred times. That Hancock's influence should have been so little felt by the authorities of the British Museum is a reflection upon the officers of this institution, who ought to have utilized the genius of their countryman in making the collection of British animals in the National Museum a model for all nations to envy and copy. Anyone who knew John Hancock, his untiring energy and his unassuming amiability, will vouch for the fact that, if the British Museum had wished to have a collection of native birds naturally mounted, and worthy of this institution, he would have been only too delighted to aid in the achievement of such a task. As it is, the Museum of his native town, which really seems to have appreciated his genius, possesses a collection of birds of which any nation might be proud, and now that he is gone, those Museums (like the one at Leicester, for instance) which have series of birds mounted by this true lover and *connoisseur* of birds in nature, are to be congratulated. Of late years it is true that our National Museum has trodden the path indicated by Hancock, and a vast improvement in its taxidermy has been the result; but it will be a long time before any Museum can show such a beautiful series of birds as that which John Hancock has mounted for the Museum of his native town. An excellent biography of this esteemed naturalist has been published in the *Newcastle Daily Chronicle* of October 13.

NOTES.

EVERYONE was sorry to hear of the death of Sir Richard Burton, the eminent traveller and Orientalist. He died on Monday morning at Trieste, where he had been British Consul from 1872. He was in his sixty-ninth year. Burton was one of the boldest and most successful travellers of his time, and produced a great impression on all who knew him by the wide range of his talents, and by his energy and manliness. His career as a traveller began in 1852, when he undertook the journey to Medina and Mecca, of which he afterwards wrote so fascinating an account. His journey with Speke in 1857, which led to the discovery of Lake Tanganyika, placed Burton in the front rank of explorers. He had previously made a successful expedition into Somaliland; and at a later period he did much brilliant work in various districts of Western Africa and in Brazil.

WE regret to have to record the death of the Rev. J. A. Galbraith. He died at his residence in Dublin on Monday. For more than half a century he was connected with the University of Dublin, where he graduated in 1840. In 1844 his distinction as a mathematician secured for him a Fellowship, and in 1854 he was chosen Erasmus Smith Professor of Experimental Philosophy, along with Dr. Haughton. Prof. Galbraith was the author of various excellent scientific manuals.

DR. ALEXANDER WILLIAMSON, who died at Shanghai on August 28, had for 35 years been a member of various missionary bodies, and in his earlier years had travelled far and wide over North China, at a time when the greater part of that Empire was unexplored. His "Journeys in North China" is still a work of interest and value, for he visited many districts which are even still far outside the ambits of the missionary and the traveller, and his great knowledge of China renders the work very instructive. But his main work in life was the establishment in Shanghai of the Society for the Diffusion of Christian and General Knowledge amongst the Chinese, which is, we believe, maintained by subscriptions from various missionary societies labouring in China. Up to the time of his death, he was the editor and chief manager of the Society. Under his superintendence some hundreds of cheap books and pamphlets on all branches of science and on literary topics, suitable to Chinese intelligence and Chinese pockets, have been issued by the Society. Usually these were compiled by specialists amongst the missionaries, but occasionally a book already published abroad would be altered to meet the circumstances of the new circle of readers, and published in Chinese. It thus comes about that if an intelligent Chinese, knowing no language but his own, desires to make a closer acquaintance with that Western knowledge and civilization of which he has probably heard so much—whether it be anatomy, zoology, botany, mechanics, steam, the history of Napoleon Bonaparte, the story of the American War, the tale of Robinson Crusoe, the telegraph, the principles of hygiene—he goes to Dr. Williamson's series of publications and selects what he wants, usually at the price of a few cents or halfpence. The Society under his care has in fact stood as an interpreter between the East and West, and has striven to give to the former all the best that the latter has to give in the way of intellectual and moral instruction. This is surely as beneficent a task as can engage the energies of any man, and in it Dr. Williamson appears to have been most successful.

THE Agent-General for the Cape of Good Hope invites applications from gentlemen of appropriate scientific training and experience, willing to proceed to the Colony for a term of years, there to fill one or other of the undermentioned posts under the Government, viz.:—(1) That of bacteriologist, to investigate the diseases of domestic animals, supposed to be caused by germs. The salary offered is £500 a year. A free first-class passage by steamer (including railway fare to port of embarkation) will be provided. (2) That of toxicologist, to attend chiefly to forensic cases and to investigate South African native plants having medicinal properties. The salary offered is £400 a year. A free first-class passage by steamer (including railway fare to port of embarkation) will be provided. Applications must be accompanied by testimonials, and by copies of any scientific publications the applicants may have issued; and should reach the Agent-General for the Cape of Good Hope (112 Victoria Street, London, S.W.) by November 15 next. They will then be submitted to the authorities in the Colony, with whom the appointments rest.

AT the meeting of the organizing committee of the Oriental Congress held on the 9th inst., at the British Museum, it was resolved that Prof. Max Müller should be invited to preside over the Congress. He has accepted the invitation. Sir Henry Rawlinson, who was to have taken the chair, has been compelled to retire on account of ill-health.

THE Council of the Institution of Civil Engineers have issued a list of subjects on which they invite original communications. For approved papers they have power to award premiums, arising out of special funds bequeathed for the purpose. The Council will not make any award unless a communication of adequate merit is received, but will give more than one premium if there are several deserving memoirs on the same subject. In the adjudication of the premiums no distinction will be made between essays received from members of the Institution or strangers, whether natives or foreigners, except in the case of the Miller and the Howard bequests, which are limited by the donors.

THE nomination list of proposed members of the Council of the London Mathematical Society, for the session 1890–91, which will be submitted to members at the annual meeting on November 13 next, contains the following changes:—Prof. Greenhill, F.R.S., to be President, *vice* Mr. J. J. Walker, F.R.S.; Dr. J. Larmor, Major MacMahon, R.A., F.R.S., and J. J. Walker, F.R.S., to be Vice-Presidents. The proposed new members are Dr. Hirst, F.R.S., R. Lachlan, and A. E. Hough Love, in place of Prof. W. Burnside, Prof. Cayley, F.R.S., and Sir James Cockle, F.R.S., who retire. At the same meeting the retiring President will read an address on "The Influence of Applied on the Progress of Pure Mathematics," and will present the De Morgan Memorial Medal to Lord Rayleigh, Sec.R.S., in recognition of his writings on physical subjects.

HERR J. DÖRFLER has successfully completed his botanical expedition to Albania, and has returned to Vienna. From Ueskueb he crossed Kalkandele to Waica, and accomplished the ascent of both the Kobilica and the Serdarica-Duran.

THE late Dr. Henry Muirhead, of Bushyhill and Longdales, Lanarkshire, gave directions in his will that his estate—subject to certain life-rent provisions and legacies—was to be used for the establishment and maintenance of an institution to be named the Muirhead College, "for the instruction and education of women in medical and biological science, where women might receive an education to fit them to become medical practitioners, dentists, electricians, chemists, &c." The trustees, having obtained probate, have had several meetings, and it is expected that in the course of a few months they will be in a position to announce the arrangements they have been able to make. As the estate consists chiefly of lands, its money value must in the meantime be more or less a matter of opinion. The trustees, however, are hopeful that £30,000 at least will be available for the College.

We are glad to hear of the continued progress of what is called the University Hall scheme in Edinburgh. Its objects are (1) to make a beginning of social residence among the students of the University, and (2) to associate with this the extension of University influence among the people. The movement was begun in 1887, chiefly by Prof. Geddes. The house in which the experiment has hitherto been carried on having always had its full complement of residents, another house—an old building of considerable historic interest—has been secured; and this was formally opened the other day by the Solicitor-General for Scotland.

At the eighth meeting of the Congress of Americanists, an interesting address on the peopling of America was given by M. de Quatrefages. He expressed a strong belief in the unity of the human race, and in the consequent facts that the original home of mankind must have been confined to a very limited space, and that the world as a whole has been peopled gradually by processes of migration. He holds that America, like Polynesia, was peopled by colonists from the Old World. The peopling of Polynesia, however, was effected, he thinks, during our Middle Ages, whereas the earliest migrations to America date from geological times.

Under the title of "The Partition of Africa," Mr. Stanford will shortly publish a small volume by Mr. J. Scott Keltie, dealing mainly with the events of the past six years, and their results. In an introductory chapter or two, Mr. Keltie will seek to show what has been the footing of Europe in Africa from the earliest times. He will endeavour to estimate the value of the shares of the various European Powers in the scramble, from the point of view of commerce and colonization.

Messrs. Simpkin, Marshall, and Co., have issued the fifth edition of Mr. Rowland Ward's "Sportsman's Handbook to Practical Collecting, Preserving, and Artistic Setting-up of Trophies and Specimens." In the same volume is included a synoptical guide to the hunting-grounds of the world.

The new number of the *Internationales Archiv für Ethnographie* (Band iii. Heft 4) opens with an interesting paper, by Dr. Ed. Seler, on old Mexican throwing-sticks. Prof. Houtsma contributes notes on some pictures which once served as illustrations of a Persian "Fālbook."

The University College of Wales, Aberystwith, has published the Calendar for its nineteenth session, 1890–91.

The Manchester Literary and Philosophical Society has issued the third volume of the fourth series of its Memoirs and Proceedings. Among the memoirs are the following: on the law of cooling, and its bearing on the theory of the motion of heat in bars, by Charles H. Lees; on the combination of hydrogen and chlorine, alone, and in presence of other gases, by Prof. H. B. Dixon, F.R.S., and J. A. Harker; on some applications of caustic soda or potash and carbon in the qualitative and quantitative analysis of minerals, by Dr. C. A. Burghardt; description of a new reflecting telescope and observatory at Bowdon, Cheshire, by Samuel Okell; on the flexure of a flat elastic spring, by Horace Lamb, F.R.S.; and on absorption spectra and a method for their more accurate determination (with eight plates), by Dr. A. Hodgkinson.

The American Association for the Advancement of Science has issued its Proceedings at the meeting held at Toronto in August 1889.

A brilliant meteor was seen in the northern hemisphere from Edinburgh on Saturday last at 3 a.m. Its advent is said to have been announced by a flash of light which illuminated the whole city. A long fiery streak marked its course, and remained visible for more than a minute.

A report from Honolulu states that an eruption of the volcano Kilauea is feared, as a lava stream has formed lately, and part of it rose 15 metres in one day.

A shock of earthquake was felt at Christiansand, on October 8, at 5.15 a.m. The shock was directed from south to north, and lasted 3 or 4 seconds.

A slight shock of earthquake was felt at Lisbon on the evening of October 17.

According to a telegram sent through Reuter's Agency from Catania on October 18, Mount Etna is in eruption. At the time when the telegram was despatched, a thick column of vapours was rising from the central cone. A slight shock of earthquake had been felt on the eastern side of the mountain at Giarre and its vicinity, where a shower of cinders had also fallen.

Probably the deepest mine in the world (according to *La Nature*) is that at Saint-André du Poirier, in France. Of its two shafts, one 3000 feet, the other 3130 feet, the latter is being sunk to 4000 feet. A remarkable feature of this mine is the comparatively low temperature found in it, never exceeding 24° C. In the gold and silver mines on the Pacific coast, with a depth scarcely half that of French mines, there is great difficulty in keeping a temperature low enough for work. In some parts of the Comstock mines the temperature reaches 48° C.

The Harveian Oration was delivered by Dr. Andrew on Saturday last at the Royal College of Physicians. In the course of the oration Dr. Andrew referred to the fact that the relationship between physiology and medicine has in many ways greatly changed during the last 250 years, and that such change is a necessary consequence of the progress made by physiology. "The goal of physiology is truth—*e.g.* perfectly trustworthy knowledge of a certain class of facts and laws; and this independently of any use, good or bad, to which that knowledge may be put. The goal of medicine is power—*e.g.* ability to manipulate certain given forces in such fashion as to produce certain effects. No doubt, theoretically, the two ends coincide, and we may hope in some remote future they will do so in reality and perfectly. For the present we must be content with having in one direction much knowledge which confers little or no power, and, on another side, very imperfect knowledge which yet brings with it very great power, too often ill-directed. Again, their methods are different. Physiology by slow degrees has come to rely more and more on purely scientific modes and instruments of research, and to apply them by preference to matters which can be brought to the test of direct experiment. Medicine, on the other hand, has no choice but to remain, so far as it has a scientific side, a science of observation; for anything like effective investigation of the matters with which it deals by direct experiment is impossible. As physiology slowly reduces to order the apparently hopeless confusion of so-called vital actions, the easiest questions are attacked and answered first, and thus those which have to be faced later in their turn are more and more difficult, more and more refractory to scientific analysis. Now, these more difficult questions are often of vital importance to medicine, and in them lie dormant vast possibilities of increased knowledge of the nature of disease, of increased power over it. And yet, from the great difficulty of subjecting them to experiment, physiology may seem for a time to fail us, and the task of employing physiological results to explain clinical facts, or to form the basis of rational treatment, becomes harder than ever."

The additions to the Zoological Society's Gardens during the past week include a Speke's Antelope (*Tragelaphus spekii* ♀) from Lake Ngami, South Africa, presented by Mr. James A. Nicolls, F.Z.S.; two Reindeer (*Rangifer tarandus* ♂ ♀), European, presented by Colonel W. B. Thomson, F.Z.S.; a Beech Martin (*Mustella foina*) from France, presented by Mr.

H. H. Sharland, F.Z.S.; two Herring Gulls (*Larus argentatus*), British, presented by Mr. Joseph White; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. V. H. Dudmesh; a White Pelican (*Pelecanus onocrotalus*), South European, deposited; a Bay Colobus (*Colobus ferrugineus* ♀) from West Africa, purchased; a Large Hill-Mynah (*Gracula intermedia*) from India, received in exchange.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on October 23 = 0h. 9m. 4s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 5046	—	—	23 57 36	+15 33
(2) G.C. 5050	—	—	23 53 52	+ 4 35
(3) 30 Piscium	4	Yellowish-red.	23 56 19	− 6 31
(4) δ Andromedæ... ...	3	Yellow.	0 33 24	+30 16
(5) α Andromedæ... ...	1	Bluish-white.	0 2 42	+28 29
(6) W Cygni	Var.	Reddish.	21 31 53	+44 53
(7) T Aquarii...	Var.	Reddish.	20 44 8	− 5 32

Remarks.

(1, 2) Neither of these nebulæ have yet had their spectra recorded. The first is described as "considerably bright; considerably large; irregularly round; very gradually brighter in the middle": the second as "pretty bright; very small; much elongated; very suddenly much brighter in the middle."

(3) A star of Group II., the spectrum being described by Dunér as "very fine." All the bands 2-9 are very wide, dark, and strongly marked. As the star is a comparatively bright one of this class, a detailed study of its spectrum should be made, special attention being given to the brightness of the carbon flutings, and the presence or absence of dark lines.

(4) Secchi thought this star had a spectrum of Group II., but Dunér and Gothard describe it as one of the solar type, the latter observer, however, stating that it approaches Group II. According to Dunér, D and *b* are strong and dark, and several other lines are distinctly visible. At the place of band 2 (the iron fluting) in Group II. stars there is only a narrow and feeble line. It seems probable that the spectrum greatly resembles that of α Tauri, but as the band in the red has disappeared, it is probably a step higher in temperature. A direct comparison with α Tauri, which can now easily be made, might lead to interesting results as to the changes brought about by an increase of temperature in such a star.

(5) A star of Group IV. The usual observations are required.

(6) There will be a maximum of this variable about October 25. The period is short (120-138 days), and the range is from 5·8–6·2 to 6·7–7·3. The spectrum is an exceptionally fine one of Group II., all the bands being very wide and dark. We do not yet know whether any variations of spectrum accompany the slight changes of magnitude of such a variable as this.

(7) The spectrum of this variable has not yet been recorded. It is one of considerable range (6·7–7·8 to 12·4–12·7), and the period is 203 days. As the magnitude at maximum is not small, the observation of the spectrum should not be difficult. There will be a maximum on October 27. A. FOWLER.

PHOTOGRAPHS OF NEBULÆ.—The current number of *Comptes rendus* (October 13) contains a note by Admiral Mouchez on a photograph of the Ring Nebula in Lyra, obtained at Algiers Observatory by MM. Trépied and Rabourdin. The nebula was given an exposure of six hours, in two evenings of three hours each. The negative obtained is said to be very dense, and a positive copy, enlarged 64 times, has been presented to the Paris Academy. With respect to the photograph, Admiral Mouchez remarked:—"This image of the nebula is certainly the largest that has yet been obtained. It shows, in a very striking manner, the distribution of light in this curious celestial object. We see that a region of maximum light exists at each of the extremities of the minor axis of the elliptical ring. These two maxima are not equal, and in each of the halves of the ring the intensity of the light diminishes gradually up to the extremities of the major axis, where it has the smallest value. These are well-known characteristics of this nebula, and such as may be observed by means of ordinary telescopes. But the photographic observation teaches us other things. In fact, according to the work done at Algiers Observatory, when we photograph this nebula with increasing exposures, the nebulosity does not extend sensibly outside the ring, but spreads more and more towards the centre. On the other hand, when we observe the body in a telescope, we find that the central part of the ring is perfectly separated from the ring itself. The interior of the ring is therefore filled with a material difficult to see, but of which the existence is demonstrated in a certain manner by photography. In fact, the central nebulous star attains an intensity in the present proof nearly equal to that of the feeblest maximum of the ring.

"At the meeting of July 7, 1890, in presenting to the Academy a photograph of the same nebula obtained at Bordeaux Observatory by MM. Rayet and Courty with an exposure of three hours, I pointed out the probable existence of three and perhaps four, extremely feeble stars which had never been previously indicated, and which formed an almost regular square around the central star in the dark part of the nebula. The existence of at least three of these very feeble stars is now demonstrated with absolute certainty, because of the long exposure, but in the enlarged image they are somewhat confused with the inner edge of the nebula."

At the same meeting of the Academy (October 13), M. B. Baillaud presented a plate of the region about the Ring Nebula obtained at Toulouse Observatory on September 8, 9, 10, and 11, with a total exposure of nine hours. The size of the plate was 9 cm. by 12 cm., and it exhibits about 4800 stars to the naked eye within an area of three square degrees.

STARS HAVING PECULIAR SPECTRA.—In *Astronomische Nachrichten*, No. 2997, Prof. E. C. Pickering notes that photographs of stellar spectra taken by Mr. S. J. Bailey, at Chosica, in Peru, show several stars having peculiar spectra. The following table contains the places of these stars, and a brief description of the spectrum of each:—

Star.	R.A. 1900.	Decl. 1900.	Mag.	Description.
	h. m.	° ′		
Cord. Gen. Cat. 7191	5 59·4	− 6 42	5·8	F line bright.
,, ,, 18859	13 47·7	−46 39	6·6	F line bright.
,, ,, 19737	14 29·2	−41 43	2·5	F line bright.
,, ,, 22855	16 48·4	−44 57	Var.	G and *h* bright.
Cord. Zone Cat. 3612	17 55·1	−32 42	9·0	Bright lines.
S.DM. −19° 4854	18 2·1	−19 25	9·6	Bright lines.
Anonymous	20 9·4	−39 29	Var.	Bright hydrogen lines.
Cord. Gen. Cat. 29232	21 13·6	−45 27	6·0	Type IV.

The spectrum of the two stars with "bright lines" is similar to that of the stars discovered by Wolf and Rayet in Cygnus.

The two variable stars in the above list are new. Their discovery resulted from an examination of photographs of stellar spectra at Harvard College Observatory. A comparison of the intensity of the spectrum of the first-named star, situated in Scorpio, with that of others on the same plate, indicated that it fluctuates between magnitudes 7 and 11·4. A similar comparison of the spectrum of the latter variable, situated in Sagittarius, with the spectra of other stars near it, shows that between May and October 1889 it decreased from 8·5 to 10·7 magnitude. Both the stars have spectra of the same character as Mira Ceti and other known variables of long period.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.—The International Committee of the Photographic Chart of the Heavens, will meet at Paris Observatory on March 31, 1891. The last details as to the execution of the work will then be discussed, and it is hoped that all the participating Observatories will be able to begin operations immediately afterwards.

D'ARREST'S COMET.—In the same journal Prof. Krueger points out that the comet discovered by Mr. Barnard of Lick Observatory on the 6th inst., is identical with that of the periodical comet of D'Arrest, for which Dr. Berberich computed an ephemeris (*Astronomische Nachrichten*, No. 2959). An observation at Strasburg on the 10th inst. confirms the identity.

A NEW ASTEROID.—Dr. J. Palisa, of Vienna Observatory, discovered a new minor planet (299) on the 7th inst. Its magnitude was 14.

THE TEACHING OF BOTANY.[1]

THE discussion was opened at great length by Prof. Marshall Ward, who reviewed the whole subject of teaching botany (1) to very young children and in schools, (2) as an academical study at the Universities, and (3) as a special subject for those who are in training for technical and other pursuits which require a knowledge of that branch of science—*e.g.*, foresters, gardeners, timber merchants, &c. He said :—

As I understand it, we may regard the study of botany as approachable from three points of view. We may speak of three ends to be attained : those of (1) elementary botany as a school subject of general education ; (2) advanced botany, as a subject of University or academic training, with a view to teaching and research ; (3) special botany, for various purposes in after life—*e.g.*, those of foresters, planters, agriculturists, horticulturists, brewers, medical men, timber merchants, &c.

This is, of course, a merely arbitrary division for the argument, and not a philosophical classification of the subject-matter of the science of botany.

The next point is the scope of the teaching in each case. I should advocate that all children pass through the preliminary training embraced under No. 1. Not only so, but I would urge the usefulness and importance of elementary botany in schools quite apart from its possible pursuit afterwards.

It seems to me that the time is gone by when we need discuss the direct applicability of teaching in elementary schools: if school training is read to mean education, in the true sense of the word, then there is no necessity for asking that a boy and girl should learn at school only those subjects of which they will make direct application as they grow older. Of course this does not preclude our keeping in mind the relative utility of the various subjects to be taught, but it does—and emphatically—preclude our falling into the error of imagining that a school-subject is of educational value only in proportion to its direct and foreseen utility in the application afterwards. In other words, educating and teaching may be, and often are, very different things.

Now, as I understand it, the nineteenth century has discovered—possibly re-discovered—the truth, that you may impart a wondrous amount of information to a boy or girl without awakening those powers of observing and comparing that lie dormant in the minds of most healthy human beings, and especially when young ; and that many a brilliant boy grows up without being able to draw correct inferences from the phenomena around him, and therefore less able than he should be to hold his own in the world he awakes in.

The peculiarity of the study of elementary botany, properly understood and pursued, lies especially in the interest it arouses in the child's mind, and the ease with which it may be taught, and I would insist and re-insist on the fact that it stimulates and cultivates just those powers of accurate observation and comparison, and careful conscientious recording of the results, which are so needed by us all ; and which, be it understood moreover, come so naturally to children who are not too much under the baneful influence of the mere instruction—the mere information—system.

What I wish to emphasize is that the educational value of this subject is no more to be measured merely by the number and kind of *facts* which the child remembers, than is the educational value of history to be measured by the dates learnt, and the lists of kings and battles committed to memory. History, reading and writing, arithmetic, and other subjects, have an educational value, if properly taught, quite apart from their value as mere accomplishments, which may be granted; but children are naturally observers, and why this side of their hungry little natures should be starved at the expense of their usefulness in after life has always been a mystery to me.

To those who allow this, and I am happy to see that their numbers are now many, it should hardly be necessary to point out that the elements of botany afford the cheapest, cleanest, and most easily attained means of cultivating in children the powers of observing and comparing direct from Nature, and of leading them to generalize accurately.

Of course no advocacy is needed for good preliminary education in elementary botany in the case of those who are about to continue the pursuit of the subject as an academic study, or for a special purpose, as noted under the headings (2) and (3); but a few words may be devoted to pointing out the shocking waste of time and energy, on the part of all concerned, in the prevailing cases where students come up to a University, or other institution for higher education, insufficiently prepared for progressive study.

It is still true that boys and young men leave school without so much as a notion of the real meaning and aims of science: this applies no less to subjects like physics and chemistry, which are professedly much taught in schools now, than to subjects like natural history and botany, which, though avowedly in the curriculum of some good schools, are usually entirely ignored.

There is considerable discussion about the details, but many practical teachers regard such subjects as unfitted for school, because the boys and girls soon cease to be interested, and get lost in the masses of facts and hard names that beset their path : this, to my mind, simply shows where the whole system is wrong, and wrong because the tyrant empiricism still rules the prevailing methods of teaching in schools.

I shall go so far as to say that the only remedy for this state of things is for the teachers to lose that blind worship of facts, as facts, which dominates our school system. I am aware that this lays me open to very serious misconstructions, but I hope to make that all right in the sequel.

I would say to the teachers, therefore, do not fall into the mistake of measuring a boy's progress by the amount of dogmatic information which he imbibes, and splutters forth on to his examination papers, but look to the quality of his understanding of the relations between relatively few and well chosen facts ; and again, pay less attention to the number of facts which a boy observes and of names he remembers, and more to the way in which he directly makes his observations, and intelligently describes them, even if untechnically.

This is, I firmly believe, the only cure for the malady under consideration—*i.e.* it is the prevention of it.

Children in schools are taught most subjects from printed books, and it is not my province to criticize the necessity of this as regards those subjects ; but let a competent teacher try the experiment of making the children read directly from Nature, and he will soon see that the new exercises have a powerful effect. They will stumble, and they will even make stupid mistakes and mispronunciations; but do they not do so when they are reading—*i.e.* observing and comparing and interpreting—printed words in a book? Of course they do, and therefore the teacher must not be discouraged by their stumbling and misapprehending when first they have to look at and compare different leaves and flowers, and give forth the articulate sounds which correspond to the impressions created on their minds.

Every weary teacher knows what a blessing is variety in the studies of the class, and it passes my comprehension why advantage is not taken of the splendid opportunity offered by the study of elementary observational botany.

We now come to the important subject of method. How should botany be taught?

Here, again, I shall consider the subject from the same three points of view referred to above.

(1) Elementary botany in schools should be confined to lessons in observation and comparison of plants, and the greatest possible care should be taken that books are not allowed to replace the natural objects themselves. Indeed, I would go so far as to advise that books be used only as an aid to the teacher, were it not that a judiciously written text-book might be employed later on by even young children as a sort of reading-book.

The chief aids should be the parts of living plants themselves, however, and, in spite of the outcry that may be expected from pedantic town teachers, I must insist that every school might be easily provided all the year round with materials for study. I even venture to think that these materials might be collected by the children themselves : at any rate there should be no difficulty about this in the country.

I will illustrate these remarks by a few examples. The teaching of elementary botany to children should commence with the observation of external form, and might well be initiated by a comparative study of the shapes of leaves, the peculiarities of insertion, their appendages, and so on.

The point never to be lost sight of is that if you teach a child to discriminate, *with the plants in hand and from observation only*, between such objects as the simple, heart-shaped, opposite, ex-stipulate stalked leaves of a lilac, and the compound, pinnate, alternate, stipulate leaves of a rose, you lay the foundations of a power for obtaining knowledge which is in no way to be measured

[1] Discussion at the Leeds meeting of the British Association, in Section D, on September 5.

merely by the amount or kind of information imparted. It does not matter whether the child learns the trivial facts mentioned above, or not, but it is of the highest importance that the child be taught how to obtain knowledge by such direct observation and comparison; and the beauty of it all is that, as is well known, the child will retain most of such information as mere matter of course.

For the main purpose in hand, therefore, it may be contended that any objects would do.

This is no doubt true in one sense, but it should not be forgotten that (1) the mental exercise on the part of the child is best exerted on *natural objects*, to say nothing of the admitted advantages of familiarizing him with Nature, and (2) the parts of plants are so varied, so beautiful, and so common, that he need never lack materials for his simple and pleasant work. Moreover, the parts of plants are clean, light, and easily handled—practical advantages which recommend themselves.

I feel convinced that, if the teachers were not opposed to it, the subject would ere now have been more widely taught; and I shall therefore say a few words in anticipation of difficulties. It has been suggested that materials would be scarce in winter. Not at all. Let the children be familiarized with the observation and comparison of the peculiarities of a sprig of holly as contrasted with one of ivy; or let them be shown how different are the buds and leafless shoots of the beech from those of the oak or the horse-chestnut. Show them how to observe the bud-scales, how to infer the leaf-arrangement from the scars, how to notice the colour, roughness, markings, &c., of the periderm. Or give them introductory notions as to the nature of a hyacinth-bulb as contrasted with a potato-tuber, confining their attention to points which they can make out by observation. Every nut or orange or apple that a child eats might be made interesting if teachers would dare step over the traces of convention, and introduce such ostensibly dangerous articles into class-work—and why not? The doctrine of rewards and punishments is applied more crudely than this in most children's schools!

Be this as it may, there is no lack of material at any season, for children to observe and compare, plant in hand, the peculiarities of shape, colour, insertion, markings, &c., of the leaves, stems, roots, and other parts. The difficulties are supposed to increase when the flower is reached: this is not necessarily the case in the hands of a sympathetic teacher, unless the choice of flowers is very unfortunate and limited.

There is one danger to be avoided here, however. Young children should not be troubled with the difficulties of theoretical morphology: they should be made familiar with the more obvious roots, stems, leaves, tendrils, thorns, flowers, bulbs, tubers, &c., as such, and comparatively, and not forced to concern themselves with such ideas as that the flower is a modified shoot, the bulb a bud, the tendril a leaf or branch, &c., until they have learned simply to observe and compare accurately. Later on, of course, the step must be taken of rousing their minds to the necessity of drawing further conclusions from their comparative observations in addition to recording and classifying them; but if the teacher is really capable of teaching, it will be found that the children begin to suggest these conclusions themselves, and, this stage once reached, the success of the method is insured.

Glimpses of the meanings of adaptations of structure to function soon follow, but they should be obvious and simple at first, and the mistake should not be made of entangling a child in a discussion as to more remote meanings. It should never be forgotten, in fact, that the first steps consist in learning to observe accurately and to record faithfully, comparative exercise being used in addition, both as a check and as a stimulus to the judgment.

The next step is to introduce the methods of the systematic botanist who works in the field, with flower in one hand and lens in the other; and the necessary preliminary and accompaniment of this is to exercise the tyro in describing common plants as a whole. The value of such training in the field can scarcely be over-estimated. As education it is excellent, for it inculcates neatness and accuracy of method, keenness of observation and judgment, and is, moreover, interesting to the young student, as well as healthy in every sense of the word. As preliminary training in all cases where the student will have to pursue the higher branches of botany, or other science, at a University or a technical institution, it is absolutely necessary. There is no need to enlarge on its value to the traveller, the philosopher, and even the *dilettante* who enjoys Nature in his garden, or in the country, or even merely as a reader of books on natural history: just think what enjoyment such a training would add to the lives of thousands who have read Darwin's works imperfectly, and reflect for a moment on what such intelligent appreciation of such writings means to a nation like ours.

(2) The necessities of the higher academic study demand previous acquaintance with the *facies* of a large number of plants—Cryptogams as well as Phanerogams—and it is on this account advisable also that the student has been well trained in field-work: he should, then, be familiar with terms and groups, and be able to observe and compare.

Two chief lines of instruction are open at once to the advanced student, and the first point for discussion is, how far they should be kept separate or together: they are morphology and physiology, for, say what we will, the two are separate studies in their aims and methods.

It is not improbable that the study of pure morphology may be carried too far, as an independent study, and that one-sided views of the nature of plants and their parts may result; but, however true this may be, I take it no botanist will deny that every student should know something of the attainments and aims of modern morphology. If this is admitted, the next point is not likely to be gainsaid—namely, that the study of morphology depends on the study of anatomy and histology, as well as upon that of external form. As we shall see, the same is true, but in a different way, of physiology; but I am concerned at present with morphology only.

It seems to me, in view of these facts, that the advanced teaching must presume an acquaintance with the elements of anatomy and histology; and here, again, I am convinced that if teachers fully recognized how clean, and light, and easily accessible the material is, and how excellent the training of hand and eye on the one side, and of the thinking powers on the other may be made, the difficulties of introducing this elementary laboratory work even into secondary schools would be overcome.

It has been overcome in many cases with regard to chemistry, and there is no reason why it should not be overcome with regard to botany.

However, be it as advanced work at school, or as elementary work at college, the student who proposes to pass on to the higher academic study of botany must face the truth that even an extensive knowledge of the outside forms of plants will not carry him far on the road to be traversed.

Now comes the question hard to answer—Should he study anatomy and histology by selecting the best known and clearest tissues, tissue-elements, &c., from any part of the vegetable kingdom; or should he choose some one plant, and explore the recesses of its structure as thoroughly as possible?

All things considered, I believe the introduction is best effected by the latter method, and for the following reasons. In spite of the drawback that no one plant can be found which shows every tissue or tissue-element at its best, one finds that, by exploring the structure of some one plant as thoroughly as possible, the thoughtful student obtains a better idea of the co-relations of the structural elements than if he seeks for xylem vessels in Maize, sieve-tubes in Cucurbita, collenchyma in one plant, sclerenchyma in another, and so on.

Moreover, the comparative survey can be better carried out, if time permits, by methods such as I advocate.

The next consideration is the selection of the type to be used as a basis. In spite of all its defects, and in anticipation of severe criticism, I maintain that the fern is, on the whole, the most useful and convenient type for the purpose.

No Thallophyte is sufficiently obviously complex in structure to give the student the necessary ideas of co-relations of parts and division of labour; moreover, the lower forms offer peculiar difficulties of observation, cultivation, &c. The moss is too specialized for some purposes, and not sufficiently complex for others. The Phanerogams, on the other hand, although they present the vegetative tissues, members, &c., in the more highly developed and specialized forms familiar to physiology, offer such stumbling-blocks to the tyro in morphology that no one will serve as a suitable type. The pine is the best of those proposed, but even it presents great difficulties to a beginner.

The disadvantages of the fern (taking *Aspidium*) embrace the following: its roots are fine, the stem is short, and the vascular bundles belong to an out-of-the-way type; the spores take a long time germinating, and the prothallus offers difficulties in the way of investigation not easily overcome by a school-boy.

On the other hand, the roots are fairly typical in structure, and introduce the student to the ideas of the root-cap, apical cell, radial bundles, and axial vascular cord. The stem, at least, shows how the vascular bundles have definiteness and continuity of course, in axis and appendages, and these bundles are so large and isolated that an introduction to the notion of their development from embryonic tissue is at least attainable; moreover, the spiral vessels, scalariform tracheides, sieve-tubes, and packing-cells suffice very well—though, of course, in different degrees—to introduce the elements of the xylem and phloem, and I regard it as an advantage to defer the complex idea of cambium.

Elementary notions of other items of complexity appear in the extra fascicular strands of sclerenchyma, while protective hairs, reserve-starch, continuity of leaves and axis, and their origin from the meristem, &c., all serve as foundation stones if properly demonstrated and discussed by the teacher.

But it is the sporophyll on the one hand, and the prothallus on the other, which make the fern so supremely useful as a type. No conceptions in the morphology of plants have been more fruitful than these, and it is of the highest importance that the student really sees and examines these and their accessories for himself.

The beauty of the fern sporophyll as a type for demonstration lies in its being so evidently a leaf, in the sense understood at once by the beginner; then the sorus, sporangium, and spore are evident and easily examined, and even the very useful ideas of the archesporium, tapetum, and the development of the spore can be mastered in the case of the fern with comparative ease.

As for the prothallus, it is admitted to be the most accessible of all, and advantages may be claimed for its independence as a chlorophyll-bearing structure, in spite of its flatttened and somewhat specialized form. The antheridia are curious, no doubt, but the spermatocytes and antherozoids and their development are easily made out so far as general features are concerned: the archegonia are not so typical, perhaps, as those of the moss, but they are sufficiently so to be very useful, and the oosphere, canal-cells, &c., are easily seen by an apt student.

Moreover, I would point out that in the hands of a properly guided student of average intelligence, the teacher can rely upon the fern prothallus for introducing some theoretical notions very difficult to acquire—*e.g.* the gradual separation of the sexual organs, and their withdrawal into the prothallus, and the eventual separation of male and female prothallia, and *their* reduction and withdrawal into the spores, leading to the final specialization of male and female spores, and *their* retention and reduced germination inside the sporophylls, which also become specialized.

I should explain here that I would not propose to carry this explanation of homologies too far at this stage, but my argument is that the foundations for much that is to follow can be laid now with better effect than at any other time. It may be contended that the elementary student cannot possibly understand the Hoffmeisterian morphology until he has mastered the structure of the ovule of the Phanerogam, and that, therefore, it makes no difference in this respect whether he begins at the one end or at the other. I grant this, but my plea is not for the crowning of the student's knowledge of morphology, but for the *foundation* of it, and I lay so much stress on his laying this foundation thoroughly—otherwise it will not bear the weight of the superstructure I should propose to raise on it—that I look for the best type for that purpose; and, bearing in mind that such a type must be convenient, and one wherein the student can find the objects and examine them himself, I believe it has been found in the fern.

It will no doubt be remarked that, in the preceding discussion, I have kept in view more especially the study of morphology as the aim of the young academical botanist, and that it is because the fern is so excellently situated midway in the vegetable kingdom that it forms so good a type for teaching purposes. If it is urged, however, that physiology is the study to be more especially kept in view, then it may be necessary to reconsider the question of a type.

But there are two reasons, to my thinking, for discarding the idea that the study of physiology should be the immediate aim of botanical teaching in schools at present, though I do not despair of its introduction in the near future.

Firstly, the appliances needed, simple as they are in most cases, nevertheless *are* appliances, and will, as matter of fact, bar the way to the study during school life for some time to come; secondly, however much we may insist that the study of the physiology of plants presents its own problems and phenomena apart from those proper to physics and chemistry—and no one can urge this more earnestly than I do myself—nevertheless it cannot be gainsaid that the student of physiology should have a fair acquaintance with elementary physics and chemistry, even at the outset. I am aware that the contrary has been asserted, and that it has been argued that a student may learn to rig up apparatus for demonstrating the respiration of germinating seeds without knowing anything about the properties of oxygen, or what happens when carbon dioxide passes into a solution of barium hydrate, and that he may perform experiments on assimilation knowing no more about starch than that it turns blue with iodine, or on transpiration without understanding anything of the physics of the atmosphere or of water; and I am not prepared to say that such training would be without benefit, but apart from the advantages of the preliminary knowledge of phenomena, every teacher knows how dull is the comprehension of the boy's mind when brought face to face with such experiments devoid of the necessary physical concepts, as they have been termed; and in any case the necessary minimum of physics and chemistry will have to be instilled at the time the experiment is performed.

Secondly, the study of histology—practical acquaintance with the microscope—is a necessary preliminary to physiology, and I am doubtful whether we are at present in a position to demand more than the beginnings of these matters from the schools, though the time will come when it will be disgraceful for a boy to leave school quite ignorant of them.

The study of the fern should be followed by that of the *pine*, and I am not prepared to demand a continued adherence to the type-system beyond this point, except under special and favourable circumstances, such as need not here be discussed. Indeed, I should be quite satisfied if we could depend on school-children learning how to describe plants fairly accurately, and on the boys and girls in secondary schools knowing something more of field botany and how to use a flora, and having a satisfactory acquaintance with the life-history and structure of a fern and a pine. When I speak of field botany as above, it is not intended to exclude an acquaintance with the external appearance of common Algæ, Fungi, lichens, and mosses, &c., though the extent of that acquaintance would necessarily depend upon circumstances.

It must not be overlooked, however, that somewhere between this stage and that of further progress to the higher departments of academic botany, the student will have to do some comparative anatomy and histology, on the one hand, and to master the details of the life-history of certain types of Algæ, Fungi, and Lichens, Muscineæ and Vascular Cryptogams, and look more deeply into that of the Phanerogams.

It depends on circumstances whether the type-system should be followed here or not. If the student is going to specialize in the direction of morphological botany, I am inclined to the opinion that he should steadily pursue the type-system, supplementing his work with comparing special structures selected from allied types as he proceeds. For instance, after working through the life-history of a *Pythium*, he should not need to devote his attention to actually exploring all the details in the life history of *Mucor* and *Peronospora*, but he should see the sporangia of these, and the haustoria of *P. parasitica;* and again, having worked through the chief stages in the life-history of *Marchantia* and *Funaria*, say, there is no need to insist on the same pursuit of detail in the case of other Muscineæ, but the student might compare with the corresponding structures in his types the sporangia of *Anthroceros* and *Jungermannia*, &c., the leaves of *Sphagnum* and *Polytrichum*, and so on.

If the student is more inclined to the pursuit of physiology, I should prescribe a different course as soon as he has examined a few types of Algæ and Fungi, a moss, and a few Vascular Cryptogams, and I should, moreover, direct his attention at once to the highest plants—the Angiosperms—instead of leading up to them as in the case of morphological studies.

In fact, the system to be pursued for a training in physiology, is to select the best illustrations of the organs, the tissues, and the histological elements of which the functions are to be studied. For the typical root I should go to one plant, but it might be necessary to employ quite another plant for showing root-hairs or root-cap: while selecting the vascular bundles of *Ranunculus repens* or of *Aristolochia* to show certain facts about the bundles as a whole, I might take those of *Cucurbita* for sieve-tubes, those of *Linum* or *Vinca* for bast-fibres, and those of quite other plants for spiral or pitted vessels, &c.

So also with other structures, the training is designed to familiarize the student with the best examples of each structure, and although he must acquire a sufficient insight into the relations of these structures and parts to be able to understand how they work together, and how the functions of some depend on those of others, still his aim is not to follow out their development and relations in space and time, but to deal with their behaviour now and in the mature plant.

Up to a certain point both morphologist and physiologist must work along the same lines: they then diverge, and it is at this period that the more extensive use of books must come in; for the student should now have so *real* a knowledge of the things discussed, that illustrations and information are clear to his understanding. The intending physiologist must put himself in possession of sufficient histology and anatomy to be able to follow the work of the specialists in this domain, and to see what bearings their discoveries have on his branch of investigation: no less must the morphologist follow the special literature, but with his own very different end in view. Both will, of course, have their special literature also.

However, it is obvious that we have now reached a point where no very rigid rules can be laid down, since the advanced academical student is in a position to strike out his own lines, and if he does not display some originality now in his methods, aims, &c., the presumption is that no amount of training on the part of teachers will lead to it. Nay, more than this, it is highly desirable that he should be left alone, for the dormant originality is as likely as not being kept down by the pressure of prescribed studies.

(3) In illustration of what is required in special branches of botanical study, I cannot do better than take the case of the properly-educated forest-student: go where you may, you are not likely to meet with a more representative "practical man" than the trained forest-officer, and consequently his case is peculiarly well adapted for my present purpose.

No one will be so rash as to argue that the botanical training of a forester should err in subordinating a knowledge of trees and wood, the phenomena of germination and nutrition, of growth, &c., to transcendental hypotheses and discussions on the nature of morphological conceptions or on abstruse questions as to the significance of movements of irritability, or the ultimate mechanism of reproduction and the molecular forces concerned in heredity: on the contrary, most people will concur in agreeing with me that the teaching of forest botany should be directed to laying down in the student's mind a good foundation of facts of observation, and showing him how to acquire others, and, further, to training his mind to reason accurately from these facts, so that he may apply his reasoning to the practice which is to be his life's pursuit.

On the other hand, there is a danger which very few people escape when talking on this subject, and that is the danger of supposing that the attention of the forest-student should be confined simply to acquiring and remembering aphoristic statements of facts, and that his accomplishments in this connection measure the fitness of his training. In other words, many so-called "practical men" argue that it is the *quantity of information* which tests the student's progress, and neglect the truth that progress is much more adequately represented by the *quality of the instruction.*

Let us put the case in another way. It is granted that the forest-student must be made acquainted with certain facts of observation, and that he must be informed of important conclusions derived after comparing these facts: it is also granted that his time for training is limited—there is no getting over this, and we need not discuss what the limits are, or why they are so. Now, the problem is, Shall the student devote the whole of this period of training to simply acquiring as many of these facts as possible, the conclusions being limited to those directly applied in the forest; or shall more attention be devoted to the methods of acquiring these facts and of drawing the conclusions from them, and the facts themselves be utilized rather in so far as they are necessary for the training, than as the ultimate aim of that training?

The answer to this question is of the highest importance. If we decide that the chief object of the forest-student's training is to make himself acquainted with the facts themselves, then his whole time will have to be given to such matters as learning the names of plants; the peculiarities of the roots, bark, wood, buds, leaves, &c., of the various trees; the empirical facts as to the relative amount of light, moisture, &c., and the degrees of temperature that each species will bear, and so on; the ascertained growth in height of each species, and the annual increment it exhibits, and so on. It is obvious that, if the student worked continuously for his two years or so of probation, he could make himself or be made acquainted with an enormous mass of such information, but it is equally obvious that he could not nearly exhaust the catalogue of facts. The latter truth becomes still more apparent, however, when we remember that he has to devote his attention to several other branches of study in addition to botany.

But is this the right decision to come to in face of the problem I put before you? I say no! emphatically no! On the contrary, it should be recognized at once that the forest-student cannot acquire more than a small proportion of the facts of his subject while he is in training, and even if he could they would be of no use to him in this shape. The selection being limited, then, it should be the aim of the teacher to direct the student's attention to a selected number of facts (you need have no fear that the list will be a short one) such as throw light upon matters that the student will not be likely to explain for himself, unless he is directed. The facts of the forest will be before him always; why, then, occupy the valuable time of training with an incomplete catalogue of them? There are thousands of other points, however, that he will never know anything about if he does not learn how to observe and infer them while he has the chance with a competent teacher by his side.

Let me give an example. The details of the different modes of germination of the various seeds of trees are numerous, but they can be collated under a few heads. Some seeds, like those of the beech, raise their cotyledons above the surface of the soil, and they become green and expand; others, like those of the oak, remain underground, and devoid of chlorophyll, and do not expand. As sown, however, the beech-mast and acorns are not seeds, but fruits, for each is enclosed in its pericarp. Both agree in having two cotyledons to the embryo; and although the beech seed contains a thin remnant of endosperm, both are usually termed exalbuminous; moreover, the cotyledons have their cells crowded with food-materials consisting chiefly of starch-grains and oil.

The seed of a date-palm, on the other hand, is provided with large stores of food-material in the form of cellulose, as thickening materials to the cell-walls of the endosperm, and it contains a relatively minute embryo, furnished with one knob-like cotyledon only; while the seed of a Scotch pine has a large, fatty endosperm, and a poly-cotyledonous embryo in its axis. The details of germination of the palm and the pine differ, and both in different ways from those of the beech and the oak.

Now it is unquestionable that the forester ought to understand what are called the phenomena of germination; but the inquiry arises, Do we mean by this that he ought to learn the details of the germination of these and a large number of other seeds, or do we mean that he should be made acquainted with what research has shown to be common to all seeds, and then with the chief classes of difference in detail? In other words, is he to be taught generalizations, and shown by a few well-selected examples how they have been and are being arrived at; or is he to be burdened merely with the details themselves, as stated in the words of and on the authority of others? Undoubtedly the former is the true method: the latter is simply empiricism.

Let none fear that the student who is thus taught will learn too few facts—the fetish of the "practical man."

In the first place he cannot proceed without sufficient information to enable him to understand the physiological value of such bodies as starch, cellulose, oils, and proteids; and, without troubling him with the refinements of micro-chemical methods, he will at least have to be made acquainted with the better-known changes which these bodies undergo in the presence of water and oxygen, and with the metamorphoses comprised under metabolism; and here his botanical knowledge comes into intimate relations with his information on elementary chemistry.

But, further than this, how is he to proceed to an understanding of even the outlines of the physiology of germination until he knows the leading phenomena of fermentation on the one hand, and of respiration on the other?

I will not enlarge upon this part of my subject however, but simply assure those unacquainted with the full bearings of these remarks, that there is no paucity of facts in this connection, and that, simply to make himself acquainted with the more salient ones, the student has to devote many hours of careful study in the laboratory.

But he will not understand the process of germination unless

he is acquainted with the structure of the seed. Here, again, it is not the details of structure of the seed-coats, the nucellus, and the embryo, which differ in each seed taken, that are to tax his memory and disgust his mind, but he must be made familiar with the leading features common to all seeds, and illustrated by a few selected examples. The nature of the seed-coats, the structure of the embryo and its relations to the endosperm, &c., are easily taught, if the teacher knows his art, and the pupil is properly led up to his work; otherwise, I fail to see how the latter is to gain any idea of what a seed is on the one hand, or of how a tree arises from the embryo on the other, and if he does not understand what a seed is, he will never comprehend the process of germination, and he thus misses the best chance of elucidation as to the development of the complex structures of the root, stem, and leaf, &c., which follow.

I have said nothing of the phenomena of growth, moreover, and yet the problems of germination will remain obscure and unintelligible until the student knows something about growth; and this presupposes at least some notions as to the phenomena of cell-division in the embryonic tissue, and of cell-growth and development.

Why say more? It is obvious that these studies lead the one to the other, and the real difficulty is to select the best illustrations and use them to the best advantage.

The forest-student's curriculum, therefore, is not to be regarded as a *narrow* one because he needs only a catalogue of facts, but as a *special* one because the exigencies of his professional time demand his attention to certain classes of phenomena. His early training—would that it began at school—should be in the observation and comparison of plants and their organs: he should then proceed to more comprehensive field-work, and exercises in the description of plants and systematic botany. In selecting his examples special attention should be paid to trees and shrubs, which are commonly neglected by students, and the lens should be always at hand.

Studies in the elements of anatomy and histology must follow, otherwise his progress will be hampered when he has to deal with the subjects of germination, nutrition, growth in thickness and formation of wood, cortex, bark, &c.

Refined histology, special anatomy, and speculative morphology will have to be neglected, nor must he aim at becoming a specialist in taxonomy. His laboratory work must be directed to the end that he may understand the general structure and relations of tissues and organs, otherwise he cannot understand what is known of their functions; that he may have clear ideas as to the parts which yield economic products, otherwise he becomes lost in the long catalogue of these; that he may grasp the salient features in the structure of the different kinds of wood, otherwise he cannot attempt to classify and identify them; that he may know something of the biology of fungi, otherwise he cannot hope to understand the diseases of timber which they cause, or the important scavenging and other work which they perform in the forest, and so on.

It would take too much space and time to enlarge on the pity of the fact that young forest-students come up for training almost totally unprepared for such a curriculum, and especially devoid of the elementary knowledge and powers of observation which they should have received at school: the consequence is, much of their valuable probation period is occupied with acquiring the elementary facts and methods without which they cannot possibly make progress in more special work. Now I should like to see all this altered, and the only way to effect the necessary salutary changes is to have some guarantee that such probationers have a suitable training in elementary botany while they are in the receptive condition of school life.

Let me now suppose the case of a young man destined for a career as a brewer. No one will deny that an essential part of his training should consist in a thorough schooling in the methods of cultivating and separating the various forms of yeast, bacteria, and moulds which are met with in every corner of a brewery, and some of which are the agents on the proper action of which he depends directly, while others are his enemies —for I need not remind you that the fermentation industries all depend on various yeasts, and that the diseases of wine and beer, &c., are due to the interfering action of other microscopic organisms of the nature of yeasts, moulds, and bacteria.

This is all clear, and generally accepted, but I am not so sure that everyone recognizes the fact that the proper study of these fungi and allied organisms is a department of botany; though I am quite sure that many people suppose that it is the province of the chemist to clear up the mysteries of these agents of fermentation and putrefaction.

It requires long practice with the microscope and with botanical methods of investigation to trace the vagaries of even the largest of these ferment-organisms, however; and without implying in the least that some of the methods and results of modern chemistry are not essential in such investigations—for the contrary is really true—I would urge the absolute necessity of a botanical training before the student can grasp the meaning of the problems to be solved.

It is surely childish to reply that the special technical methods of the brewer's microscopist can be acquired without the preliminary training in botany which is here pleaded for. I know they can be acquired, as merely technical processes, and I do not deny that relatively good work has occasionally been done under such conditions by men of genius and industry, who have acquired the botanical knowledge as they proceeded; but the point is that the technologist who has had no training in botany is found groping over problems in a manner he would never have had to do had he a proper view of the nature of plants and plant-life such as a suitable training in the elements of botany would give him.

This training, if commenced at school with exercises on observing and describing plants, and then pursued far enough to give him correct ideas of structure, of the nature and grouping of the histological elements, and of what is best known as to their functions in the physiology of nutrition, growth, and reproduction, would at least save the student from those crude notions as to the so-called physics and chemistry of a yeast-cell or of a fungus-hypha which one so commonly meets with.

I am not in any sense implying that a brewer's technologist should be a botanist, in the accepted meaning of the term: I only urge that he has to confront problems of *physiology* and of *morphology*, over and above his every-day riddles of chemistry and physics; and that even if we concede that physiological actions are nothing more than complex and conditioned physical and chemical actions (and I do not deny this), it is still true that he should be quite clear that this implies much more than it is commonly supposed to imply, and have at least an inkling of what we know as to the complexity of metabolic and other processes.

Now he cannot be clear on this subject unless he knows something of modern plant-physiology; and he cannot follow the teachings of physiology unless he is familiar with what is best known as to the structure of plants, and their general nature. How far he should go in these studies is not for me to limit, but he must at least be able to grasp enough to enable him to understand the progress of the science, and to see how far he is justified in drawing inferences from phenomena observed in other plants and applying his conclusions to the plants he is studying. To attempt to study the behaviour of a yeast-cell, or of a bacterium or mould, without clear ideas as to what is known of the plant-cell generally, seems to me very like obstinately attempting to open a lock in a dark room when you are ignorant of the whereabouts of the lock and have not found the right key.

What I have said with respect to the study of ferment-organisms holds good with regard to the study of what is called bacteriology, and to an even greater extent. For no one is likely to gainsay that such extremely difficult and delicate investigations as those made in the domain of pathology cannot be properly conducted without an intelligent acquaintance with the physiology of parasitic and saprophytic fungi and bacteria, and this being conceded the rest follows as a matter of course.

Yet it is in just this region of special scientific investigation that the grossest sins are committed. It is pitiable to see the wild struggles with facts that have been carried on in the name of bacteriology, and which might have been avoided had the investigators been properly trained in botanical science.

Bacteriology, however, is only one special branch of what is popularly known as the study of germs, and the truth of what has been above stated comes out with yet more startling clearness when we recognize the benefits that have arisen from the study of parasitic fungi and their relations to the diseases of plants. Taking the latter as a special pursuit, it is very difficult to say what should be omitted in a training designed to fit the botanist for investigation. It is only quite lately that pathologists have clearly recognized that the study of the diseases of plants (so important to horticulturists, planters, and foresters) implies by no means a mere acquaintance with the forms of fungi and their

systematic relationships, but that it demands, on the one hand, the most patient and refined researches into the life-history of these organisms, and the variations in their biology due to changes in the environment, and, on the other hand, as deep an insight as can be obtained into the normal physiology of the host-plants, and the variations in this due to changes in the environment. In other words, not only must the investigator attack the question of the mutual relations between parasite and host (and he cannot understand these without studying the normal biology of both), but he must also look into the relations of each to a varying physical environment.

As I said before, it would be hard to say what botanical information can be superfluous in such a training.

But there are other technical pursuits which demand a training in elementary botany, and among these that of the timber merchant, and those of the builder, carpenter, and architect may be grouped together.

It is admitted that these people should understand the nature and properties of timber in the wide sense, and especially of certain kinds of wood in particular. My case is made out quite clearly by the efforts one meets with in various articles and books on timber, designed for the information of those engaged in the trades and professions referred to, and by the lamentable failures in conveying clear instructions, owing to the want of acquaintance with the elements of botanical science.

I maintain that no one can properly understand the markings,

Tabular Résumé of the Various Branches of Botanical Study, as grouped for the preceding argument.

Specialist in morphology — Specialist in pathology of plants — Specialist in physiology

Comparative morphology — Comparative physiology — Specialist in bacteriology and ferments, &c.

Specialist in palæontology (archæology, &c.)

Foresters, planters, farmers, gardeners, fruit-growers, &c.

Timber-merchants, carpenters, architects, builders

Artists, designers, travellers, &c.

Systematic study of comparative anatomy and histology; refined microscopic work, &c., &c. — *University and college life*

Specialist in systematic botany (including geographical botany, origin of species, &c.)

Brewers, bakers, wine-makers, &c. — *Special courses for technical schools, affecting*

Pharmacologists, druggists, tanners, dyers, and others who use oils, resins, &c.

Museum curators and collectors, &c.

Paper makers and those who use fibres

Study of types illustrating life-history, structure, and elementary morphology and physiology; introduction to microscopic work and drawings — *Higher school and college work*

Elementary organography: descriptive and systematic botany, lens in hand; drawings, and field-work — *School boys and girls of about 10 to 14*

Lessons in observation and comparison of the parts of common plants. The pupil to have specimens in hand; the teacher to use black-board — *Adapted for young children*

colour, texture, and other technological peculiarities of timber who is ignorant of its structure; and I have had abundance of proof afforded me of the interest taken in this subject by individuals connected with the numerous callings centred around that of the timber merchant—*e.g.* wood-carvers, turners, cabinet-makers, wheelwrights—as well as by archæologists and geologists, who are brought face to face with problems which require an acquaintance with the structure of timber for their solution.

Now, the structure of timber is a very interesting subject if properly approached, but it is a very complex and hopeless subject for one who is unacquainted with the meaning of the four or five histological elements which compose wood, and of their development from the cambium-cells; and, to comprehend these things, the student should know the elements of botany.

But it is not only the properties of timber that have to be understood by the workers and dealers in wood. An important subject, which is coming more and more to the front, is that of the classification and identification of timbers. It is astonishing how cleverly practical experts can find their way through the difficulties which beset those who have to decide upon the value of timber, and the suitability of different pieces of wood for various purposes; but even more astounding is the vagueness of their replies to the very natural question, How do you decide in difficult cases? One thing is clear—the expert bases his conclusions on keen observations of minute details, and yet these observations are not recorded: the whole system is one of empiricism and blind rule-of-thumb guess-work. It serves the purpose in many cases, just as rough measurements by an expe-

rienced eye and hand are often said to serve the purposes of those concerned at the time; but will anyone doubt that scientific accuracy and system would be more reliable? I am aware that "practical men" doubt this, but repeated contact with "practical men" assures one that they pay a heavy penalty in loss of time for their triumphs.

It is repeatedly observable that the "practical man"—the man of experience, in other words—has to spend long periods of time in the acquirement of his unsystematized powers, and the conviction forces itself upon the observer that he could do much more if he were systematically and logically observant, instead of being merely spasmodically so. In other words, he is scientific in so far as his successes go, for in the end it all resolves itself into keenness of observation and comparison; and he would save himself many failures if he were properly trained. How often is it pointed out that such and such a man is unscientific but practical! Well, this resolves itself into a fallacy, for he is really practical in so far as he is scientific in his methods—clumsily so, it may be, and the science in him has been unconsciously acquired and pursued; but it is there, and it is just where his science breaks down that he becomes a mere bungler. This truth need not blind us to the further one that even a bungler occasionally stumbles upon success, but my argument is that his conclusions would be more constantly trustworthy if he pursued a consistent and recorded course of methodical observation and comparison, instead of trusting to the unsystematized impressions from which his keen mind draws the conclusions of of which he is so vain.

It is, to my thinking, one of the most curious problems of the human mind that "practical men" can persist in upholding empiricism, on the grounds that such knowledge as the above is most real and useful. Of course, it is real and useful in so far as it has been acquired during long years of experience in contact with facts; but look at the opportunities lost in this expensive and wasteful training—at the mistakes made and the wrong lines pursued, until correction comes, sharp and merciless because it involves failure. Surely, a better method is to prepare the man to gain his experience at least cost, and to profit to the utmost by his mistakes; and, when all is done, see the equivocal position the "practical man" is put into—his only real knowledge is scientific, and the wild hypotheses and ignorant fallacies to which he is a slave might have become fruitful thoughts, leading him to far higher attainments had he learnt to observe and record, and compare and judge when he was young. Personally, I know no more contradictory being than the one who prides himself on being a "practical man," and is continually throwing at one's head the adage, "An ounce of practice is worth a ton of theory," for at every turn one finds him involved in endless tangles of error, and his ignorance of this is only equalled by the obstinacy with which he contends the contrary.

The second speaker was Prof. F. W. Oliver, who considered the question of botanical teaching only so far as it bears upon the training of medical students. He argued that, since all scientific medicine is based upon elementary biology, it is necessary to bear in mind that, in a course of say fifty lectures, designed for the requirements chiefly of medical students, some things must be sacrificed in order that certain fundamental truths may be driven home. The only questions are, What must go? and what must be retained? And the reply is that much of the study of types, and of such transcendental subjects as the alternations of generations, and so forth, as found in the schedule of the London University, for instance, should be sacrificed in order that the teacher may concentrate his attention on such parts of the subject as are of real importance and interest to the medical student, and others composing large classes. He would go so far as to say that about thirty out of the fifty lectures should be devoted to the organography and elementary physiology of the higher plants; for in that case the teacher is dealing with beings of which everybody knows something, and there is more human interest to the student when the *facies* of the organism is so familiar as is that of common flowering plants. In conclusion, Prof. Oliver pointed out that the responsibility of these matters rests with the examiners and those who draw up such schedules as that of the London University, and laid some stress on the importance of this responsibility.

Prof. F. O. Bower followed, and directed his remarks chiefly to the subject of teaching mixed and elementary classes in a University. He wished especially to deplore the threatened divorce between morphology and physiology, and advocated that such a divorce should be prevented at all hazards. In regard to this, and to some other points, he must differ from Prof. Marshall Ward's conclusions, though he heartily concurred with most of what he had said. He thought that, taking into account the value of the mental exercise, so useful a study as that of morphology should be introduced early, and that the teaching of the main homologies should be insisted upon. With regard to the cut-and-dried schedules now so universal, Prof. Bower was of opinion that, while they protect the weaker teachers, they hamper the strong ones, and he wished very much that more individual freedom should be allowed to lecturers.

Mr. Forsyth was especially interested in Prof. Marshall Ward's remarks on the teaching of botany to children in schools, and described an experiment now being tried in the Leeds Higher Grade School. The children are being taught to bring plants themselves, and to observe them in the field, and the speaker was of opinion that the new departure is a signal success.

Prof. Green spoke very strongly against the "type-system" as now pursued in the teaching of botany. Not only does it occupy too much time, but it is quite a mistake to begin with an unknown and minute object like the yeast plant: not only is the *Saccharomyces* plant a strange object, but the student obtains no adequate notions of its size or properties. He advocated less section-cutting and less work with the compound microscope, and more observation with the simple lens, at any rate until the student is familiar with common objects.

Prof. Hartog differed from previous speakers in thinking it a mistake to be afraid to teach children technical terms, and pointed out that children take very readily to hard names, and are very proud of having acquired them. He also differed entirely from those who advocate that the fern is a good type to begin with: the fern is a difficult type, abnormal in its phloem, its stomata, and other respects, and should be avoided for some time. He thought it much better to select the various tissues and elements from the first, and then pass on to the study of types.

Prof. Hillhouse agreed with Prof. Marshall Ward that technical terms should be introduced carefully and not too early, and considered that botany has suffered in the past from being regarded as associated with hard words. He also advocated that botany affords the best means for introducing students to the use of the microscope.

Prof. Geddes has often found that schools are detrimental to the observing powers of children, and that the real way to interest the pupils is to let them make discoveries for themselves. He advocated the establishment of a botanical garden for every school, and pointed out that very useful notions of geometry can be taught from flowers. Prof. Geddes objected to the type-system for children, and urged that the life of the plant, and not its destruction, should be the aim of teaching. He would interest students in such subjects as insectivorous plants, and so infuse general interest into their studies.

Prof. Johnson remarked that at South Kensington, the home of the type-system, they have for some years past tried varying the order of teaching the several types, and have found that it is best to work down from the higher to the lower plants.

Prof. Marshall Ward having briefly replied, the discussion was then closed by the President.

THE PRESENT POSITION OF THE HYDRATE THEORY OF SOLUTION.[1]

IT is but four years since this Section devoted a day to the discussion of the nature of solution;[2] since then, however, the general aspect of the question and the position of the advocates of the two rival theories have undergone such a complete change, that in renewing the discussion we shall run but little risk of going over the same ground which we then trod. At Birmingham, Dr. Tilden opened the discussion by passing in review all the well-known and long-known facts which might by any possibility throw some light on the nature of solution, and those who followed him in the discussion each gave the interpretation of these facts which harmonized best with his own views, and, as the facts themselves were susceptible of several different interpretations, the not surprising result followed that

[1] Paper read before Section B, at the Leeds meeting of the British Association, as an introduction to a discussion on the nature of solutions and the theory of osmotic pressure.

[2] B. A. Report, 1886, p. 444.

each disputant departed holding precisely the same opinions which he had brought with him. Since then, however, each party has obtained, or thinks that he has obtained, positive evidence in favour of his own views; evidence which, if upheld, must be accepted as conclusive, or which must be overthrown before his opponents can claim the victory. The supporters of the hydrate theory claim that the curved figures representing the properties of solutions of various strengths show sudden changes of curvature at certain points, which are the same whatever be the property examined, which correspond to the composition of definite hydrates, and which, therefore, can only be explained by the presence of these hydrates in the solutions; while the supporters of the physical theory, now identified with the supporters of the osmotic pressure theory, claim to have shown that, with weak solutions at any rate, the dissolved substance obeys all the laws which are applicable to gases, and that, therefore, its molecules must be uninfluenced by, and uncombined with, those of the solvent.

In another respect also I may notice that our position to-day differs considerably from what it was four years ago; for instead of having to argue the matter out amongst ourselves, as we did then, we are now favoured with the presence of some of those whose work in this very subject has made their names familiar household words with every physicist and chemist throughout the scientific world.

I propose in the first place to give a brief summary of the evidence which has lately been adduced in favour of the hydrate theory, and in the second place to inquire whether the conclusions drawn from this evidence are invalidated by the important facts elucidated by Raoult, van't Hoff, Arrhenius, and Ostwald.

In one respect the supporters of the hydrate theory start now under a distinct advantage—namely, that their most active opponents do not altogether deny the existence of hydrates in solution, although it is only in the case of strong solutions that they will admit their presence; in such solutions, indeed, it is difficult to see how their presence could possibly be denied. The only means which we have of proving that a liquid is a definite compound is by ascertaining whether its composition remains unaltered by its passage through the gaseous or solid condition—by fractionating it by means of distillation or crystallization. With liquids of comparatively small stability, such as hydrates, crystallization is the only method available; the results of crystallization have led us to conclude that the liquid represented by H_2SO_4 is a definite compound, and precisely similar results must force us to accept the definiteness of the liquids $H_2SO_4SO_3$, $H_2SO_4H_2O$, and $H_2SO_44H_2O$: in the case of each of them the liquid freezes as a whole, and without change of composition; the temperature remains constant throughout the solidification, and any excess of either water or sulphuric anhydride which may have been added may be separated from the pure compound, which alone crystallizes from the mixture. Thus, in the instance taken, between the anhydride on the one hand and water on the other, we have four definite compounds, all existing in the liquid condition.

It does not follow, however, that every hydrate which exists in solution can necessarily be obtained in the solid condition; probably no solution, even when it possesses the exact composition of some existing hydrate, consists of that hydrate only, but of a mixture of it with the products of its dissociation (though the amount of these may be very small); and whether the hydrate or one of these dissociation products crystallizes out on cooling must depend on the relative ease with which the bodies in question assume the solid condition; when the hydrate does not crystallize easily we can hope to obtain evidence of its presence by indirect means only.

Mendeleeff's conclusions respecting the densities of solutions of sulphuric acid and alcohol,[1] mistaken though I believe they were, led to the discovery of the means whereby such evidence might be obtained.

He stated that on plotting out the rate of change of the densities with the percentage composition of the solution (the first differential coefficient) he got a series of straight lines, forming figures with well-marked breaks at points corresponding to definite molecular proportions; but on plotting out the experimental points which he said formed these figures, it is impossible to see any justification for this statement; in the case of sulphuric acid the points and Mendeleeff's drawing of them have been given side by side in the Trans. Chem. Soc., 1890, p. 81, and in the case of alcohol they will be found in the *Zeit. f. Phys. Chem.* VI. i. 10. Crompton then showed[1] from an examination of Kohlrausch's values for the electric conductivity of sulphuric acid solutions that a second differentiation might in some cases be necessary before rectilineal figures with breaks in them were obtained. In my own work on various properties of solutions of the acid I have made free use of this process of differentiation, but I have combined it with, and now nearly entirely rely on, an examination of the original curves with the help of a bent ruler.

In the *Phil. Mag.*, 1890, vol. i. p. 430, will be found rough sketches of the figures representing the densities, contraction on formation, electric conductivity, expansion by heat, heat of dissolution, and heat capacity of the solutions, and in the Trans. Chem. Soc., 1890, p. 338, that representing the freezing-points. In some cases, such as the freezing-points of solutions near 58 and 100 per cent. strength, a mere inpection of the figure enables us to locate the position of abrupt changes of curvature; in general, however, the recognition of such changes is more difficult. On attempting to draw any of these figures with the help of a bent ruler it was found that the whole figure could only be drawn in several sections, and it was also found that each section thus drawn consisted of a single curve of a parabolic nature, although a ruler, when bent by the pressure exerted by the two hands, by no means necessarily forms a parabola; and moreover—and this is the most important part of the evidence—it was found that these figures, though differing so greatly in their general appearance, all split up into the *same* number of sections, indicating the existence of changes of curvature at the *same* points; and, further still, these points corresponded to solutions of definite molecular composition in all cases where the ratio of the acid to the water was sufficiently large to render any such comparison possible; the average difference between the composition indicated by the changes of curvature and that of definite hydrates was only $0{\cdot}057H_2O$. With weak solutions it is, of course, impossible to assert that the changes occur at definite molecular proportions, owing to the smallness of the change in percentage composition which would be caused by an additional molecule of water to each H_2SO_4; but the changes with these weak solutions are of precisely the same character as those with strong solutions, and, unless some strong evidence to the contrary be forthcoming, we must attribute them to the same cause.

To discuss fully the value of the evidence thus obtained would take me more hours than I can now afford minutes; but I think that I may say that these results stand at present unquestioned and uncontroverted, and that unless they can be controverted we must accept the presence of hydrates in solution as having been proved. I may also add that my results with sulphuric acid solutions have been strengthened by obtaining analogous results with solutions of several other substances: that one of the hydrates indicated by them has been proved to exist by isolating it in the crystalline condition: and lastly, that a law governing the freezing-points of solutions has been formulated, according to which we can calculate within experimental error the freezing-point of any solution, whatever its strength may be, provided we acknowledge the existence of every hydrate which my work has indicated; whereas, if we deny the existence of these, the freezing-points calculated according to this or any other law show such divergences from the found values that all semblance of agreement disappears. I am indeed labouring under no small disadvantage in attempting to support the hydrate theory when the greater part of the evidence existing in favour of it is as yet unpublished.

Before proceeding to the second part of my subject I wish to draw attention to the great complexity of some of the hydrates which my work has indicated, as well as to the fact that the indications of sudden changes are nowhere more marked than they are with these very weak solutions. The changes, which are observed in the heat of dissolution curve from 5 per cent. downwards,[2] afford a good illustration of this latter fact; or, again, the freezing-points of weak solutions may be instanced,[3] where the rate of fall from 0 to 0·07 per cent. is a quarter as great again as it is from 0·07 to 1·0 per cent. The complexity of the hydrates indicated is so great that in the extreme cases they must be represented as containing several thousand H_2O molecules, and the suggestion of such complexity will no doubt prejudice many against my conclusions in general; though on what grounds I know not, for we are entirely in ignorance at

[1] *Zeit. f. Phys. Chem.*, i. p. 275; Chem. Soc. Trans., 1887, p. 778.

[1] Chem. Soc. Trans., 1888, p. 116.
[2] *Ibid.*, 1890, p. 107.
[3] *Ibid.*, p. 343.

present as to the possible complexity of liquid molecules. It is interesting to note that a similar complexity of molecular grouping must be admitted if we accept Raoult's original statement that one molecule of any substance dissolved in 100 molecules of a solvent lowers the freezing-point of this latter by about 0°·63; for, if this be so, we must assign to the molecules of the various substances entered in the second column of Table I. the magnitude there indicated when they are dissolved in the solvent named in the first column, for it requires that proportion of these bodies to lower the freezing-point of 100 molecules of the solvent by 0°·63; and, amongst these few instances which I have collected from my own determinations, we find molecular aggregates containing as many as 200 of the fundamental molecules, and even this number, I may mention, probably understates the complexity to a very considerable extent; for the depression in this and some of the other cases had to be estimated from that observed with solutions containing as much as 10 gram molecular proportions to 100 of the solvent, and the molecular depression increased rapidly with the strength of the solution: $1000H_2O$ would probably be a low estimate of the complexity of the molecules of water when dissolved in a large excess of the hexhydrate of calcium chloride, a complexity comparable with that of the hydrates, which my other work has indicated, and that too in the case of that very substance which these hydrates contain—water.

TABLE I.—*Molecular Weights of Substances in Various Solvents.*[1]

Solvent.		Dissolved substance producing 0°·63 depression.[2]
$100H_2SO_4H_2O$	... ...	$32H_2O$
,,	... ...	$63H_2SO_4$
$100H_2SO_44H_2O$	... ...	$8H_2O$
,,	... ...	$15H_2SO_4$
$100(CaNO_3)_24H_2O$	... ...	$90H_2O$
,,	... ...	$42Ca(NO_3)_2$
$100CaCl_26H_2O$	... ...	$210H_2O$
,,	... ...	$63CaCl_2$

Now as to the question of how far the theory of osmotic pressure, and the results on which it is based, are antagonistic to the hydrate theory: and let me first define clearly the position which I take in this matter. I do not for one moment call in question any of Raoult's classical work, which is now so familiar to us, nor do I question that these results reveal the existence of a depression of the freezing-point which is approximately and generally constant; and I consequently admit that we can generally obtain an approximately correct value for the molecular weight of the substance by observing the depression which it causes; nor, again, do I wish to question the correctness of the mathematical relationship which van't Hoff and Arrhenius have shown to exist between osmotic pressure, the lowering of the freezing-point, and other properties, provided we accept the fundamental assumptions on which their calculations are based—the truly gaseous nature of dissolved matter, and the dissociation of salts into their ions. But what I do question is that the facts of the case warrant such assumptions, and that the constancy and regularity of the results are so rigorous as to justify the conclusion that the solvent has no action on the dissolved substance, and that there are no irregularities such as would be caused by the presence of hydrates.

According to the osmotic pressure theory, the dissolved matter, so long, at any rate, as it is not present in greater quantity than it would be in the same volume of its gas, if it were gasified under normal conditions, is really in the gaseous condition, and obeys all those laws which apply to gases. According to the hydrate theory this will be but partially true. That the dissolved substance is in a condition comparable with that of a gas, in so far as the separation of its own particles from each other is concerned, must be admitted—indeed, I arrived independently at this same conclusion from a study of thermochemical data; but inasmuch as there is present the solvent, which we believe is *not* an inactive medium, its molecules cannot have the same freedom as if they were truly gaseous, and will, therefore, obey the laws of gases imperfectly only.

It will be well to confine our attention to but one of those properties connected with osmotic pressure, and to select for that purpose the one which has been most fully investigated—the lowering of the freezing-point of a solvent: and the tests which may be applied to assertain whether in producing this lowering the dissolved substance behaves as a perfect gas or not, may be grouped under three principal headings:—

1. Is the molecular depression (*i.e.* that produced as calculated for one molecule dissolved in 100 molecules) constant, independent of the nature of the solvent?

2. Is it independent of the strength of the solution, so long as this strength does not exceed the limits ("gas" strength) above mentioned? (Boyle's law.)

3. Is it independent of the nature of the dissolved substance? (Avogadro's law.)

In the *Phil. Mag.*, 1890, vol. i. p. 495, will be found instances of the variation in the molecular depression which may be noticed by altering the solvent (see also Table I. above). With water in six different solvents it varied between 1°·072 and 0°·003; with sulphuric acid in four different solvents, between 2°·15 and 0°·01; with calcium chloride in two different solvents, from 2°·773 to 0°·01; and with calcium nitrate in two solvents, from 2°·5 to 0°·015; while many instances may be collected from Raoult's data showing that the same substance which acts normally in one solvent may act abnormally (give only half the usual depression) in another. Such variations are so great—from 100 to 35,600 per cent.—that there can be no doubt but that the solvent is *not* that inert medium which the supporters of the physical theory would have it to be, but that it has a very great influence on the results obtained. It must be noted, however, that this objection, though applying to Raoult's original views, does not, or, at any rate, may not, apply to van't Hoff's theory, for according to this theory the nature of the solvent *has* an influence in determining the lowering of the freezing-point, W, in van't Hoff's equation, $\delta t = \frac{0{\cdot}02T^2}{W}$, representing the heat of fusion of the solvent. But the lowering is according to this equation independent of the nature or the amount of the dissolved substance, so that the two following objections will apply to van't Hoff's theory as well as to Raoult's statement.

Secondly, as to the influence of the strength of the solution. It is remarkable that, although the osmotic pressure theory depends on the behaviour of solutions below a certain strength, no attempt whatever has been made by its supporters to obtain any data respecting such solutions. The data on which their views were founded referred to solutions considerably stronger than the requisite "gas" strength, and though, no doubt, it was convenient to work with data which afforded a ready excuse for any awkard irregularities which might be met with, such data must lack the conclusiveness which is so eminently desirable. The few data which I have accumulated as to solutions of an "ideal" strength can leave no doubt that, even in their case, the depression is not a constant independent of the strength.

A solution of sulphuric acid containing $0{\cdot}08H_2SO_4$, $100H_2O$ would be of a strength comparable with the gas from the acid if it could be gasified at normal pressure and temperature, and the molecular depression should be constant for all solutions below this strength: it should be represented by a horizontal line such as AB in Fig. 1, whereas the observed deviations from constancy are very great, being represented by the lines marked H_2SO_4; and, moreover, these deviations are by no means regular, and cannot therefore be attributed to imperfect gasification; they possess none of the characteristics of the deviations of gases from Boyle's law. The determinations on which these results are based are very numerous; there are about sixty experimental points on the portion here shown, and the mean error of each point as determined in two different ways was only 0°·0005, a quantity represented by one-tenth of one of the divisions of the paper; the deviations from regularity amount to thirteen times this quantity, and to as much as 16 per cent. of the total depression measured.

The other lines in Fig. 1 represent the deviations from regularity in the case of calcium chloride, calcium nitrate, and alcohol respectively, and these, though they are smaller than in the case of sulphuric acid, are far too great to be attributed to experimental error; and the fact that they occur sometimes in one direction, sometimes in the other, precludes the possibility of attributing them to any constant source of error in the instruments used or in the method adopted.

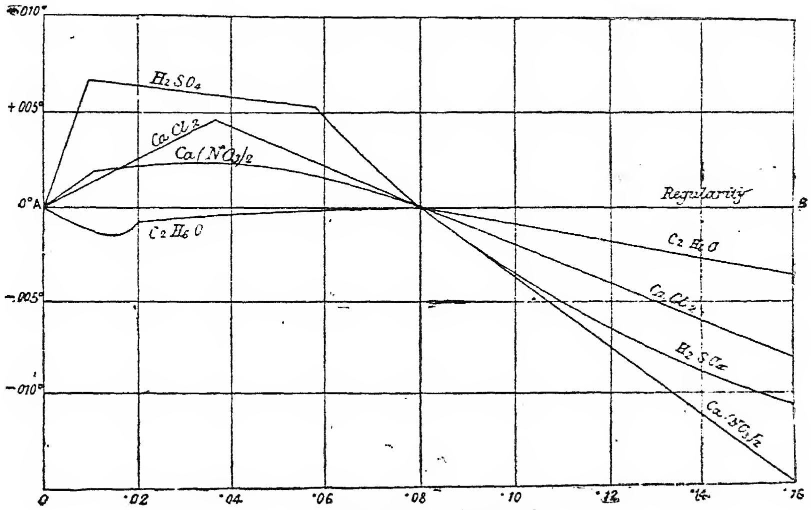
Remembering that these are the only data which we have at present respecting very weak solutions, we must conclude that the hypothesis that such solutions exhibit perfect regularity is

[1] Other instances of high molecular weights are mentioned by Brown and Morris (Chem. Soc. Trans., 1888), and Gladstone and Hibbert (*Phil. Mag.*, 1889, vol. ii. p. 38).

[2] Determined from the freezing-points of very weak solutions.

wholly untenable; and it must be specially noticed that one of the substances showing these irregularities—alcohol—is a non-electrolyte, in which case the theory of dissociation into ions cannot be brought forward as an explanation of their existence.

It is important to observe that when we pass on to stronger solutions, where the actual magnitude of the deviations becomes so great that they would be revealed by the roughest experiments—deviations of even 70°—and where, I believe, even the supporters of the osmotic pressure theory would not hesitate to attribute them to the disturbing influence of hydrates; these deviations occur in precisely the same irregular manner as they do in the case of weak solutions, and must evidently be attributed to the same cause. The results with alcohol given in Fig. 2 illustrate these irregularities in a very striking manner. It must also be pointed out that, apart from the irregularity of these deviations, their very direction shows that they cannot be attributed to the



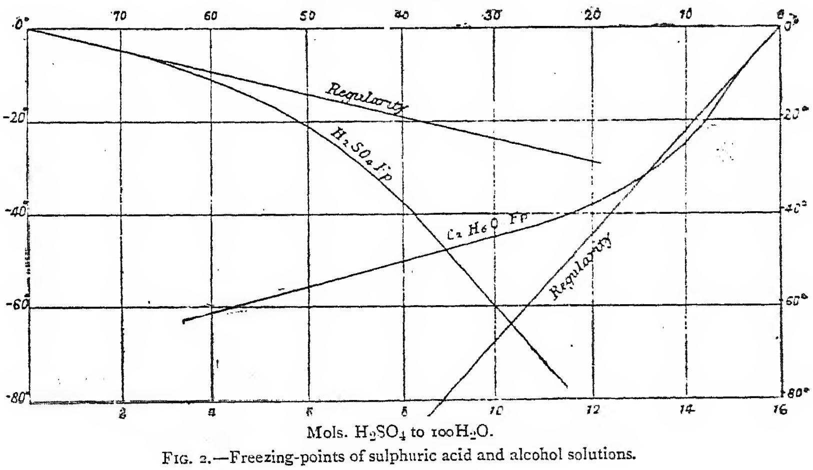
Molecules dissolved in $100H_2O$.

FIG. 1.—Deviation from regularity of the freezing-points of very weak solutions.

dissolved particles being brought within the sphere of each other's attraction, as in the case of the deviation of gases from Boyle's law, for the result of this would be that their attraction on the particles of the solvent would be diminished, and the freezing-point of this latter would consequently be lowered to an abnormally small extent, whereas precisely the reverse is the case in nearly every instance at present investigated: the freezing-points of strong solutions are abnormally *low*. Various instances of this will be found in the *Phil. Mag.*, 1890, vol. i. p. 500, that of sulphuric acid, which is illustrated here in Fig. 2, being by no means the most prominent; while the case of alcohol, now for the first time displayed (Fig. 2), is the only exception which has, so far, been met with, and that is an exception only in the case of excessively strong solutions.

From the instances above mentioned some answer may be obtained to the third question—whether the molecular depression

Mols. $(C_2H_6O)_2$ to $100H_2O$.



Mols. H_2SO_4 to $100H_2O$.

FIG. 2.—Freezing-points of sulphuric acid and alcohol solutions.

is independent of the nature of the dissolved substance. The values obtained with these four substances, taking solutions of a strength corresponding to that of their gases, are:—

Calcium chloride	2°·850
Calcium nitrate	2°·744
Sulphuric acid	2°·313
Alcohol	2°·180

a variation of 30 per cent., which must give an emphatic denial to the idea of absolute constancy; and if we take instances from other substances, where the data available refer to solutions of somewhat greater strength, we find that the very substances on which the idea of constancy was originally founded show variations reaching 60 per cent. (*Phil. Mag.*, 1890, vol. i. p. 492), while in other cases, which I have quoted elsewhere (*loc. cit.*, p. 493),[1] the variation attains the still larger dimensions of 260 per cent.

[1] The depression produced by H_2O in $100H_2SO_4$ is 1°·07 instead of 0°·07 as there given.

To every one, therefore, of the three test questions as to constancy and regularity, the experimental results give an unhesitating negative.

In the instances quoted above the depression actually found for alcohol has been doubled in order to simplify the comparison of it with the other substances. Alcohol belongs to that class of bodies which give just half the value in water that the majority do, and of which there are some instances in the case of every solvent yet examined. The explanations which the supporters of the chemical and physical theories give of these half values differ so radically from each other that it is hopeless to attempt to arrive at any agreement as to the nature of solution till this difference is settled. The chemists say that these half values are in all cases the abnormal ones, just as Raoult did originally, and explain them by representing the molecules of the dissolved substances which give them to consist of two fundamental molecules. The physicists give exactly the same explanation in the case of every solvent except water, but in this case they say that the smaller values are the normal ones and the larger the abnormal, the double magnitude of these being caused by the dissociation of the dissolved molecule into its two ions, whereby two molecules or acting units are formed from every one originally added.

If Raoult's views as to the constancy of the molecular depression can be maintained, the data themselves are conclusive against making this exception in the case of water; for, since the substances which give the lower values are supposed to act normally, it is evident that, if the values given are in any way abnormal, this abnormality must be due to the solvent. Now the values certainly *are* abnormal; they are about $1^\circ{\cdot}03$, whereas the normal value for one molecule dissolved in 100 molecules of other solvents is $0^\circ{\cdot}63$, and the excess can, therefore, only be explained by assuming that the molecules of water are more complex than those of other solvents in the proportion of $1{\cdot}03$ to $0{\cdot}63$, or $1\frac{1}{2}$ to 1; in other words, the water molecules must be $1\frac{1}{2}H_2O$. This view cannot be reconciled with the atomic theory.

Indeed the theory of dissociation into ions is altogether unintelligible to the majority of chemists. It seems to be quite irreconcilable with our ideas of the relative stability of various bodies, and with the principle of the conservation of energy. Of course we know that each ion when dissociated is not supposed to be permanently dissociated, but to be continually combining with its neighbours and separating again from them as in every other case of dissociation; but at any particular moment a very large proportion of them is supposed to be free; a proportion which, according to the very results under discussion, must be very nearly, if not quite, 100 per cent. of the whole; and we have to settle whether it is probable or possible that a decomposition such as this could have been effected by introducing the compound into water. And how can we regard it probable that compounds of such stability and compounds formed with such a development of heat as sulphuric or hydrochloric acid should be thus entirely dissociated by water; still less that these, and all the most stable compounds which we know, should be thus demolished, while all the less stable ones—such as hydrocyanic, sulphurous, boric acids, &c.—remain intact? How can we admit that the more stable a body is, the more prone it is to be dissociated?

And if such a dissociation has occurred it must have been without any absorption of heat, and, consequently, energy must actually have been created. Take one of the simplest instances, that of hydrochloric acid. If anything at all is certain about atoms, it is that the atoms in an elementary molecule are united very firmly together, and that therefore in separating them a very large absorption of heat would occur. To separate 2HCl into 2H and 2Cl would absorb far more than the 44,000 cal. which we know are absorbed in separating 2HCl into H_2 and Cl_2. Yet the supporters of the dissociation theory would have us believe that this separation has actually taken place, not only without any absorption of heat, but actually with a development of 34,630 cal.; that is, that $44{,}000 + 34{,}630 + x$ cal. have been *created*, and that too through the intervention of the water, which has *ex hypothesi* no action whatever.

This difficulty is realized by the supporters of the physical theory, but the way in which they meet it does not appear to me in any way to overcome it. To explain the non-absorption of heat in the dissociation of the salt, they suppose that a charge of electricity combines with the liberated atoms, and, in doing so, evolves an amount of heat exactly equivalent to that absorbed in the separation of the atoms from each other; and a later development of the theory is, I believe, that the atoms, though separated, are still held together by means of these charges, so that the net result is the supplanting of the chemical bond by an electrical bond of a precisely similar value. It appears to me that nothing substantial is gained by such a substitution, and that its occurrence is not merely hypothetical but impossible. Whence come these electric charges, and by what agency are they brought into play? On what grounds can it be maintained that a charge can combine with matter so as to evolve heat, and that the heat so liberated is always exactly equal to that absorbed in the decomposition of the compound? If this equality exists, how can we account for the force which develops the one overcoming the *equal* force which develops the other? and how, again, can we account for the heat developed in the act of dissolving? If, on the other hand, the heat of the combination of these charges is supposed to be equivalent to the heat of combination of the atoms *plus* that of the heat of dissolution, we are met by the objection that the latter is often negative, and that, therefore, the heat of the combination of the charges must often be less than that of the combination of the atoms and molecules, so that the lesser force must be regarded as overcoming the greater.

That free ions exist in solution is supposed to have been proved by a recent observation of Ostwald's, to the effect that the ions may be separated and brought into different parts of the liquid by the proximity of a charged body. The separation of the ions is, of course, recognized by the subsequent liberation of hydrogen, oxygen, acid, alkali, &c., and it is certain that on allowing these to mix and combine heat will be developed, and the salt solution re-formed; and thus, by replacing and removing the charged body, it would evidently be possible to produce an unlimited amount of heat. Now, if the charged body has lost none of its charge, and if no mechanical energy has been expended, this heat must have been produced out of nothing, and the whole ground-work of physical science must be false; whereas, if energy in some form has been expended on the solution, the experiment proves nothing, for there is nothing to show that this energy has not been utilized in bringing about the very dissociation the previous existence of which was in question.

I have already shown that the experimental data prove the absence of that constancy and regularity which ought to exist according to the physical theory, and to place the hydrate theory on unassailable grounds it is only necessary to show that deviations from constancy and regularity are of a magnitude such as might reasonably be assigned to deviations due to the presence of hydrates. That variations of 260 and 36,000 per cent. in the value of the depression—such as are observed by altering the dissolved substance or the solvent respectively—are amply sufficient to satisfy the most exalted views of the influence of chemical attraction, requires, I think, no demonstration, and we may therefore content ourselves with examining the deviations observed when the proportions of the solvent are altered—such deviations as are illustrated in Fig. 1.

It cannot be maintained that the energy of the chemical combination of, say, water with sulphuric acid, is the only reason why the temperature of the mixture of the two must be cooled below 0° before any of the latter will crystallize out; some lowering of the freezing-point will be caused by the mere interposition of the foreign molecules of sulphuric acid between those of the water, and on certain grounds, which I have explained elsewhere,[2] I estimate this mechanical lowering, as I term it, at $0^\circ{\cdot}56$ for each dissolved molecule to 100 of the solvent (a molecule of solvent water being $3H_2O$), a value which, it may be noted, is not far removed from Raoult's experimental value of $0^\circ{\cdot}63$. There is also another source of lowering depending mainly on the heat capacities of the substances concerned, which I term for convenience the physical lowering; but its value, in the case of weak solutions, is very small, and I need, therefore, say no more about it here. Both these lowering causes would exist whether there were hydrates present or not; but if these were present we should get a further depression due to their existence. Any given hydrate would have to be decomposed into the next lower one before it could give up any water for crystallization, and a certain amount of resistance would thus be offered to this crystallization, to overcome which the solution would have to be further cooled.

[1] On the view that hydrates exist in solution, there is no difficulty, as I have shown elsewhere, in explaining the absorption of heat during dissolution, without violating the principle of the conservation of energy.

[2] Proc. Chem. Soc., 1889, p. 149.

The necessary cooling may be estimated in the following way: Supposing the solution to be a mixture, and to be cooled below its normal freezing-point, then, on solidification, the temperature would rise to this point, but if this solidification involved a chemical decomposition which absorbed x cal., the rise of temperature would be thereby reduced, the reduction thus caused amounting to $x \div$ the heat capacity of the solution. As the heat absorbed in the decomposition of the various hydrates of sulphuric acid is known, we can calculate the lowering produced by their presence.

TABLE II.—*Freezing-Points of Solutions of Sulphuric Acid.*

I. Per cent. H_2SO_4.	Calculated.				IV. Found F. p.	Next hydrate.	
	II. Mech.	III. Phys.	IV. Chem.	V. Total.		VII. Calc.	VIII. Found.
						Per cent.	Per cent.
0·068	0·0209	0·0	0·0110	0·0347 [1]	0·0354	0·37	0·36
0·362	0·1114	0·0004	0·0248	0·1508 [1]	0·1582	1·43	1·05
1·06	0·3275	0·0044	0·0589	0·4314 [1]	0·4272	3·54	4·02
4·02	1·285	0·071	0·077	1·582 [1]	1·59	8·40	8·59
8·59	2·879	0·388	0·189	3·815 [1]	3·80	18·17	18·49
18·49	6·96	3·23	1·59	11·78	11·83	29·7	29·5
29·53	12 85	18·82	3·50	34·17	34·00	37 5	37·7

In Cols. II., III., and IV., I have given the depression due to the three above-mentioned causes in the case of certain solutions, Col. V. containing their sum; and it will be seen what a small proportion of this total lowering can be attributed to purely chemical causes. With most solutions it does not exceed 10 per cent. of the total, and with weak solutions, such as are generally used in freezing-point determinations—say 5 per cent.—it amounts to considerably less than 0°·1; this, too, in the case of sulphuric acid, where the heat of formation of the higher hydrates is greater than with any other known substance.

The reason, therefore, why the deviations from constancy are so small as to have escaped detection hitherto, and the reason why solutions behave almost as if their chemical nature was non-existent, becomes apparent; but this near approach to constancy and regularity, instead of proving the correctness of the physical theory and giving a death-blow to the chemical theory, is really one of the strongest arguments which can be adduced in favour of the latter. If the hydrate theory is right, the influence of hydrates must often be nearly inappreciable.

But it is not only a general concordance between the found and calculated magnitude of the irregularities which the hydrate theory is capable of affording, but a concordance so exact that the precise value of the deviation at any point may be calculated. In Col. VI. of Table II. are given the observed freezing-points of the solutions, and these show an average difference of but 0°·004 for the three weaker solutions, and 0°·06 for the four stronger solutions, from those calculated (Col. V.). The last two columns exhibit this concordance in a different manner; from the observed freezing-point we can calculate the composition of the hydrates which must exist in the solution (Col. VII.), and these are found to agree so fully with those indicated by the examination of the curved figures representing various properties of the solution (Col. VIII.) that the maximum difference between the two is only 0·48 in the percentage of acid present.

When we can by simple calculations, based on one series of determinations, prove that the hydrates in solution must be the same as those which totally independent experiments have led us to suppose, we have, I think, arrived at proof as nearly absolute as it is possible to conceive; and, if I have succeeded in showing that this proof may be accepted without in any way rejecting the facts on which the advocates of the osmotic pressure theory rely—approximate constancy, approximate regularity, and approximate similarity between dissolved and gaseous matter—I shall feel that I have done far better work than the mere establishment of the hydrate theory, by pointing out a possible *modus vivendi* for both theories almost in their entirety, and by helping to break down that wall of separation between physicists and chemists which is fast crumbling into dust.

SPENCER UMFREVILLE PICKERING.

[1] The actual total has been increased by 10·4 per cent. of its value to give the figures quoted in these five cases, for reasons which will be given elsewhere. Some of the numbers in this table may be subject to slight corrections, as they have been quoted in the absence of the original calculations.

A TEACHING UNIVERSITY FOR LONDON.

THE following letter has been addressed to the Lord President of the Privy Council:—

MY LORD,—We, the undersigned, the President of University College, London, and the Principal of King's College, London, beg leave to address your Lordship in reference to the joint petition from the Councils of our two Colleges for the incorporation of a Teaching University in London, which has for some time been before the Privy Council. Your Lordship had the goodness to receive a deputation from the Councils of our two Colleges in July 1889; and your Lordship then intimated your judgment that the University of London should be allowed a reasonable time in which to propose a new charter in accordance with the recommendations of the Royal Commission on the question of a Teaching University in London. In obedience to this intimation from your Lordship, our Councils have, at the request of the Senate, entered into negotiation with them, and have consented, subject to the satisfactory settlement of some points affecting the Faculties of Law and Medicine, to a scheme for our union with the University, embodying a separate system of graduation for our students in the Faculties of Arts and Science. We desire that power should be reserved in certain events to make similar arrangements in regard to the Faculty of Law. With respect to medicine, the Senate have stipulated that they should be at liberty to make different arrangements, separately from our Colleges; and in the absence of opportunities for conference with the other institutions specially interested in this Faculty, we have not thought fit on this ground to break off the negotiations; but we reserve power to reconsider our position, if arrangements are contemplated by which it would be seriously affected. We claim, further, as essential to the efficiency of our teaching in science, that our medical students, for the purpose of their examination in pure science, known as the "Preliminary Scientific Examination," shall be considered as belonging to the Faculty of Science on the teaching side of the University, and not to a separate Faculty of Medicine.

Having been informed that urgent protests are raised by University Colleges in the country, particularly at Birmingham, against influence being given to London Colleges in the Senate while they are excluded, we beg to remind your Lordship that the amalgamation of the proposed Teaching University for London with the existing University was not our proposal, but has been, thus far, accepted by us in deference to the principal Report of the Royal Commissioners. We consider that, if this amalgamation is effected, we are entitled to a representation on the governing body of the reconstituted University proportionate to our concern in University teaching for London, considered as one of its two spheres of work; and that the nature of the case does not admit of a similar effective representation of institutions elsewhere. If this reconstitution of the existing University should be found, by reason of such opposition, or for any other reason, impracticable, we desire to be replaced in our original position, as petitioners for the establishment in London of a Teaching University upon the lines of our petition presented in 1887, and of the draft charter thereto appended, to which, in that case, we still respectfully adhere.

We have the honour to remain, your Lordship's obedient humble servants,

JOHN ERIC ERICHSEN,
President of University College, London.
HENRY WACE,
Principal of King's College, London.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The election of a Professor of Mechanism and Applied Mechanics, in succession to Prof. James Stuart, will take place on November 12. The names and testimonials of candidates are to be sent to the Vice-Chancellor by Saturday, November 8. The electors are the Vice-Chancellor, Mr. W. Airy, Dr. Besant, Sir F. J. Bramwell, Dr. Cayley, Mr. H. Darwin, Mr. Martin, Dr. Phear, and Lord Rayleigh. The stipend is £700. The Senate has approved a new scheme for the management of the department, under which the Professor is directly responsible for the carrying on of the workshops.

Mr. Chaplin, the President of the Board of Trade, has proposed to the Chancellor that the University should undertake

the systematic education of students of agriculture. The question of funds stands in the way, but a syndicate is to be appointed to consider the question, and it is hoped that by a subvention from the County Councils, or by private benefaction, means may be found for the formation of an agricultural department.

Mr. Wynter Blyth and Dr. Ransome have been appointed additional examiners in Sanitary Science. Between fifty and sixty candidates presented themselves for examination, of whom about forty satisfied the examiners, and have received the University diploma in Public Health.

Mr. J. G. Adami, of Christ's College, has been elected to the John Lucas Walker Studentship in Pathology, in succession to Dr. William Hunter, of St. John's College.

Mr. E. Lloyd Jones has been appointed Demonstrator of Pathology in succession to Mr. Adami, resigned.

Mr. L. R. Wilberforce, of Trinity College, has been appointed Demonstrator of Physics, in succession to Mr. F. Newall, resigned.

The honorary degree of M.A. has been conferred on Dr. Joseph Griffiths, Assistant to the Professor of Surgery, and Pathologist to Addenbrooke's Hospital.

Dr. Donald MacAlister, of St. John's College, has been appointed Assessor to the Regius Professor of Physic.

The following have been nominated as Examiners in Natural Science:—Physics: Prof. Carey Foster, F.R.S., and R. T. Glazebrook, F.R.S. Elementary Physics: Prof. J. J. Thomson, F.R.S., and L. R. Wilberforce. Chemistry: Prof. Liveing, F.R.S., and Prof. Emerson Reynolds, F.R.S. Elementary Chemistry: M. M. Pattison Muir and Dr. Ruhemann. Geology: Prof. A. H. Green, F.R.S., and J. E. Marr. Botany: Prof. D. H. Scott and Prof. J. R. Green. Zoology: Prof. Ray Lankester, F.R.S., and A. E. Shipley. Elementary Biology: Prof. Marshall Ward, F.R.S., and A. Sedgwick, F.R.S. Anatomy: Prof. Macalister, F.R.S., and Prof. Windle. Physiology: L. E. Shore and C. S. Sherrington. Pharmaceutical Chemistry: H. Robinson and E. H. Acton.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 13.—M. Hermite in the chair.—M. Tisserand presented the second volume of his "Traité de Mécanique Céleste," and noted that it deals principally with two subjects—viz. the figure of celestial bodies, and their movement of rotation.—Presentation of the fifth volume of the "Bulletin du Comité international de la Carte du Ciel"; state of progress of preliminary works, by Admiral Mouchez.—On a photograph of the Ring Nebula in Lyra, obtained at Algiers Observatory, by the same author.—On a photograph obtained with a nine hours' exposure at Toulouse Observatory, by M. B. Baillaud. (For the three above communications, see Our Astronomical Column.)—Observation of D'Arrest's comet (rediscovered by Mr. Barnard on October 6, 1890) made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. The observation for position was made on October 10.—On the linear equations from partial derivatives, by M. A. Petot.—Vibrations of a platinum wire rendered incandescent by an electric current, under the influence of successive interruptions of this current, by M. T. Argyropoulos. The author has stretched horizontally a platinum wire, 0·70 metre long and less than a millimetre in diameter, and has raised it almost to white heat by means of an electric current. By inserting a commutator in the circuit, the wire immediately vibrated, and became subdivided into a series of waves having well-marked ventral segments and nodes. The number of segments was augmented by very slowly decreasing the tension of the wire. On increasing the tension the number was diminished until the incandescent wire vibrated transversely with a single ventral segment at the middle.—Combinations of cyanide of mercury with lithium salts, by M. Raoul Varet. The following compounds have been prepared: (1) an iodocyanide of mercury and lithium, having the composition $HgCy_2$, $2LiCy$, HgI_2, $7H_2O$; (2) a bromocyanide of the same metals, for which the formula $2HgCy_2$, $2LiBr$, $7H_2O$ is given; (3) a chlorocyanide of mercury and lithium, of doubtful composition.—Researches as to the best conditions for the preparation of mono-isobutylamine in quantity, by M. H. Malbot.—On a general process for the synthesis of β-ketonic ethers and nitriles, by M. L. Bouveault. The author gives the most general method for the formation of β-ketonic nitriles, and shows that these bodies may readily be transformed into the corresponding ethers. The method is given in sufficient detail, and several examples of its application shown.—Upon the presence and the disposition of trehalose in mushrooms, by M. Em. Bourquelot.—On the lateral nerve of Cyclopteridæ, by M. Frédéric Guitel.—Physiological researches on floral envelopes, by M. Georges Curtel. It is concluded that (1) the flower possesses energetic respiratory and transpiratory functions, superior in general to those of the leaf of the same plant; (2) the assimilation is generally feeble, and cloaked or much diminished by the very intense respiration; (3) the volumetric proportion of carbon dioxide emitted to oxygen absorbed is always small, and less than unity.—On the porphyritic eruptions of Jersey, by M. A. de Lapparent.

SYDNEY.

Royal Society of New South Wales, August 6.—Dr. Leibius, President, in the chair.—Seven new members were elected.—A letter was read from the Committee appointed by the Victorian branch of the Royal Geographical Society of Australasia and the Royal Society of Victoria conjointly, inviting the co-operation of the Royal Society of New South Wales in carrying out the proposed Swedish-Australian expedition to the Antarctic Regions, and stating that Barons Nordenskiöld and Oscar Dickson had promised to defray half the cost of the expedition, providing an equal amount (£5000) was raised in the colonies.—The following papers were read:—On the theory of repetition measures of angles with theodolites, by G. H. Knibbs.—Record of hitherto undescribed plants from Arnheim's Land (part ii.), by Baron Ferd. von Mueller, K.C.M.G., F.R.S.—On the Australian aborigines, varieties of food and methods of obtaining it, by W. T. Wyndham.—On some photographs of the Milky Way, recently taken at the Sydney Observatory, by H. C. Russell, F.R.S.

September 3.—Dr. Leibius, President, in the chair.—The following papers were read:—Record of hitherto undescribed plants from Arnheim's Land (part iii.), by Baron Ferd. von Mueller.—On the application of the results of testing Australian timbers to the design and construction of timber structures, by Prof. Warren.—Exhibits: Enlargement of photograph of a negative of Fresnel's interference bands, for lecture purposes, by Prof. Threlfall; Edison's latest perfected phonograph, by C. L. Garland.

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THURSDAY, OCTOBER 30, 1890.

SEEBOHM'S "BIRDS OF JAPAN."

The Birds of the Japanese Empire. By Henry Seebohm. Pp. i.-xxiv., 1-386. (London: R. W. Porter, 1890.)

MR. SEEBOHM'S work on the ornithology of Japan is sure to be welcome to naturalists, as it is always useful to have the avifauna of any country or group of islands monographed and historically brought up to date. In his latest work Mr. Seebohm has incorporated all the results obtained by recent explorers in Japan, and we have now a very fair idea of the birds of this portion of Eastern Asia. The map which accompanies the volume also helps to a better understanding of the relations of Japanese ornithology with those of the adjacent countries. Mr. Seebohm has further made use of the present work to amplify and expound his recently-published "Classification of Birds," so that the work contains his latest views on this important subject. In his original work he gave two schemes of arrangement, giving a preference to the second or "alternative" one. He appears now to have changed his mind, and to have reverted to his original idea (" Classif. B.," p. vii.), with this important modification, that he now places his Coraceiiformes after his Pico-Passeres, ending with Mimogypes (American Turkey Vultures), which lead from the Ground Hornbills (*Bucorax*), and are followed by the Sub-class Falconiformes. There is no doubt that this is a great gain in idea, and we are glad to see that Mr. Seebohm is modifying his first notion, that it is absolutely advisable to reduce the orders of birds to a small number of Sub-Classes. We are of opinion that a still further increase in the number of Orders will have to take place before the scheme works to the satisfaction of ornithologists.

The exigencies of arranging the Passerine Birds in the present work, or at least the bulk of the Palæarctic genera, have obliged Mr. Seebohm to declare himself on the subject of their classification, and this is his declaration:—"With some slight modifications, I have adopted that defined by Mr. Oates ('Fauna of British India: Birds,' i., p. 8), which seems to me to be a distinct advance upon previous arrangements." Mr. Seebohm is under a misapprehension here, unless we allow that his "slight modifications" are intended to entirely subvert Mr. Oates's arrangement by turning it topsy-turvy—a new method of appreciation. The latter gentleman begins with the *Corvidæ*, Mr. Seebohm with the *Turdinæ*, not one of Mr. Oates's families being allowed full rank, but all of them relegated to the position of Sub-families in the family *Passeridæ!* The *Crateropodinæ* (a bad substitute for the *Timeliidæ* (*inæ*) follow the *Turdidæ* in Seebohm's arrangement, whereas in Oates's classification they come after the *Paridæ*, which are by him considered to be a sub-family of the *Corvidæ*. Oates's *Sylviidæ* are separated from his Timaline birds by whole families of *Certhiidæ*, *Sittidæ*, and *Regulidæ*, while in the Seebohmian arrangement the *Sylviinæ* follow the *Crateropodinæ*, and are in turn followed by the *Parinæ*, which contains Gold-crests (= Fam. *Regulidæ* of Oates), Tits (= Sub-fam. *Parinæ* of Oates), Wrens and Creepers (= Fam. *Certhiidæ* of Oates), as well as the Nuthatches (= Fam. *Sittidæ* of Oates). The *Laniidæ* and *Sturnidæ* are the only families which are similarly located by both authors, and in our humble opinion both of them are wrong. If Mr. Seebohm should ever honour us by following any classification of ours in the way in which he has followed that of Mr. Oates, *with slight modifications*, we can only beg to be protected from our friends!

The present work commences with a useful table of the literature relating to the avifauna of Japan, to which may be added a paper by Salvadori and Giglioli, "Uccelli raccolti durante il Viaggio della Corvetta *Vettor Pisani*, &c.," where there are some useful notes on the Scoters (*Œdemia*) and other birds. It seems to us a great pity that, having looked up all his books with so much assiduity, Mr. Seebohm did not think it worth while to publish a full list of references to Japanese birds, which would have been most useful, and is even necessary in a work of this kind. The chapter on the "Geographical Distribution of Japanese Birds" is very interesting, and the subject is worked out with all Mr. Seebohm's accustomed energy and speculativeness, aided by full statistics.

In the third portion of the work, the "Classification and Identification of Japanese Birds," the reader will find a great deal more than the mere title denotes, for, as we have hinted before, the author has seized the opportunity of amplifying all his previous work on the classification of Birds, so that this portion of the book is of the highest interest to ornithologists of every country. We find, however, that some of the woodcuts are not explained in the text, and are apparently added as make-weights to the diagnostic characters of the orders, but the reason for so doing does not seem very clear.

Apart from the omission of the name of the Natural History Museum (Preface, p. iii.) from the list of fourfold obligations which are considered to be due to other Museums of Europe and America, a very uncomplimentary allusion to the work of the present writer occurs on p. 113, under the heading of *Motacilla japonica*. We have no intention of following Mr. Seebohm in his reasoning with regard to this species. He devotes nearly a page to show into what confusion (partly through his own fault, as he admits) these black-backed Wagtails of Japan had fallen, and then he claims to have fixed, in 1884, that Swinhoe's name of *japonica*, bestowed in 1863, must be restricted to the larger form which we re-named *M. grandis*, "a useless synonym," as Mr. Seebohm is kind enough to call it. Nevertheless, we can assure Mr. Seebohm that if Swinhoe intended to give his name of *japonica* to one of the black-backed Wagtails of Japan, it was to the *small* one and not to the *large* one, that he meant it to apply, as a specimen in the British Museum labelled in his own handwriting shows! Swinhoe's name, therefore, is a synonym of *M. lugens*, and neither Mr. Seebohm nor any one else can "fix" the name of *japonica* for the large species. So far from being a "useless synonym," the name of *M. grandis* is the only one which can properly be applied to the latter, and even if Mr. Seebohm's argument had been correct, his manner of criticism is needlessly disagreeable.

In a work like the present, which is nothing if not exhaustive, it is surprising that we can find no reference to *Garrulus lidthi* and *Accentor fervidus*. The only evi-

dence to hand at present is that the former bird inhabits the mountains of the interior of Japan, whilst the *Accentor* may be only *A. rubidus*, but it is at least well to say so. Then again the recent work of Mr. Ogilvie-Grant on *Platalea* and *Turnix* was worth a little consideration. The nomenclature proposed for the latter genus is not adopted, and in spite of the large series of measurements given by Mr. Grant to show that the Eastern race of the common Spoonbill has a longer bill than the Western race, Mr. Seebohm states that he has been "unable to find the slightest evidence of the truth of this statement," a mode of criticism more forcible than exact. We might also ask the author why he persists in calling the Woodcock, *Scolopax rusticola* instead of *S. rusticula*, and the Wild Duck, *Anas boschas* instead of *A. boscas?* Also why does he misspell Linnæus's name throughout the work? Mr. Seebohm has, however, his own ideas as to the fitness of things, and he is in many respects too ultra-conservative for us to hope that our criticism will move him. Otherwise we might ask what is the use of *Eurhinorhynchus* having its spoon-shaped bill, if it is to be merged in the genus *Tringa?* Again, to merge so many species under the genus *Picus*, and again under *Fringilla*, which most of us consider to belong to recognizable genera, tends to fog and confuse the ideas of geographical distribution, and by no means simplifies the study, as Mr. Seebohm would have us believe.

We may add that the work is illustrated by figures from the author's work on the *Charadoiidæ*, but a large number of new cuts are added, which increase the utility of the diagnoses in the classificatory part of the work.

R. Bowdler Sharpe.

JEANS'S "WATERWAYS AND WATER TRANSPORT."

Waterways and Water Transport. By J. Stephen Jeans, M.R.I., F.S.S. (London and New York: E. and F. N. Spon, 1890.)

THIS volume is intended to give a description of the waterways of the world and water transport, and more particularly means of transport by artificial waterways. Under the heading of "The Transportation Problem," the author deals with the vast improvements made during recent years in roads, both ordinary and rail, and with the great advancement of trade caused by better means of transport during the last hundred years. He shows that, although canals may be considered as belonging to a bygone day, they are now coming again into prominence as a cheap means of transport, and that probably they will in many cases be made the nucleus of a new and better system, under which the great inland towns of Lancashire, Staffordshire, and Yorkshire may practically become maritime places. Chapters ii., iii., and iv. deal with the English river and canal system, and the waterways of Scotland and Ireland, giving an historical account, and showing how most of them in many ways have grown and improved. Readers of this volume will be surprised no doubt at the network of canals in this country: one is accustomed to think of railways as the only means of transport, and to forget the really large traffic carried by canals in many counties. The author tells us of the many continuous lines of water communication between different commercial centres of importance in England, and points out how often it happens that the through routes are rendered useless for really large boats, owing to the locks being shorter or narrower on one section than on another—thus allowing the smallest lock to be the gauge of the boat—down to the very low maximum of twenty-four tons on the canal system between the Derbyshire district and London.

On projected canals the author has much of course to say. The Manchester Ship Canal, which has attracted so much attention, no doubt has been the cause of many similar projects. The Forth and Clyde Canal is designed to enable vessels of considerable tonnage to pass from sea to sea, the present waterway being too contracted to be of much use. The Sheffield and Goole Canal is projected to form an improvement on the present navigation, to enable barges carrying 700 tons, and small sea-going steamers carrying 300 to 400 tons, to come to Sheffield for cargoes, and to serve the South Yorkshire collieries. The proposed waterways from Birmingham to the sea are now being considered in that district. In short, the present tendency seems to be to bring the ship to the manufactory, and thus save the railway charges to the coast for the carriage of the manufactured article.

The book is divided into three sections, the first of which concludes with a good detailed description of the waterways of different countries. Holland, the land of dykes and ditches, appears to have a splendid system of water communication, and the United States has received ample notice at the hands of the author. The waterways of British India are described, and the question of canals *versus* railways in that country is discussed. The author says that "Sir Arthur Cotton has even advocated the summary and indefinite suspension of nearly all railway schemes and works, in order that the attention of the Government might be concentrated upon canals, mainly for irrigation, but also adapted for purposes of navigation." This is all very well from the canal point of view, but it would be interesting to calculate the capacity of canals capable of contending with the present traffic on the railways, excluding any military questions from the subject; and in case of the famine railways, *i.e.* railways built to distribute food as a primary reason for their existence, and where quickness of delivery becomes the all-important consideration, the comparison becomes absurd. Much important work has, however, been done in India by an extensive system of artificial waterways serving the dual purpose of irrigation and navigation, and by careful superintendence the country is greatly improved and enriched by their use.

Section II. of the volume treats of the important subject of ship canals. The greatest artificial waterway constructed up to the present time has been the Suez Canal, and this monument of engineering skill is very properly dealt with first in this section. It is interesting to note that some of the earliest canals recorded were constructed between Suez and the Nile, and these were for some reason allowed to fall into decay. The author gives an excellent account of the construction of the Suez Canal; the political and monetary difficulties encountered by M. de Lesseps in the early days of the company are explained; and the ultimate completion and enormous growth of traffic through the canal are well described. On p. 208, we

are told that "vessels of nearly 200 feet in *width* propel themselves through the canal." This must be a misprint.

Of the Panama Canal we find a good descriptive account. The many early surveys made to locate the best course for such a project are described, and it is interesting to note that as early as the year 1588 the proposal to construct such a canal is recorded. The floating of the original company, the commencement of the works, and the ultimate complete failure of these works, are well described by the author. Everything appears to have happened to seriously hamper the work on every side: political strife on the isthmus delayed the work; an act of incendiarism destroyed a number of buildings erected for the purpose of the canal; and the heavy mortality among the *employés* obtained an unenviable notoriety, and rendered the supply of good men uncertain. Bad as these events proved, the real reason for the ultimate failure of this undertaking must ever be ascribed to the insufficient data obtained of the country and of its geological formation by the company's engineers; the original estimates have proved to be understated and entirely wrong, and the many engineering difficulties must have been practically overlooked.

The Report of the Special Commission appointed in 1889 to inquire into the affairs of the company was published in May, and describes in detail the position of the undertaking. It is estimated that some 30 millions will be required to finish it, so that its ultimate construction does not appear very probable.

The projected Nicaraguan Canal, a purely American project, is also described. The author says:—"The distance from ocean to ocean by the route that has recently received the approval of the United States Government, and is now in course of apparent realization, is 169·8 miles. Of actual canal there will be 40·3 miles, the remaining 129·5 miles being free navigation through Lake Nicaragua, the Rio San Juan, and the valley of the Rio San Francisco."

Chapter xxiii. brings us home again, and deals with the Manchester Ship Canal, a monument of engineering now fast reaching completion. From the excellent description given of these works the reader will obtain a good idea of the undertaking generally.

Chapter xxiv. commences Section III., and deals with the transport problem with special reference to railways and canals. The question of railways *versus* canals is here discussed, and the steady decline of canal navigation from the date of the commencement of railway competition is pointed out. The author says that at that time, "one by one, canals dropped out of the race, and were bought up by the railway companies, either with a view to getting rid of competition, and so securing absolute control over the traffic, or in order to make way for new railway lines." Curiously enough, the *Engineer* of the 3rd inst., in a leading article on this subject, illustrates the above quotation by a reference to the Sheffield Canal, which has been allowed by the present owners—the Manchester, Sheffield, and Lincolnshire Railway Company—to silt up and become nearly useless.

The railways in this and other countries are getting to be considered gross monopolies, and the improvements in the canal navigations are being looked upon as a means of relief. The Manchester Ship Canal is the firstfruits of this feeling on the part of traders and manufacturers, and other ship canals are being talked of.

On the comparative cost of water and land transport the author has much to say. In discussing the relative cost of carriage in the States and in this country, we must not forget that the capital charges per mile on open lines in this country cannot fairly be compared with those in the States, for the reason that the land was in the first instance bought from landowners anxious to obtain the largest sum; the average station buildings and fixed plant are of a far more expensive description; and the kind of traffic carried is of a different type.

The railways in the United States appear to be able to carry goods at a remarkably low rate, no doubt severe competition for the traffic being the reason; at the same time, excluding capital charges and the like, the amount of coal burnt per ton mile in this country is far below that used by the American locomotives.

If traffic is to be moved from town to town at the cheapest rate, it is necessary that it shall be moved in large masses, or trains. It is on this account that the American traffic can be transported by railway cheaper than in this country: were it possible in England to obtain a steady through traffic in any large volume, the weight of trains hauled would certainly increase, and the rates would probably drop in proportion. Canals, when properly managed and with proper appliances, ought to carry heavy traffic with the same regularity as the railways; but as long as they are controlled by the railway companies, they are, in the nature of things, bound to decay and become a secondary means of transport.

The author explains in chapter xxviii. various mechanical means of haulage in vogue at the present time, and then goes on to deal with locks, planes, sluice-gates, and the like. The volume concludes with a chapter on the acquisition by the State of the waterways. The subject is handled in a masterly manner. In this book we have a large amount of information put together in a readable form, and no doubt it will prove very useful to those interested in a very important subject. N. J. L.

SANITY AND INSANITY.

Sanity and Insanity. By Charles Mercier, M.B., Lecturer on Insanity at the Westminster Hospital Medical School, and at the Medical School for Women. The Contemporary Science Series. (London: Walter Scott, 1890.)

TO bring the facts of any department of knowledge before the non-scientific in an easily assimilable form, without offence to the good taste of some one or other section of the community, is by no means so simple a matter as the prolific literature of this class in late years might seem to indicate. It is not every author or lecturer, however able as a man of science, who can thus cater satisfactorily for an omnivorous, but captious and critical public. Every mechanics' institute and popular lecture-room exemplifies this truth—the enthusiasm of the aspirant to public honours in this field is often inversely proportional to his qualifications and actual attainments. The first requisite condition is that the author be

thoroughly and profoundly acquainted with his subject, so that in a popular *résumé* facts should assume their due perspective—that mole-hills be not amplified into a *bizarre* prominence, or that the great mountain tracts encircling the subordinate features of the territory lose not in the distant haze their outlines—in other words, that principles be clearly enunciated, and inductions marshalled in harmonious sequence. The next pre-requisite qualification is a keen realization of the obstacles which beset his own path of observation; those knotty points, those complex junctions of thought which cause so much delay in the history of all intellectual effort. The most learned authorities are often the most laboured and tedious exponents of their craft; but we have only to glance at the essays and popular lectures of Clifford, Tyndall, Huxley, or Haeckel, to learn what a degree of excellence is thus attainable by a profound thinker and a cultured mind. It is on account of the rarity of this style of writing that we hail with pleasure the appearance of Dr. Mercier's book, which is an excellent example of the perspicuity with which a cultured mind can delineate an obscure and difficult subject. Dr. Mercier's numerous contributions to psychological literature which have appeared from time to time in *Brain*, *Mind*, the *Journal of Mental Science*, and his book on the "Nervous System and the Mind," are a sufficient pledge of his capacity for a graceful handling of the subject of insanity. After a preliminary sketch of the mechanism of the nervous system, and the modern view of its mental correlate, given in simple but pleasing outline, the author devotes his fourth chapter to a discussion on the "Nature of Insanity," which he defines as a disorder in the *process* of adjustment of the organism to its environment—a disorder not subject to correction. The faulty adaptation of organism to environment is fully considered, and all qualifications in any such definition of insanity are lucidly expressed.

In this chapter above all others the author exhibits his analytic abilities and discriminative capacities to greatest advantage, and the conception of the nature of insanity so framed is to our minds a mental synthesis which has remained unchallenged since first enunciated by Dr. Mercier in 1882. In this connection he makes the trite remark that such process of adjustment is simplified by a simplification of the environment, and hence the major utility of asylum treatment. Undoubtedly, the moral factor will long remain the most important in the treatment of insanity; and here Dr. Mercier would seem at one with Dr. Clifford Allbutt, who with becoming pungency ridiculed the idea of "curing insanity out of the bottle."

The following six chapters are devoted to etiological inquiries, and the causes of insanity are grouped under the headings of heredity, direct and indirect strain, neurotic instability, the laws of inheritance. Reversion and its limitations by the "massive pressure of race heredity" are ably discussed, as is also prepotence in its relations to insanity. The potency of the *moral factor* in the production of insanity, always tinctured with more or less mystery to the laity, is largely developed by the author, and reduced to its simple elementary terms; in fact, the work before us is calculated very largely to remove the repellent aspects of insanity which so long have been created by ignorance and a false appreciation of its nature.

With respect to the community of origin of the religious and sexual instincts, we cannot find ourselves in accord with Dr. Mercier's views; throughout his argument we believe he places undue emphasis on the significance of the sacrificial element. Cogent as are the arguments so frequently used to indicate such lineal relationship, we think equally strong reasons may be advanced to show that the development is along parallel lines of contiguity, rather than the sublimation of the religious out of the sexual element. The concluding five chapters deal with the various forms of insanity; and the vagaries of the insane mind are graphically registered in these short but concise and interesting delineations. We congratulate Dr. Mercier on the production of a work which deserves a widespread popularity.

W. B. L.

OUR BOOK SHELF.

A Guide to the Literature of Sugar: a Book of Reference for Chemists, Botanists, Librarians, Manufacturers, and Planters, with a Comprehensive Subject-Index. By H. Ling Roth. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1890.)

Although published in 1890, it is right to say that this compilation only brings our knowledge down to the beginning of the year 1885. It is intended to have a supplement ready soon, and to bring the work up to date. In the meantime we can speak highly of the evident care and labour bestowed on this volume by the compiler. The arrangement is based on that of Mr. Daydon Jackson's "Vegetable Technology." There is a catalogue of authors, a list of anonymous publications, a list of periodicals, a list of Parliamentary publications, and a chronological table. The first part of the latter is taken from Dr. Falconer's "Sketch of Sugar in Early Times" (1796). The comprehensive subject-index forms a very valuable part of the work. It is arranged in sections as follows:—Bibliography and History, Statistics and General Economy, Illustrations, Geographical Distribution, Chemistry, Origin of Vegetable Sugars (the various plants yielding sugars), Beet Sugar, Cane Sugar, Parasites, and Distillation. It will be easily seen that this guide to the literature of sugar covers practically the whole field in regard to vegetable sugars. It is a work that will prove of much interest to numerous readers having to do with the cultivation and manufacture of sugar, whether derived from the sugar-cane, beet, sorghum, palm, maple, or maize. We only hope the compiler will be encouraged to bring out the promised supplement. During the last five years considerable activity has been displayed in the United States in regard to the production of sugar from sorghum; and there is, besides, the very important fact that the sugar-cane has recently been shown to produce mature seed, and possibly capable of improvement by seminal selection. The literature in regard to this point alone is well worthy of being carefully traced.

D. M.

Practical Plane and Solid Geometry. By I. H. Morris. (London: Longmans, Green, and Co., 1890.)

Students will find this work to be a most instructive course, arranged in such a way that no external aid will be required. Section I. begins at the very beginning of the subject, and in it there are many problems dealing with lines, areas, use of scales, plans and elevations of solids, sections, &c. Section II. treats of descriptive geometry; and various problems on the projections of lines, oblique surfaces, and solids are given, and thoroughly worked out. The concluding chapter of this section is devoted

to graphic arithmetic, in which there are both questions and examples on multiplication, division, addition, subtraction, fractions, involution, &c.

Throughout the book the figures are placed on the right-hand pages, and the text opposite them on the left—a very good arrangement. The diagrams and figures are neat and clear, especially the complicated figures required in the drawing of sections of some solids. The exercises have been selected from the papers of the Science and Art Department, College of Preceptors, Oxford and Cambridge Locals, and various Military Colleges. They are carefully graduated, and, when necessary, hints have been added to facilitate their solution.

Madagascar; or, Robert Drury's Journal. Edited by Captain P. Oliver. (London: Fisher Unwin, 1890.)

THIS book may be divided into three parts: Captain Oliver's introduction and notes, Robert Drury's journal, and a description of the island by the Abbé Rochon. In the first part Captain Oliver tries to prove that the journal is more or less fictitious. At the beginning of the introduction he gives the names of—as he himself says—the best authorities in France, all of whom believe the journal to be true; also a letter which leads him to say that the book was credited in the middle of the eighteenth century. After having quoted these authorities in favour of the truthfulness of the journal, Captain Oliver proceeds to give his own ideas on the subject, which are that the book was written by Defoe from Drury's story, and a great deal of the matter taken out of French books—namely, François Cauche, 1658, and Hacourt, 1661. He then goes on to say that the original journal had a French map, and he regards that also as evidence against Drury. Drury acknowledges himself to have almost forgotten the language and manners of his own country, and, as he was but fourteen years of age when he left, we may take it for granted that he did not know how to draw a map. What then could be more natural, when he had his journal edited, than to take the best map then published, which happened to be a French one, and give it with his journal?

After reading the introduction, one almost thinks that the book is fictitious; but when half-way through the journal, in which every little action is described so minutely, one comes to the conclusion that it is true—at least, that it has not been proved untrue. The journal itself is interesting, but very monotonous.

The description of the island by the Abbé Rochon is very interesting, as it tells all about the first attempts of the French to colonize Madagascar. H. C. L.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Large Meteors.

THE "ball of fire" seen by Mr. C. Randolph at Milverton, Somerset, on October 16, at 12h. 5m., and the "brilliant meteor" observed at Edinburgh, on October 17, at about 15h. (see NATURE of October 23, pp. 615 and 620), were probably members of the October meteor shower, which has a maximum on about October 18-20, and a radiant point at $92° + 16°$ in the extreme north-eastern limits of Orion.

There was also a fine meteor on October 19 at midnight. I was engaged in telescopic observation at the time, and was intently watching a new nebula I had just discovered about 2° N. of the star α Camelopardi, when I became aware of several brilliant flashes which lit up both sky and landscape in a startling manner. Quickly withdrawing my eye from the telescope, I turned towards the direction from whence the flashes proceeded, and saw the end point of a magnificent fire-ball which had fallen in the vapours on the western horizon. It left a bright streak just east of β Aquarii, or from $326° - 8°$ to $319° - 10°$, but this soon died away.

This meteor must have been a grand object to observers in the Bristol Channel and in the western counties of England. The city clocks were striking the hour of twelve when it appeared, and from the direction of its flight it evidently belonged to the well-known Orionid meteor shower.

The new nebula I have referred to is situated at $71° + 68°$, and is a fairly conspicuous object in my 10-inch reflector with a power of 60. I watched it for more than an hour for traces of motion, but detected none, so I assume it was not a comet. Since October 19 we have had clouded skies, and I have had no opportunity to re-observe the object.

Bristol, October 24. W. F. DENNING.

Extraordinary Flight of Leaves.

THE pastoral farm of Dalgonar is situated near the source of the Skarr Water, in the parish of Penpont, Dumfriesshire. The ridge of hills on the farm as per Ordnance Survey is 1580 feet above sea-level. There are only five trees on the farm—two ash and three larch. An extraordinary occurrence presented itself to the eyes of Mr. Wright, my informant, at the end of October 1889, on this farm, which has been narrated to me in a letter received from him, as follows:—

"I was struck by a strange appearance in the atmosphere, which I at first mistook for a flock of birds, but as I saw them falling to the earth my curiosity was quickened. Fixing my eyes on one of the larger of them, and running about 100 yards up the hill until directly underneath, I awaited its arrival, when I found it to be *an oak leaf*. Looking upwards the air was thick with them, and as they descended in an almost vertical direction, oscillating, and glittering in the sunshine, the spectacle was as beautiful as rare. The wind was from the north, blowing a very gentle breeze, and there were occasional showers of rain.

"On examination of the hills after the leaves had fallen, it was found that they covered a tract of about a mile wide and two miles long. The leaves were wholly those of the oak. No oak trees grow in clumps together nearer than eight miles. The aged shepherd, who has been on the farm since 1826, never witnessed a similar occurrence." JAMES SHAW.

Tynron School, Dumfriesshire, October 21.

On the Soaring of Birds.

IN answer to my criticism (NATURE, September 4, p. 457), Mr. Blix refers (October 16, p. 593) to an article in the *Skand. Arch. f. Physiologie*, in which he has given "an account of the weighty reasons" leading him "to suppose that soaring birds are able to undertake successive alterations of direction with very little loss of *vis viva*." To bring forward reasons, however, tending to show that birds *can* do certain things is no answer to an objection with regard to *how* they do them.

Mr. Blix has thought it superfluous to point out "that the manœuvre of the bird is the same, and the loss of energy thereby equally the same, whether the bird turns in a calm or in a uniform wind," from which it is to be inferred that he had thoroughly grasped the truth of this himself. Why did he, then, propound a theory founded upon what is directly contrary to his own conviction?

It is not easy to see what has led Mr. Blix to suppose that I hold any other opinion, since my letter was written with the intention of pointing out this fact to him.

19 Well Walk, Hampstead, October 23. C. O. BARTRUM.

MANNERS AND CUSTOMS OF THE TORRES STRAITS ISLANDERS.[1]

IT is not my intention this evening to attempt a special study of any particular institution or series of customs, nor even to discuss the ethnological affinities of the natives inhabiting the islands of Torres Straits.

[1] Friday Evening Lecture delivered at the Royal Institution, by Prof. Alfred C. Haddon, on May 23, 1890.

The comparative study of institutions and customs has led to brilliant suggestions, and has especially thrown light upon obscure facts in our own culture, and given a new significance to observances which, because they are of every-day occurrence, are passed by without comment. This field of inquiry is one which has only recently been systematically tilled, but it promises a rich harvest of unexpected results.

The detailed study of a single tribe or natural assemblage of people has great interest, as it puts one in touch with such varied subjects as the physical, mental, and moral characters of the people; and the tracing out of their affinities requires wide study and careful comparisons. A patient research of this kind always opens up questions of wider import than the initial inquiry.

Neither of these methods will occupy us to-night, as I wish to present before you as vivid a conception as I can of some of the manners and customs of a people small in number but rich in interest. We will consider, therefore, neither a composite image of savages in general, nor of rude customs, but the particular habits of a disappearing people, who thirty years ago were naked, unknown savages, who to-day are British subjects, and who in a very few years will have lost the last remnants of their individuality, and possibly ere long will practically cease to exist—at all events as a distinct people. The dissolving views which I shall exhibit this evening are a fit emblem of the facts which they illustrate.

My anthropological inquiries in Torres Straits may not inaptly be compared with the methods of the palæontologist, especially in his study of the more recent fossils. Amongst such fossils we find some representatives of existing forms, others slightly different from those we are accustomed to, others again which are quite dissimilar, and often of these only disconnected fragments may remain, and it takes great patience and careful piecing together to restore the latter into any semblance of their former selves; nor should surprise be felt if mistakes are occasionally made in the attempt.

A similar experience occurs to those who study an isolated people which is rapidly becoming modified and is dying out at the same time. Some facts collected from legend and myth precisely resemble the present habits of the natives; others have only lately fallen into desuetude. Lastly, some customs are so dissimilar from anything in our own country, that it is difficult to thoroughly understand them under favourable circumstances; but when these customs are no longer practiced, and but imperfectly remembered, when they have to be described through the unsatisfactory medium of Jargon English, and when one bears in mind the great difference in the mental conceptions of narrator and listener, what wonder is there that disconnected narratives are recorded, or that errors creep in?

Happy is that traveller who has the opportunity of studying existing habits. It was my lot to recover recently lost or fast dying-out customs; our archæologists grapple with the problems of the past; it is the object of all to assist towards a complete History of Man.

Torres Straits, as you are aware, separate New Guinea, the largest island in the world, from Australia, the smallest continent. Although the Straits are eighty miles wide in their narrowest part, yet, owing to the presence of islands and of numerous and often extensive coral reefs, there is only one channel suitable for ocean-going steamers, and that averages a mile in width, and in places is much less.

The islands in Torres Straits may be divided into three geological groups by the lines of longitude 142° 48′ E. and 143° 30′ E.

The islands to the west are composed of old igneous rocks, and are surrounded by fringing reefs. These islands may in fact be regarded as disconnected portions of Northern Queensland. They are fertile, but there is no particularly luxuriant vegetation; doubtless irrigation and cultivation would greatly improve their productiveness.

The central group of islands is composed of low coral islets formed by wind and wave action; the soil is poor, and supports only a scrubby vegetation. Coco-palms grow on some of the islands, and there are occasional mangrove swamps.

The eastern islands, Uga, Erub, and the Murray Islands, are of volcanic origin, and are also fringed with coral reefs. In these the soil is rich and vegetation luxuriant, Uga and a great part of Mer being simply large gardens of coco-palms, bananas, and yams.

It is interesting to find that the inhabitants of the volcanic islands form one tribe, which I term the Eastern Tribe; the Western Tribe occupying all the remaining islands. The customs of the two tribes are different and their languages distinct, so much so that there are only a few words in common, and these are mainly trade words. Four subdivisions of the Western Tribe can be distinguished, the members of each of which inhabit certain intermarrying groups of islands.

Independently of the above-mentioned subdivisions, the islanders were divided into clans, each clan having some animal for its *augŭd* or "totem." For example, in the Western Tribe there were the dugong, turtle, dog, cassowary, snake, shark clans, and so forth. There was supposed to be some relation between the clans and their respective *augŭd*, "all same [*i.e.* similar to] family," as it was expressed to me. A dog-man, for instance, was credited with understanding the habits and feelings of dogs, or a cassowary-man prided himself on having thin shanks like a cassowary, which would enable him to run quickly through the grass. With the exception of the first two clans, no one was allowed to kill or eat the totem of his own clan; if he did, his other clansmen would probably kill him for sacrilege. On a dugong expedition, no dugong-man might keep the first dugong he captured, but he might partake of the rest; the same restriction applied also to the turtle and the turtle clan. If only one dugong or turtle was caught on the first day, the dugong- or turtle-man had to relinquish it; supposing only one was caught on the succeeding day, the account was, so to speak, "carried forward," and there was no *sabi* (*tabu*) on it. The dugong and turtle were too important articles of food for the clan members to be entirely deprived from partaking of their *augŭd*.

The women, or at all events some of them, used to have a representation of their *augŭd* cut on the small of the back. I made inquiries on this point on most of the islands in the Straits, but could only find four old women who had them; these I sketched, and two of them I also photographed.

[Various photographs illustrating the appearance of the natives were then thrown on the screen.]

I have alluded to the fact that different customs characterize the Eastern and Western Tribes; as an example of this I may mention that in the latter tribe the girls proposed marriage to the men, while in the Eastern Tribe the more usual course was adopted.

It might be some time before a lad had an offer; but should he be a fine dancer, with goodly calves, and dance with sprightliness and energy at the festive dances, he would not lack admirers.

Should there still be a reticence on the part of his female acquaintances, the young man might win the heart of a girl by robbing a man of his head. Our adventurous youth could join in some foray; it mattered not to him what was the equity of the quarrel, or whether there was any enmity at all between his people and the attacked. So long as he killed someone—man, woman, or child—and brought the head back, it was not of much consequence to him whose head it was. Possibly a man killed would redound to his greater glory, but any skull was

better than none, and its possession was recognized as an order of merit. How much more distinction would a man gain when he could boast of a whole trophy of skulls!

The girl's heart being won by prowess, dancing skill, or fine appearance, she would plait a string armlet, *tiapururu;* this she intrusted to a mutual friend, preferably the chosen one's sister. On the first suitable opportunity the sister said to her brother, "Brother, I have some good news for you. A woman likes you." On hearing her name, and after some conversation, if he was willing to go on with the affair, he told his sister to ask the girl to keep some appointment with him in the bush.

When the message was delivered, the enamoured damsel informed her parents that she was going into the bush to get some wood or food, or made some such excuse.

In due course the couple met, sat down and talked, the proposal being made with perfect decorum.

The following conversation is given in the actual words used by my informant, Maino, the chief of Tud.

Opening the conversation, the man said, "You like me proper?"

"Yes," she replied, "I like you proper with my heart inside. Eye along my heart see you—you my man."

Unwilling to give himself away rashly, he asked, "How you like me?"

"I like your fine leg—you got fine body—your skin good—I like you altogether," replied the girl.

After matters had proceeded satisfactorily, the girl, anxious to clench the matter, asked when they were to be married. The man said, "To-morrow, if you like."

They both went home and told their respective relatives. Then the girl's people fought the man's folk, "For girl more big [*i.e.* of more consequence] than boy;" but the fighting was not of a serious character, it being part of the programme of a marriage.

"Swapping" sisters was the usual method of getting a wife. If a man had no sisters he might remain unmarried, unless he was rich enough to pay for a wife with a shell armlet (*waiwi*) or a canoe, or something of equal value. If a youth was "hard up," an uncle might take compassion on him and give one of his own daughters in exchange for a wife for his nephew.

This exchange of girls—a sister for a sister, or female cousin for another man's sister—was an economical method of getting a wife, as one was a set-off against the other. The usual feasting occurred, but the presents were dispensed with, or at all events the purchase-money was saved, and probably there would be no fighting.

When a young man of the Eastern Tribe arrived at an understanding with a girl, he put his *gelar* ("law," *i.e. tabu*) on her, and made arrangements to fetch her away. She kept awake on the appointed night, listening for the preconcerted signal, and they quietly stole away to his parents' house, and the next morning he sent a messenger to say where the girl was. The girl's friends armed themselves with bows and arrows, sharks' teeth fastened on to sticks, and other weapons, and proceeded to the other village; but the fight was not a serious affair. On the same day the girl would be painted red by her future mother-in-law, and clothed with a large number of leaf petticoats; and numerous ornaments would be suspended on her back, these made a clanking sound whenever the girl moved. For some months she remained in the house, and under the constant supervision of her future mother-in-law, the young man residing elsewhere. After say three months, negotiations would commence between the two families, and the girl's relations would come to *taaugwat* (or scrape hands), and presents would be exchanged, and some alteration made in the decking of the girl. After a further probation period of a few months, some friend, in the secret, would engage the young man in conversation, and the bride would steal up behind him with some food she had previously cooked, and, while still behind his back, would thrust it by his side. He, looking round, exclaimed, "Why, that's my woman!" and then hung down his head in shame. Being informed that all was duly performed according to old usage, the couple ate food together, this being the ratification of the contract.

It appears that in the Eastern Tribe marriage was regarded as a state of *tabu*, the man isolating one woman as his exclusive property, for he had powers of life and death over his wife. For several reasons I suspect that the Eastern Tribe has arrived at a slightly higher stage in the evolution of the family than the Western, as the man has a more independent position, and does not live more or less with his wife's people after marriage, as is the custom among the Western Tribe. In both tribes a wife had to be paid for; a canoe, dugong-harpoon, shell-armlet, or articles of equal exchange value, being the usual price.

Manhood is with us a gradual development of youth; with nearly all savages it is a state of privilege, the full advantages of which can be gained only by the observance of special ceremonies.

The growth of hair on the face warned the father that his boy was growing up, and he consulted with other fathers who had sons of about the same age.

"Good thing," he might have remarked; "boy no stop along woman now: he got hair, time we make him man now;" and arrangements would be duly made.

The following information, respecting the former initiation ceremonies, was gained at Tud (usually known as Warrior Island), the natives of which island were probably the most warlike of all the Western Islanders:—

The lads were handed over to their uncles, or to some old man, by their fathers, who then ceased to have any intercourse with them. They were conducted to the *Taiiokwod*, or open space sacred to the men, where no woman or child ever ventured, and which henceforth had for them many deep-rooted associations. The uncles washed the youths with water and then rubbed charcoal into the skin; this being daily repeated till the probation period was over. The lads were covered with mats doubled up like a tent with closed ends, and there they sat the livelong day in groups, without moving, playing, or even speaking. Four large mats stretched across the *Taiiokwod*, the mats belonging respectively to the *Sam* (cassowary), *Umai* (dog), *Kodal* (crocodile), and *Baidam* (shark) clans. For each mat there was a fireplace, the fire being tended by the young men of their respective clans. The old men sat on their appropriate mats, in the centre were the drums, and the dance-masks were placed along one side. Opposite the centre was a small mat, on which sat the chief of the island; for, contrary to the general custom of the tribe, this island had a recognized chief, the result, probably, of their belligerent habits. By the side of where the chief used to sit, a large ovoid stone was pointed out to me; it had a dire significance, for long ago four boys, tired of the irksomeness of the discipline, broke bounds, and meeting their mothers in the bush, asked for food. They were recaptured, and were all killed by the old men with that stone, which was then placed in its present position, as a warning to other youths. The boys of the cassowary and dog clans sat at the end beyond the shark fireplace, and the crocodile and shark boys were placed at the opposite end of the clearing.

Their instructors watched the lads, and communicated to them the traditions of the tribe, rules of conduct were laid down, information in all branches of native lore taught, and thus, generation after generation, the things of the fathers were transmitted to the sons.

The following are some of the rules which I was informed were imparted to the youths by the "old men":—

"You no steal."

"If you see food belong another man, you no take it, or you dead."

"You no take thing belong another man without leave; if you see a fish-spear and take it, s'pose you break it and you no got spear, how you pay man?"

"S'pose you see a dugong-harpoon in a canoe and take it, and man he no savvy, then you lose it or break it, how you pay him? You no got dugong-harpoon."

"You no play with boy and girl now; you a man now, and no boy."

"You no play with small play-canoe, or with toy-spear; that all finish now."

"You no like girl first; if you do, girl laugh at you and call you a woman." [That is, the young man must not propose marriage to a girl, but must wait for her to ask first.]

"You no marry the sister of your mate, or by and by you will be ashamed; mates all same as brothers." [But "mates" may marry two sisters.]

"You no marry your cousin; she all same as sister."

"If anyone asks for food, or water, or anything, you give something; if you have a little, you give a little; if you have plenty, give half."

"Look after your mother and father; never mind if you and your wife go without."

"Don't speak bad word to mother."

"Give half of all your fish to your parents; don't be mean."

"Father and mother all along same as food in belly; when they die you feel hungry and empty."

"Mind your uncles, too, and cousins."

"If woman walk along, you no follow; by and by man look, he call you bad name."

"If a canoe is going to another place, you go in canoe; no stop behind to steal woman."

"If your brother is going out to fight, you help him; don't let him go first, but go together."

Who will say, after this, that the Torres Straits Islanders were degraded savages?

At length the month of isolation expired, and for the last time the uncle washed the lad; he then rubbed him with scented leaves, and polished him up with oil. Then he was decorated with armlets and leglets, breast-ornaments, and possibly a belt, his ears ornamented, and a shell-skewer passed through his nose; bright-coloured leaves would be inserted in his armlets, and his hair rolled into the approved string-like ringlets. So they "make him flash—flash like hell—that boy."

The afternoon of the eventful day was occupied in this congenial task, and at nightfall all the lads who were being initiated were marshalled by their uncles behind a large mat, which was held vertically. In this wise they marched to the village until they arrived at an open space where a mat was spread on the ground before a circle of friends and relatives. When the approaching party reached this mat the lads seated themselves upon it, and then the screening mat was lowered. Suddenly, for the first time for a month, the fathers and female relatives saw the boys, and great were the crying and shouting and exclamations of delight at the brave show. With tears the mothers cried out, "My boy! my boy!" and they and other elderly female relatives rushed up to them and fondled and caressed them, and the mothers surreptitiously put dainty morsels by their boys.

Sitting with legs crossed under them and down-turned faces, the boys neither moved nor exhibited the least emotion, for now they were men.

Less precise is my information respecting the corresponding rites of the Eastern Tribe. So far as I could gather, there were in Mer, the largest of the Murray Islands, two important ceremonies, which we may term the initiation and recognition ceremonies. For the first the lads were assembled near a sacred round house, or *pelak*, in which the awe-inspiring masks were kept. The ceremony was conducted by three *zogole*, or sacred men, and their *tămileb*, or attendants. The latter arranged themselves in a double row, from the *pelak* to the place where the boys were assembled, and, holding long sticks, performed certain movements. Slowly the dread apparition advanced; the chief *zogole* came first, wearing a huge mask with human features and a beard of jaw-bones; the second *zogole* steadied this mask with a rope; the third *zogole* wore a long mask, shaped like a shark. Then for the first time the names of these masks were revealed to the lads—BOMAI and MALU. These were the sacred names which it was not lawful to communicate to the outsider, death to both being the penalty. Their collective name of *Agud* was, however, known to all.

I can only allude to the customary food-offering presented to the *zogole*, and the course of instruction instilled into the youths, one item of which was the narration of the legend of Malu, and must pass on to the recognition ceremony. This function took place in the afternoon on the sand beach outside the village of Las. A great concourse of people was assembled—men, women, and children—the newly initiated lads occupying the front row.

First four men of the dog-clan played about in pairs. (I may here parenthetically remark that it took me a fortnight's work to glean what little information I have respecting these two ceremonies. On one occasion I induced a number of men to rehearse some of the dances for me on the actual spot where they were originally performed, in order that I might gain a clear comprehension of them. One of my photographic "studies" I now throw on the screen.) The dog-men were followed by pigeon-men, who danced and beat their chests; later, whirling along the strand, came a body of dancers, circling from left to right as they advanced, an outer ring with sticks, an inner ring brandishing stone clubs, and possibly some drum players in the centre. Lastly, the three *zogole* appeared, completely covered with white feathers, and each carrying five wands. Although seen by the women, their identity was supposed to be unknown.

This was the final function, and was followed by the ever-recurring feast. Thenceforth the lads took standing as men.

Strangely enough, at neither Tud nor Mer could I discover that the bull-roarer was employed at these ceremonies. The widespread use and sacred character of this simple instrument has been emphasized by Mr. Lang in one of his charming essays. Knowing its universal distribution in Australia, I was not surprised to find that in Muralug, or Prince of Wales Island, which lies close to Cape York, its use was associated with the initiation of the lads. It was only by speaking in a low voice to the chief of the island and his son Georgie, whose photograph you have already seen, and by assuming more knowledge than I actually possessed, that I could induce them to admit of its being employed. Cautiously looking round to see that no one was near, its name, *wanes*, was whispered to me. After much persuasion, a model of one was made for me, on the express understanding that I should not show it to any woman on the island; and I did not. It is now in the British Museum. All that I could gather was that it was whirled in the bush and then shown to the lads. Death was the penalty to both if a man exhibited it to a woman, or to anyone who had not been initiated.

Great was my surprise when, shortly afterwards, I saw the Saibai boys who were staying at the mission station on Mer, playing with bull-roarers identical with the one with which I had been so secretly intrusted. The most sacred emblem in one island was a toy in another. In case some of you may not be acquainted with this most interesting implement, I have brought one of these bull-roarers.

From these important initiation ceremonies we may

pass to others which had a less sacred significance. All the native ceremonies were associated with processions, or with movements of a less regular character, the performers of which were invariably specially dressed for the occasion—usually there was a special costume for a particular rite, one distinguishing feature of which was the wearing of masks or head-dresses. It is convenient to describe these functions as dances; and a series can be traced extending from the most sacred initiation and funeral dances on the one hand, through the seasonal dances to the war and ordinary festive dances on the other.

Profanation of the initiation or of the funeral ceremonies was punished with immediate death. In some instances, at all events, dance-masks could only be worn at the appropriate festival; even the casual putting on of one was supposed to cause slow but certain death. It was my good fortune to witness a seasonal dance at Thursday Island. This was anticipatory of the fishing season during the north-west monsoon.

The men were clothed with a petticoat made of the shredded sprouting leaves of the coco-palm, and adorned with various armlets and leglets; but the striking part of the costume was the mask, of which the lower portion represented a conventional crocodile's head, surmounted by a human face; above this was a representation of a saw-fish, some five feet in length, and overtopping all was a long red triangular erection decked with feathers. The ceremony was called the *Waiitutu kap*, or "saw-fish dance." The actual dance consisted of two men at a time coming out from behind a screen and going through their simple evolutions to the monotonous accompaniment of the drum and a lugubrious chant.

More varied was the costume of the secular dance. All their bravery was donned. The effective head-dress of egret's feathers, or the cassowary coronet, framed the face, a shell skewer pierced the nose, breast ornaments, coco-palm leaf petticoats, armlets, leglets, ornaments or implements carried in the hand, all went to make up a picture of savage finery. Here, too, the women were occasionally allowed to participate, though of course both sexes never danced together. When women were allowed to be present at the more important dances, they were merely spectators.

The large canoes of the Torres Straits Islander of former times must have been very imposing objects when painted with red, white, and black, and decorated with white shells, black feathers, and flying streamers; and not less so when actively paddled by a noisy, gesticulating, naked crew, adorned with cassowary coronets, shell ornaments, and other native finery; or swiftly sailing, scudding before the wind with mat sails erect.

The body of a canoe is a simple dug-out, on to the sides of which gunwale boards are lashed. There is a central platform supported on a double outrigger. The thwart poles of the outriggers are usually six feet apart, and extend to some ten feet beyond the stem of the canoe; a doubly-pointed float is attached to the ends of the thwart poles on each side. Receptacles are built into each side of the platform for the storage of bows and arrows, fishing gear, water-bottles, and other belongings.

The sails are two in number, and are oblong erections of matting placed in the bows, some twelve feet in height, and each about five feet wide. The mats are skewered on to two long bamboos, which support the sails along their length; a bamboo stay also serves to keep the sail upright.

The longest canoe I measured was nearly sixty-eight feet in length. A stone lashed on to a rope is kept in the bow for an anchor. When sailing, a man stands in the stern holding the steering board.

The canoes are made at the mouth of the Fly River, in New Guinea, and are fitted with but a single outrigger, as theirs is only river navigation. I was informed that it was at Saibai that the canoes were re-fitted, this time with two outriggers, and an attempt at decoration was made, but the latter having a purely commercial significance was rather scant. The ultimate purchasers ornamented their canoes according to their fancy, as they usually prided themselves on having fine canoes.

I was much puzzled when I first went to Torres Straits by occasionally seeing a canoe with a single outrigger. I afterwards found it belonged to a native of Ware (one of the New Hebrides) residing at Mabuiag, and that he had re-outrigged a native canoe according to the fashion of his own people. When I was staying at Mabuiag some natives of that island were fitting up a canoe in imitation of this one. Here a foreign custom is being copied; how far it will spread among the Western Tribe it is impossible to say; but, strangely enough, the Eastern Tribe has entirely adopted an introduced fashion, and I did not see a solitary canoe with a double outrigger. It would be tedious to enter into a comparison between these various canoes. In the Eastern Islands the platform baskets are absent, and European sails are in universal use—mainsail, foresail, and jib. Among the Western Tribe, European sails have not yet quite supplanted the original mat sails. Throughout the Straits the canoes are not decorated in the old style. It was in Mabuiag alone that I found two canoes which were more or less decorated. Utilitarian ideas are now two widely spread for the æsthetic faculty to be indulged in.

I have dwelt at some length on this subject, as it is important to record all transitions. As an example of how rapidly and completely some changes occasionally come about, I may mention that at Mer, one of the Eastern Islands, some, at all events, of the young men did not appear to know that there had been a change in the rig of their canoes.

But, after all, the most interesting feature in connection with the canoes is the method by which they are purchased. I have previously mentioned that they were made on the mainland of New Guinea on the banks of the Fly River. Supposing a native of Muralug (Prince of Wales Island, the island which is nearest to Cape York) wants a canoe. He sends word, say, to a relation of his in Moa, for the inhabitants of these two islands often intermarry. The latter sends a message to the next island of Badu. A Badu man passes on the word to Mabuiag (these two also were intermarrying islands); the Mabuiag native informs a friend in Saibai, who in turn delivers the message at Mowat, on the mainland of New Guinea, or Daudai, as the islanders call it, thence the word passes along the coast till it reaches the canoe makers. As soon as the canoe is ready it retraverses the route of the order, being handed on from place to place, and island to island, until it at length reaches its destination. Should, however, there be a new canoe for sale on any of the intermediate stations, this might be sold, and thus obviate the tedious delay of waiting for one to be made to order. Another trade route is through Nagir and Tud to Mowat. The Murray Islanders send to Erub, and the natives of the latter island trade directly with Parem and the mouth of the Fly River. The most remarkable feature in these transactions is that payment is usually extended over three years; in fact, that canoes are purchased on the three years' hire system. This method of purchase, though but recently adopted by ourselves, has for an unknown period been practised by the naked islanders. The mere fact of its existence demonstrates a high level of commercial morality, for if the debts were often repudiated, the whole system would long ago have collapsed.

This commercial morality corroborates to a considerable extent the ethical standard said to be imparted to the youths during initiation. Nor would I like to say that they acted less up to their standard than we up to ours:

I doubt whether we would be much the gainers by a comparison. In making this statement it must be distinctly understood that I am only comparing their lives with their own ideals, and not judging them by the ethical standards of other races. It is true they were treacherous, often murdered strangers, and were head-hunters; that their ideas of sexual morality differed from ours, but these "crimes" were not prohibited by public conscience, and there was therefore no wrong in their committing them.

Our higher civilization has swept over these poor people like a flood, and denuded them of more than their barbarous customs; the old morality has largely gone too.[1]

FRENCH POLICE PHOTOGRAPHY.

M. ALPHONSE BERTILLON, who has so completely demonstrated the futility of the photograph as a means of judicial identification on any extended scale (see my description of M. Bertillon's system of police anthropometry in the *Fortnightly Review* for March last), when a mere mass of photographs is accumulated with no scientific scheme to aid them, has himself, nevertheless, done more than anyone else to develop and demonstrate the proper subordinate use of the photograph as an agent of the law. M. Bertillon's studies on the subject are not only most valuable to the members of the public administration, but are intensely interesting and instructive to the general reader, and the general scientific student especially, as will be readily acknowledged on a perusal of the young French official's latest publication.[2] He has not only offered me the privilege of making such extracts as I please from this work, but has kindly furnished me with some of the diagrams in the text. This new volume has already attracted considerable attention in France, and will doubtless be received with as much interest in England as have M. Bertillon's previous studies in the domain of anthropology, so that an account of the work in the columns of NATURE seems most opportune.

M. Bertillon begins by describing the sharp distinction between ordinary photography and judicial photography. Artistic and commercial photographs are subordinate to considerations of taste and fashion—not by any means for the purpose of recognizing the subjects of the photographs when met with in after time. The judicial photograph, on the other hand, takes no heed of artistic pose, but must conform to rules which enable the skilled eye to recognize the subject under the most unfavourable circumstances. It relates to various classes of subjects, some known and to be recognized hereafter, such as dangerous criminals; and some unknown and to be, if possible, identified by distant witnesses at the present time, such as suspected persons under arrest, corpses at the Morgue, the wandering insane, lost children, subjects of paralytic shocks, and innumerable human mysteries constantly falling into the hands of the police. The police are thus obliged to be constantly circulating photographs of their own manufacture, and it is of the utmost importance that such photographs should be taken upon the most scientific lines for accomplishing the object in view. Above all, in collecting vast numbers of judicial photographs for future reference—the photographic archives, "cantly" known in English as the "Rogues' Gallery" (though by no means confined to rogues in the eyes of the law)—it is important that the portraits should be taken with uniformity, the questions of full face or profile, full length or bust, &c., being decided beforehand, a fixed scale being adopted. Otherwise two photographs will be often of little use for purposes of comparison.

There is but one object to be attained, and that is easily analyzed—to produce the most perfect likeness, or rather to produce the likeness easiest to recognize, the one most easily identified with the original. The problem in this shape depends on a new factor: Under what circumstances and aspects did those who will be called upon to give an opinion on our photograph know our subject? and leads to this further question: What is the object sought by the judicial inquiry?

If it is a question of taking a sort of print of the individual which, together with his description and judicial record, will enable him to be identified after the lapse of many years, then above all things it is necessary to have recourse to the most lasting features of the human body, and to consult the natural sciences, more especially anthropology. If, on the other hand, it is a question of identification with the past—that is, that our photograph is destined to be compared with others that have been preserved in jails or police offices—the solution is very simple, and consists, above all other considerations, in reproducing the pose, the light, the size, and scale of reduction used in the archives to which our portrait is to be sent.

In regard to the important subject of light M. Bertillon speaks as follows:—"Absolute similarity is unfortunately unattainable. The aspect of the studio, the hour of sitting, the state, more or less cloudy, of the sky, will always betray themselves by the difference in the direction, and the greater or less intensity, of the shadows. We ought first of all to reject, as too complicated, all artistic or fantastic lights. For the full face the light should come principally from the left, a little in front. The pose chair and the apparatus being fixed to the floor at an unchangeable distance, we have for the profile but the direct front or back light to choose from. The light from behind gives more accentuation to the full face, and a more artistic tone. But the interior folds of the ear are necessarily in the shade, and the silhouette does not stand out so clearly as with a front light. The necessity of our profiles being taken with a front light, together with the early hour at which they are taken (so as not to interfere with the magistrates, whose work commences at 12), forces us to take the right profile to the exclusion of the left. In fact, the photograph studios generally facing north, and the sun being south-east between 10 and 11 o'clock, the left profile can only be lighted by a counter light to the camera. In a judicial studio, therefore, thus lighted from the north, the apparatus would be placed on the east side and the pose chair on the west, the work being done in the morning. By a curious coincidence, and no doubt from analogous causes, the greater number of ethnographic photographs of profile, especially those which compose the superb collection of Prince Roland Bonaparte, are taken from the right side."

The author next discusses the scale to be employed, advocating the necessity of including the shoulders, to show on occasion the crook-backed carpenter, or stiff Briton or Prussian (presumably contrasted with the supple Frenchman), preferring a reduction of 1 in 7 and a distance of 2·56 metres, various technical details being given for the benefit of the artist.

In his second chapter, M. Bertillon takes up the question of the use of the judicial photograph after it is obtained—firstly, as regards identification of two photographs; secondly, identification of a photograph with a person in custody; thirdly, with a person at liberty; lastly (the operation most familiar to the public), identification with a recollection in some one's mind. Of course, it is for this latter object that police portraits are strewn broadcast for the eye of the community at large.

[1] Further information as to customs and legends of the Torres Straits Islanders will be found in the Journal of the Anthropological Institute, vol. xix. 1890, and in *Folk-lore*, vol. i. 1890.

[2] "La Photographie Judiciaire, avec un appendice sur la classification et l'identification anthropométriques." Par Alphonse Bertillon, Chef du Service d'Identification de la Préfecture de Police. (Paris: Gauthier-Villars et fils, 1890.)

But the chief exercise of this function in the ranks of the police themselves is the search among their store of portraits for a person of whom they receive a description, the crucial points in this description being set out, and specially the dangers to be avoided. Thus certain colours of hair and complexion make photographs almost unrecognizable, and peculiarities of gesture and movement are so characteristic of some persons as to make mere immobile portraits little suggestive even to familiar acquaintances. As regards comparison between two photographs, the author calls attention to the points which should prevent two apparently dissimilar commercial photographs from being pronounced different subjects; and, on the other hand, the striking family likenesses which should make one careful in declaring two similar portraits the same person. M. Bertillon gives the clue to the physiological data which should govern judgment on these occasions. He illustrates a clever contrivance (but lately borrowed from their French brethren by the English police) by which a newly-taken portrait of a person in custody and an old portrait are compared on equal terms by a covering up of all but the unchangeable portions of the face—hair, beard, and moustaches being obliterated. M. Bertillon makes his most daring speculations, however, in relation to identification of a person at large from a photograph in hand. He says, even as the word "chime" is not conveyed to the brain without a sensation not only of sight of the bells but the sound as well of its ring, so identification should come from certain clue characters of personal appearance, suggesting the absolute identity. It is of no use to sit down and study in detail a photograph which probably tallies in few points with the same person as he is likely to be encountered abroad; but the unchangeable individual data must be seized upon by the trained mind versed in the language of anthropology, so that an encounter with the desired object will never fail to tell the secret. Thus, in the frequent necessity of stopping persons on embarkation at a sea-port, the profile is, of course, the thing to be kept before the inspecting eye; but even this must be understood in relation to the disguise of bearing, expression, &c., all of which must be considered—not the mere photograph taken under far different circumstances. M. Bertillon concludes this interesting section as follows:—"The officer charged with so difficult a mission as the search for, and arrest of, a criminal by the aid of a photograph, should be able to repeat and write from memory the description of the face of the man he is in search of. It is the best means of proving to his chiefs that he has at heart the task confided to him. The reader will see later the special terminology which a knowledge of the subject necessitates. More than one of our readers will be surprised to see that police science borrows some of its methods from natural history and mathematics. We think that this descriptive study of the human frame will interest the portrait photographer as much as it will the judicial inquirer. Are not both scrutinizers of the human physiognomy, though truly from a different point of view?"

The author's third chapter is devoted to other applications of photography to judicial purposes. Here he refers to the notorious pocket cameras, which he puts aside as rarely of much use for police purposes, it being at a critical moment more of an object to capture a malefactor than to photograph him. Still, he admits an occasional value for this kind of photography, and gives a startling example of the scene of a most dramatic murder in the suburbs of Paris at the instant of its discovery, before anything of the surroundings had been disturbed. This, M. Bertillon contends, would naturally be most valuable in the hands of the prosecution. Numerous other uses of photography are mentioned, such as cases of mine accidents, traces in the snow before it melts, and other matters of future judicial investigation. Many objects connected with crime may become the subject of photography, such as weapons and portions of dress; and photochromography comes in opportunely to spread abroad not only the form but the colour of the articles considered important in tracing a criminal, so that evidence as to such articles may be forthcoming.

In his appendix, M. Bertillon gives an interesting summary analysis of the human figure, based on the studies of Quételet; but as this is only indirectly connected with the subject in hand, I will only refer to the two features on the importance of which in judicial investigation the author lays the greatest emphasis—namely, the nose and the ear. That important and delicate subject over which so much concern is evinced in the social circle and the domain of literature—the human nose—M. Bertillon considers equally worthy of prominence in anatomical study and police practice. He offers a scheme whereby noses may be studied with profit to the judicial mind, discarding the considerations which chiefly appeal to the ordinary eye in comparisons—colour, size, and general shape—and confining the classification to the line profile pure and simple, apart from all other elements. M. Bertillon makes fifteen classes, into which all noses, even the most eccentric probosces, may be sorted: firstly, three grand divisions—the *elevated*, the *horizontal*, and the *drooping*, according to the nature of the base-line; each of these to be again divided by the bridge line into *concave*, *straight*, *convex* (or curved), and, lastly, *undulating* (wavy, broken, or irregular in outline). The detective or judicial functionary, when called upon to say whether a face under his surveillance corresponds with a photograph in question, will find great help in a thorough nasal analysis, for two noses are never exactly alike.

Yet more important is the ear, which, M. Bertillon insists, should always be shown in the portrait. His remarks on this feature are so valuable that I will conclude my summary of his unique little volume by an abstract from this portion, illustrated by the accompanying diagrams, the use of which has been so kindly allowed me by the author:—

"We will close our examination of the profile by studying the ear, which, thanks to the projections and depressions with which it abounds, is the most important factor in the problem of identification. It is all but impossible to find two ears identically similar in all their parts, and the variations in the conformation which this organ presents appear to remain without modification from birth until death. We believe that the registration at birth of certain peculiarities in the ear would render any substitution of persons, even when adults, impossible. From birth unchangeable in its form, uninfluenced by surroundings or education, this organ remains throughout life like the untangible legacy of heredity and interuterine life. Nevertheless, on account of its immobility itself, which prevents its taking any part in the play of features, no part of the face less attracts the attention of the profane. Our eye is as little accustomed to notice it as our tongue is to describe it. In fact, the denominations of the principal parts of which it is composed have been but very summarily described in most of the anatomical treatises.

"It will be sufficient for us to confine ourselves to the prominences which border the depressions, to give a good idea of the latter, and it will enable us to shorten our description by one-half. The prominences are five in number.

"(1) The border of the ear, or helix, a semicircular projection commencing at A (Fig. 1) in the middle of the ear, above the auditory passage, reaching to the periphery, and bordering two-thirds of the upper ear.

"(2) Where it ceases, the lobe commences soft and rounded, terminating at the base the circumference of the ear.

"(3) Then the tragus—small, flat, triangular, cartilaginous prominence—placed outside in front of the auditory

passage. Its shape presents but few individual variations.

"(4) Opposite, separated by the auditory passage, is the antitragus, less prominent than the tragus, but of far greater descriptive value.

"(5) Finally, above it the fold of the anthelix, which, after rising about 1 centimetre, bifurcates in two branches, the upper and the horizontal, the latter rejoining the helix above its original starting-point.

"The above order of enumeration enables us to draw the different contours of the ear without raising our pencil, starting from point A, and finishing at point M; this course of the pencil is shown on Fig. 1 by the alphabetical order of the capital letters, which separate each of the subdivisions, of which we are about to describe the most characteristic morphologic variations.

"The border may be divided into four parts—the starting-point, A B; the anti-superior part, B C; the posterior, C D; and the final, D E. Each of these parts may vary independently—that is to say, may be small, medium, or large. It also happens pretty frequently that the beginning and ending portions (A B or D E) are altogether missing; at other times the portion C D is more fully developed than the superior or upper part, or less so. The irregularities of contour that result therefrom are very characteristic. Lastly, the final part, D E, may

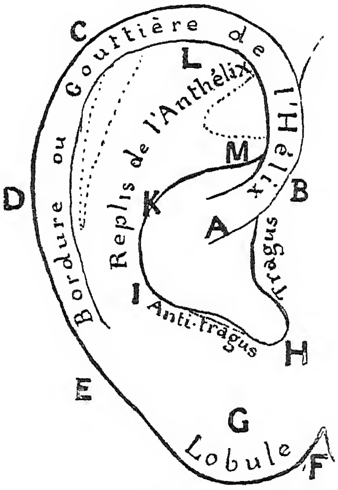

Fig. 1.

be very much developed and continue round the lobe to the cheek.

"The *lobe* should be considered—

"(*a*) The outline of its free edge, E F, which may terminate in a descending point, and attached to the cheek, or squared, or in rounded ellipsoid.

(*b*) "Its degree of adherence to the cheek, F H, which we called 'fused,' or it may be joined by a membranous fold, which only becomes visible when the ear is stretched from the cheek. Finally, it may be entirely separated from the cheek.

"(*c*) Of the shape of the anti-exterior surface, G, which may be traversed by the prolongation of the helix, level or mammilated.

"(*d*) Of its dimensions in height, which may be small, medium, or large.

"The *antitragus* presents a general line of direction, the inclination of which may vary from horizontal (the head being in its normal position) to obliquity of 45°. In relation to this line, represented in the drawing by the dotted line H I, the antitragus can profile in line with an upper concavity, or rectilinearly, or slightly sinuous, or projecting. Finally, the antitragus (especially its free extremity) may be inverted outwards or straight. Putting aside all questions of shape, the antitragus may vary also with reference to its indefinite dimensions.

"The parts I K and K L of the fold of the *anthelix* may each separately be small, medium, or large. When the anthelix, and specially the upper branch, K L, is little accentuated, the ear stands out from the head, and takes a shape which resembles the ear of the mammifer. The horizontal portion, K M, of the anthelix has a bearing sometimes truly horizontal, sometimes oblique, sometimes intermediate.

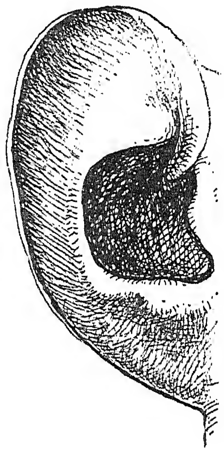

Fig. 2.—Ear showing all the characteristics at a minimum.

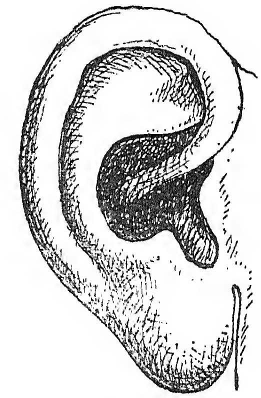

Fig. 3.—Ear showing all the characteristics at a maximum.

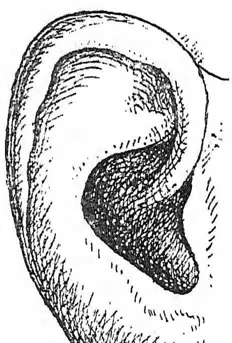

Fig. 4.

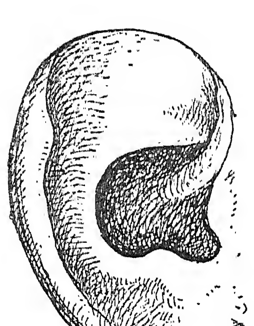

Fig. 5.

Figs. 4 and 5.—Ears showing the corresponding peculiarities alternately at a minimum and at a maximum.

"The whole ear, including the lobe, may equally deflect from the head; hence the shape called "peduncular" by some authors. In other cases the deflection is most noticeable in the posterior part, or upper, or even in the lower part of the ear. We must also notice the presence of a prominence in a certain number of ears, between the points C and D. This protuberance is called 'the tubercle of Darwin,' after the celebrated English naturalist, who saw in it a survival of the pointed ear of certain monkeys (Fig. 5)."

It is to be hoped that M. Bertillon's work will be seen in English, for I have given but a fragment of the choice, fresh matter in the little volume.

Edmund R. Spearman.

THE CINQUEMANI "CHRONOLOGE."

THIS is a very singular and interesting contrivance. It is a clock with only one toothed wheel, yet it shows the hours, minutes, days of the week, &c., and strikes the hours and quarters at each quarter of an hour. Moreover, there is an arrangement for repeating the hours and quarters at will. The single toothed wheel spoken of is the escape-wheel, and this propels a pair of pallets and pendulum in the ordinary way. The rest of the work is done in the fall of a small leaden ball, a long chain of these balls being intermittingly elevated, and one of them discharged over a revolving drum each quarter of an hour. We will follow one of these balls through the course of its multifarious duties. It first enters a sling in a tape wound over the escape-wheel axle, and we notice that it is the weight of this and three other balls (which have been previously deposited in preceding slings) which is keeping the escape-wheel going. As the wheel turns round, the balls descend, and after a quarter of an hour the lowest will have arrived at a funnel-shaped opening, where it will get liberated from its sling, and fall. It first strikes a lever which enables the drum to move on and discharge another ball into a sling upon the escape-wheel tape. Then rushing down a tube it enters a zigzag. It is within this zigzag that the striking of the quarters is performed, for at each of its angles a bell is placed, against which the ball strikes sharply as it passes them. After leaving this zigzag, the ball is projected down another, where it strikes the hours. As the number of blows to be struck is regulated by a similar contrivance at each zigzag, we will confine our attention to that for the hours. The channel down which the ball passes is vertical to the face of the zigzag. Now the front or zigzag side of this channel is a moving tape, which carries a little trap. As the tape is always moving, the position of the trap depends upon the time, and the position of the trap also determines the stage of the zigzag upon which the ball will be projected. Thus, when the trap is opposite the sixth stage of the zigzag, the ball will encounter six corners upon its way down, and consequently six blows will be sounded. When the trap is at the top, twelve blows are sounded; and when the trap is at the bottom, no blows are sounded. When the ball leaves the zigzag, it enters a sling at the lowest part of the chain first spoken of, and is intermittingly carried up again to begin its work over again. For repeating the hours and quarters at will, there is a separate reservoir of smaller balls; and by pulling a handle one of these can be discharged above the first zigzag, and when it has done its work it disappears through a hole, which the regular balls cannot penetrate, back to its own reservoir. It may be mentioned that, in lieu of bells, the hour zigzag has a single vertical sonorous tube for each set of corners. The time, days of the week, &c., are shown by means of tapes carrying pointers suspended over the escape-wheel and another axle. The inventor, the Rev. Canon Cinquemani, maintains that the simplicity and precision, by reason of the constant force on the escapement of his "chronologe" (which he has patented), render it peculiarly advantageous for missionary and other distant stations, where the assistance of professional clockmakers is not readily procurable. H. D. G.

THE NEW AUSTRALIAN MAMMAL.

IN vol. xxxviii. of NATURE (p. 588), Dr. E. C. Stirling. of Adelaide University, described as a "new Australian mammal" a small mole-like animal which had been obtained in Central Australia near the telegraph line between Adelaide and Port Darwin. The same description, with some additions, was afterwards published in the Transactions and Proceedings of the Royal Society of South Australia, vol. x. p. 21. But no decision was arrived at as to the exact affinities of this animal—not even whether it is a Marsupial or a Monotreme—nor has any name been given to it. On behalf of the zoologists of this country, who have waited patiently two years for further information, I now venture to urge Dr. Stirling to send one of his specimens of this extraordinary creature (in a letter subsequently addressed to me he speaks of having received two additional examples) to London, and allow us to endeavour to decide what it really is. I need not point out the extraordinary interest of this discovery. If a Monotreme, as seems probable, it will be the third known form of this very peculiar type of mammal-life; if a Marsupial, it is quite different from all known members of that group; and if it turns out to be a Placental Mammal, it will revolutionize our canons of zoological geography. On behalf of the Zoological Society of London, I think I may promise that the specimen, if forwarded, shall be submitted for examination to our very best authorities on the subject, and shall be fully described and illustrated in our scientific publications. Such a grand discovery should certainly not be concealed from the world's knowledge any longer.

P. L. SCLATER.

RICHARD BURTON.

WE have already announced the not unexpected death, at the age of 69, of Sir Richard Burton, one of the most versatile geniuses and extensive explorers of any time, and one who, so far as Africa is concerned, deserves to be ranked with Stanley and Livingstone. He was born on March 19, 1821, at Barham House, Herts, of old families on both sides, and with a heritage of fighting and wandering propensities. It is curious now to think that Burton was sent to Oxford with a view to taking orders. He soon destroyed all prospects of any such career by getting himself rusticated. He succeeded in obtaining an appointment in an Indian regiment, and, while yet little more than a boy, his restless wanderings began. For half a century Burton lived a life of the fiercest intensity—equal to the lives of three ordinary men. Before his famous journey to Mecca he had published more than one book on his travels in India and neighbouring countries. Before attempting the hazardous enterprise to the holy city of the Moslems, in 1852, he took every precaution to delude his fellow-pilgrims into the belief that he was one of the faithful. His extraordinary gift of picking up languages made this easy; and whether his disguise was or was not penetrated, he succeeded in getting inside, and, better still, outside Mecca, to tell an expectant world of mysteries practically unrevealed before. This journey would certainly have made his name famous; but he meant to do even greater things. At that time it was as dangerous to attempt to enter fanatical Harrar as it was for a Christian to join the Haj. But Burton did it, and lived to tell the world the story of it; though he and Speke had a narrow escape when, the following year (1855), they attempted to reach the Nile through the Somali country.

A preliminary trip to Zanzibar produced a learned and interesting book on that island and its people. We say preliminary, because by this time, 1856, Burton had something much more important in view. Livingstone, it should be remembered, had been at work in Africa for many years; in 1856 he returned to England to tell the full story of his crossing of the continent. Through Livingstone, through Krapf and Rebmann, and others, rumours had been for a long time coming out of great lakes in the interior. Before D'Anville, in the end of last century, made a clean sweep of all the crowded features on the map of Central Africa which had accumulated since the end of the sixteenth century, there were lakes in plenty, scattered over the centre of the continent, and great rivers and mountain ranges, some of them an inheritance from the days of Ptolemy. But no one knew how these features ever came there. The hydrography they in-

dicated was impossible; and there was no evidence that any white traveller had ever seen them. The probability is that these lakes and rivers were put down from the reports of natives who had communications with the interior. Much of the existing geography of Africa rests on no better foundation; but then we know better how to sift native reports now than our predecessors did 200 years ago. At all events, as some of us who were at school then may remember, the map of Africa, in 1856, had the word "Unexplored" spread all over its centre. As has been said, Krapf and Rebmann, the missionaries, who had seen Kilimanjaro, and thought they got a glimpse of Mount Kenia afar off, had heard of great lakes in the heart of Africa. It was to seek these lakes that Burton, accompanied by Captain Speke, set out from Zanzibar in June 1857. The expedition was under the auspices of the Government and of the Royal Geographical Society. Before leaving Zanzibar Burton wrote home that he was about to set out in search of "the Great Lake." His eyes were gladdened by the sight of the waters of Tanganyika, at Ujiji, on February 14, 1858. It is scarcely possible for us to realize what this meant at the time. The route, now so well known, from Zanzibar to Ujiji had never before been trodden by the feet of white men. The difficulties which beset this pioneer expedition were disheartening. Before it set out, there was no Tanganyika, no Victoria Nyanza, no Albert Nyanza, no Bangweolo on the map, and only the lower 200 miles of the Congo. Burton's discovery of Lake Tanganyika may be regarded as the centre from which all succeeding discoveries in Central Africa have radiated. It is the great central lake round which all others are grouped. Indeed Burton's companion, Speke, as we know, made a run to the north on the homeward route, and discovered that other great lake, the Victoria Nyanza, which he rightly surmised to be the source of the White Nile. Of the unhappy relations between Burton and Speke this is not the place to write, even if we had any inclination to revive a bitter controversy that ought to be allowed to lie in the grave where it was placed many years ago. That Burton's bitterness against Speke blinded him to the importance of his companion's discovery all will admit. That Burton was of a rough type, given, like other great and successful men, to carrying out his purposes at any cost to themselves and others, there can be no doubt. The big things in the world have generally been accomplished by such men.

Burton's discovery gained him the medal of the Royal Geographical Society, but hardly anything else. After a run to America, he, in 1861, with his newly-married wife, went as Consul to the White Man's Grave—Fernando Po. From here he explored the Cameroons, the Gorilla country, and Dahomey. A few years later a Consulate in Brazil gave him the opportunity of exploring the highlands of that country. After a short stay at Damascus, Burton was appointed Consul at Trieste in 1872, and there he was allowed to vegetate till his death, with no greater reward for all his valuable services to science than a K.C.M.G., given him four years ago. Visits to Iceland, to Midian, and to the Gold Coast, produced several volumes to add to the many he had already published; probably no traveller has ever been so prolific in books. It says little for the intelligence and enterprise of a Government that could find no better use for the services of a man of such power as Richard Burton than to give him the charge of a third-rate Consulate. Of Burton's versatile scholarship and its published results we need not speak in detail. He was one of the few survivors of the old type of adventurer of which our country has been so prolific—men who have been the makers of our Empire and the founders of modern knowledge. Science is bound to remember him as one of her pioneers into the great unknown.

K.

PROFESSOR HEINRICH WILL.

The sad announcement of the death of this well-known chemist from heart disease, on the 15th of this month, is made in the *Chemiker Zeitung* of the 22nd inst. Dr. Will was for thirty years Professor of Chemistry and Director of the Laboratories at the University of Giessen. He was born on December 8 in the memorable year 1812, at Weinheim, where his father held an important official position. After completing his studies at the High School of his native town, he devoted himself for a time to pharmacy. But in 1834 he entered the University of Heidelberg, and in the same year undertook the position of assistant in the laboratory under Prof. Geiger, and after that eminent chemist's decease, in 1836, under the celebrated Prof. L. Gmelin. In 1837, at the request of Prof. von Liebig, he removed to Giessen, where he occupied the position of assistant until his graduation as Doctor in 1839. He then habilitated himself at the University as Privat-docent of Chemistry, his dissertation consisting of a description of his "Investigation of the Constitution of the Ethereal Oil of Black Mustard." In 1842, Dr. Will undertook the direction of the newly-founded Filialslaboratorium, and in 1846 he received a call to the then recently inaugurated laboratory of the College of Chemistry in London. He, however, declined the offer, and was shortly afterwards appointed extraordinary Professor in the University of Giessen. After Prof. von Liebig's departure for Munich, in 1852, Dr. Will became ordinary Professor of Chemistry and Director of the Chemical Laboratories of the University. During the session 1869-70, Prof. Will occupied the distinguished post of Rector of the University, and his inaugural address was a memorable one, treating of the relations between matter and force considered from the chemical standpoint. After forty years' unceasing labour as a teacher and an investigator he retired, at his own request, in October 1882.

As an original investigator Prof. Will was characterized by his precision and the acuteness of his observation. He was also a most excellent teacher, understanding as few others of his time the art of explaining to students that which was so clear to himself. What, however, most struck those who had the good fortune to listen to his lectures, was the deep earnestness which he threw into his subject, and the manner in which he used to carry his students along with him through the most intricate branches of chemistry. His powerfully energetic character was even more apparent if possible in the laboratory, as he passed from student to student, speaking the right word of help and encouragement to each, and inculcating habits of work and thought which raised many of those students to positions of honour and usefulness in the chemical world. His especial fitness for the leadership of a laboratory is very manifest from a perusal of his textbook, "Anleitung zur chemischen Analyse," which appeared in its twelfth edition in 1883, and has been translated into several languages.

A. E. T.

NOTES.

The Queen has been pleased to command that the Government institution now known as the Normal School of Science and Royal School of Mines shall in future be called the Royal College of Science, London.

The President of the Institution of Electrical Engineers and Mrs. John Hopkinson will give a *conversazione* in the galleries of the Royal Institute of Painters in Water Colours on Wednesday evening, November 19.

The death of Robert M'Cormick, F.R.C.S., R.N., Deputy Inspector-General of Hospitals and Fleets, is announced. He was one of the oldest and most eminent officers of the medical

profession in the Royal Navy. He accompanied Sir E. Parry in H.M.S. *Hecla* in his attempt to reach the North Pole, and in 1839 joined the *Erebus*, which, in company with the *Terror*, was employed in the expedition for magnetic observation and discovery in the South Polar regions. During this voyage, which lasted four years, he discharged, in addition to his medical work, the duties of geologist and zoologist. In 1847 he called the attention of the Admiralty to the fate of Sir John Franklin, and laid before the Board plans of search for the missing vessels. His plans were accepted in 1852, and in the course of his subsequent exploration he settled various geographical questions. In 1857 he received the Arctic medal. Among his writings are: "Geology of Tasmania, New Zealand, Antarctic Continent, and Isles of the South," "Voyages of Discovery in the Arctic and Antarctic Seas," and "Round the World, with an Open Boat Expedition, in the *Forlorn Hope*, in Search of Franklin."

WE have to record the death of Mr. E. C. Nicholson, F.C.S., who was well known as a manufacturing chemist. He died on the 23rd inst., at the age of sixty-three.

A SCIENTIFIC and commercial expedition to the West Coast of Africa, under the auspices of the British Government, is about to leave London. Commander V. Lovett Cameron has been appointed chief of the staff, the whole expedition being under the superintendence of Mr. James Bennett, of the firm of E. C. Bennett and Co.

STUDENTS of Egyptian archæology will be glad to learn that the Catalogue of the Gizeh Museum will be published in January.

THERE have been some unpleasant rumours lately about the destruction of the Pyramids for building material. The Cairo correspondent of the *Times* says there is no truth in these reports. The real facts are that the loose stones accumulated at the base are being removed, which will lay bare the lowest courses and display the Pyramids to greater advantage. The work is being conducted under the superintendence of the Museum authorities.

THE botanical explorer, Mr. C. C. Pringle, was engaged, during the early part of this year, in investigating the high land between Mexico and Tarupico; he has made large collections, including, as he believes, many new species.

DR. R. VON WETTSTEIN returned in July from his botanical expedition to Tuzla, Zwornik, Vlasenica, and Srebrenica in Eastern Bosnia. He has obtained interesting results, which will be published in the *Oesterreichische botanische Zeitschrift*.

ON October 16, Prof. Wallace delivered the inaugural address to the class of agriculture and rural economy at the Heriot-Watt College, Edinburgh. He chose as his subject dairy practice. The address has now been published.

THE October number of the *Auk*, which completes the seventh volume of that publication, is a somewhat bulky part, and besides the usual excellent papers on North American ornithology, it contains some essays by European naturalists. Dr. A. B. Meyer describes a new species of Humming Bird (*Eriocnemis aurea*) from Colombia. Mr. Eagle Clark gives an account of a collection of birds made by Dr. Gillespie at Fort Churchill, Hudson's Bay, and presented to the Edinburgh Museum in 1845. Mr. J. A. Allen describes a new *Icterus* from Andros Island in the Bahamas, as *Icterus northropi*. Mr. D. G. Elliot also publishes the first portion of his description of a collection of birds obtained by Mr. C. F. Adams at Sandakan in North-Eastern Borneo. This paper contains many errors, and the author is evidently not acquainted with the recent literature on the subject. *Copsychus adamsi*, sp. n., is certainly *C. niger* of Wardlaw Ramsay (P.Z.S., 1886, p. 123). Mr. Seebohm, not Mr. Sharpe, is the author of the fifth volume of the "Catalogue of Birds," and he will probably be interested in the occurrence of *Geocichla interpres* in Borneo, which is here recorded for the first time. *Pitta venusta* of Mr. Elliot's paper will assuredly prove to be *Pitta ussheri*, already recorded from Sandakan, by Mr. Bowdler Sharpe, in the Proceedings of the Zoological Society for 1881, a paper which seems to have escaped Mr. Elliot's notice. He will also find Mr. Alfred Everett's "List of the Birds of Borneo" of great use to him in his future studies.

THE authorities of the Government Central Museum, Madras, are having an index collection made on the same principle as that adopted in the British Museum (Natural History). This is noted in the Administration Report of the Museum for the year 1889-90. The index collection, in its complete state, should teach the most important points in the structure of the principal types of animal and plant life, and the terms used in describing them. The series exhibited during the past year illustrated, by means of specimens with descriptive labels, arranged in wall and table cases, the outer covering or integument of mammals and its modification into hair, nails, claws, hoofs, horns, antlers, &c. Other series are in course of preparation, illustrating the dentition and osteology of mammals, the external characters and osteology of birds, the structure and forms of shells, mimicry, &c. The exhibition of these series, the Superintendent hopes, will be of use both to those who are engaged in teaching, and to students in Madras, and bring the Museum more into touch with the Educational Department than it has been hitherto.

SOUNDINGS have lately been carried on in the Straits of Dover, in connection with the proposal for the construction of a bridge across the English Channel. According to a telegram sent through Reuter's Agency from Paris, the result of the surveys made shows that the route which has been investigated is a little shorter than was expected, that it presents every guarantee as regards solidity and stability, and that the depths are not quite so great as was anticipated. M. Renaud, a hydrographic engineer, who was designated for the work by the French Minister of Marine, is of opinion that if the Bridge Company went a little further north a perfectly straight route could be obtained with a better foundation and less depth of water. This route would be four kilometres shorter, and would therefore considerably decrease the cost of construction. It also appears to be less exposed to the wind, which would prove a great advantage, especially during the progress of the works.

THE U.S. National Museum has issued its Thirty-eighth Bulletin. The work consists of a valuable contribution, by Prof. J. B. Smith, towards a monograph of the insects of the Lepidopterous family Noctuidæ, of Temperate North America.

SOME time ago a memorial on the decimal system was presented to the London School Board by the Decimal Association. The Board have now informed the Association that, on the recommendation of the School Management Committee, they have asked the Education Department to modify Schedule I of the new Code, so that decimal fractions shall be taught in the fourth standard at latest, and the metric system of measurement and weight be included in the teaching of the fourth and upper standards. The School Management Committee of the Board have also decided that models illustrating the metric system shall be added to the Board's requisition list in the event of the Education Department accepting the proposal of the Board.

MESSRS. J. B. BAILLIÈRE ET FILS are issuing, in weekly parts, a work on the various races of mankind ("Les Races Humaines"), by Dr. R. Verneau. It will complete the work entitled "Merveilles de la Nature," for which Brehm wrote the

"Vie des Animaux." The first part of the new work has been sent to us. The author arranges his facts clearly, and there are some good illustrations.

Mr. Fisher Unwin has published an interesting volume on "Teneriffe," by George W. Strettell. It is an expansion of a paper read by the author before the Congress at Brighton last August. Mr. Strettell records his personal experiences, and in describing Teneriffe as a health resort, avoids, with equal care, extravagant laudation on the one hand, and undue depreciation on the other.

The new volume of the "Minerva Library" consists of reprints of Darwin's "Structure and Distribution of Coral Reefs"; his "Volcanic Islands"; and his "Geological Observations on South America." Prof. J. W. Judd contributes a critical introduction to each work.

A correspondent inquires as to the titles of any works in the nature of scientific guide-books to Switzerland and the neighbouring countries, and to the usual tourist resorts for health, &c.—which deal with their chief features of botany, zoology, geology, ethnology, &c. The books should not exceed the size or cost of ordinary guide-books, and may be French or English. We shall be glad to print any titles that may be sent to us.

A somewhat severe shock of earthquake occurred at Hechingen on October 14, at 2.30 a.m. At Nexö, on the Island of Bornholm, a slight rumbling of earthquake was noticeable for almost an hour on October 8, the same day on which several shocks were felt in Norway.

The formation of icebergs was watched, this last summer, by Mr. H. B. Loomis and Prof. Muir, while staying seven weeks near the Muir Glacier (*Amer. Journ. of Science*). The falling of blocks from the terminal wall was very irregular: at times, about every five minutes; while at other times the observer might wait an hour without seeing one fall. One day, in twelve hours, 129 thundering reports from the falling bergs were heard at camp, about a mile off. In heavy rain, especially, it seemed as if a thunderstorm or cannonade were going on. Sometimes a block, breaking off, bursts into fragments, and falls like a cataract. Again, an enormous block will sink unbroken into the water, then rise, perhaps 250 feet, even with the top of the glacier, the water pouring off it; then topple on its side with a heavy thundering roar, scattering spray in all directions, and wallow about among other icebergs like a huge monster.

A pamphlet on "The Law of Storms," considered with special reference to the North Atlantic, has been sent to us by the author, Mr. Everett Hayden. It is an abstract of a paper read by him before the National Geographic Society in November last. Hurricanes are most frequent in the summer months in each hemisphere. Originating in the tropics they move westward, then poleward, and finally eastward in higher latitudes, gradually receding from the equator. Between hurricanes north and south of the line the essential difference is that in the northern hemisphere the rotation of the cyclonic whirl is against the hands of a watch and in the southern with them. The author goes on to say that the noted hurricane regions are the West Indies, coast of China and Japan, Bay of Bengal (especially in May and October at the time of the change of the monsoons), and the South Indian Ocean (about Mauritius). In the brief statements and descriptions regarding hurricanes and storms, including some of the latest hurricanes that have occurred in the last two or three years, the most recent, most important, and best established facts, which every navigator ought to know, have been written concisely and intelligibly. Accompanying these descriptions are charts which illustrate clearly the wind currents and barometric depressions. To explain the great cloud bank, and the storm wave or general elevation of the sea caused by the spirally in-blowing winds and low barometer, the author has given a very neat little sketch in cross-section, and a second sketch is added to convey a clear mental conception of the actual motions of the particles of air as they flow inwards from below, their whirl upwards and flow outwards at the top.

In a paper on moles, lately read before the Bristol Naturalists' Society, and now printed in its Proceedings, Mr. C. I. Trusted calls attention to the fact that there are said to be no moles in Ireland. He has never seen a mole-hill in that country, and an acquaintance of his at Belfast—"a good and observant naturalist"—says, "It *is* a fact that moles do not exist in Ireland." Yet, as Mr. Trusted points out, there are in many parts of Ireland wide districts which seem to be well suited to the mole's habits.

The Department of Public Instruction, in New South Wales, have printed in their technical education series a valuable paper on wattles and wattle-barks, by J. H. Maiden. In an introductory note the Minister for Public Instruction says that experiments in wattle-culture in Victoria and South Australia have resulted in a practical success. He knows of no sound reason why similar enterprise should not be equally profitable in New South Wales, which has, in many parts, soil and climate well adapted for this industry.

Some persons digging peat near the village of Fochterloo, Friesland, lately came across a sunken forest of trees with enormous trunks. The trees are lying on a sandy soil, in the direction from north-west to south-east; it is not yet decided to what species they belong. The exteriors resemble oak, but the insides are brittle, and burn like tinder.

We have received the third edition, just published, of a general list of observatories, astronomers, astronomical societies, and astronomical reviews, prepared by Mr. A. Lancaster, librarian of the Royal Observatory of Brussels. Under the heading of each observatory will be found its latitude and longitude, the names of all those who compose its staff, and its yearly publications. Under astronomical societies the information given relates to the following: date of foundation, object, memoirs published, and names of the president, secretary, and treasurer. The next part is headed, "Institutions diverses," and includes—among other institutions—the Bureau of Longitudes of France at Paris, Bureau of the *Nautical Almanac* at London, Bureau of the *Nautical Almanac* at Washington, Solar Physics Committee, &c. The staff employed or members in each of these institutions is given, together with the yearly publications. The fourth section deals with astronomical reviews and journals, and the information that is brought together mentions the name of the editor, the price, the frequency of publication, and the date of first appearance. The fifth and sixth sections consist of alphabetical lists, with addresses, of astronomers and instrument makers. With the help of the good general alphabetical table that is added at the end, the book will be sure to be found very handy and useful for reference.

R. Friedländer and Son, Berlin N.W., 11 Carlstrasse, have issued part xxiii. of their "Catalogues of Books." It contains titles of a large number of important works dealing with every branch of astronomical science, hence it will be of great use to those in search of rare books, and also to the general bibliographer.

We have received the sixth fascicule of the "Works of the Aral-Caspian Expedition," which contains the geological diary of Prof. Barbot de Marny during the Expedition. This diary, which unhappily remained unpublished for thirteen years after the Professor's death, is rich in accurate observations, which

have lost none of their value, notwithstanding subsequent exploration; and the whole gives a vivid description of the explored region, from the Caspian Sea, over the Ust-Urt plateau, to the mouth of the Amu-daria and to Samarkand. Of the many short notes scattered in the diary we may select one in which it is mentioned that Barbot de Marny has had the opportunity of finally ascertaining that the so-called *bugry* of the Caspian shore are simply due to denudation. The layers of clay and sand of which they consist are mostly horizontal, and, when they are not so, the stratification has no relation whatever to the exterior shape of these elongated low ridges. The next contribution to the same fascicule is by M. Andrusoff. It is full of geological data, and its conclusions are very interesting, the author never failing to discriminate between what is already proved and what still belongs to the domain of hypothesis. His remarks about the Caspian Sea and its present fauna being remains of a Miocene sea, and the enumeration of the geological problems in connection with that fact which remain yet unsettled, will be read by geologists with interest, the more so as the substance of this paper has been given in German in the *Jahrbuch der k.k. Geologischen Reichsanstalt*, vol. xxxviii.

THE following are the arrangements for Tuesday evening lectures at the Royal Victoria Hall during November:—November 4, Mr. Arnold Mitchell, "Old Buildings and the Story they tell"; November 11, Mr. A. H. Gilkes, "Columbus"; November 18, Mr. A. P. Laurie, "Air and Water," with experiments; November 25, Mr. Hilliard Atteridge, "The New Divisions of Africa." The oxyhydrogen lantern will be used with all these lectures.

IN our note on the Rev. J. A. Galbraith, on October 23, the ast three lines should have been printed as follows:—"In 1854 he was chosen Erasmus Smith Professor of Experimental Philosophy. Along with Dr. Haughton, Prof. Galbraith was the author of various excellent scientific manuals."

IN a communication to the current number of the *Comptes rendus*, M. Moissan announces the result of his redetermination of the atomic weight of fluorine. The method adopted consisted in converting a known weight of sodium, calcium, or barium fluoride, prepared in a manner specially devised by M. Moissan in order to exclude impurities, into sulphate by repeated ignition with pure sulphuric acid in a small platinum retort. The process for obtaining pure sodium fluoride was as follows. An already fairly pure specimen of sodium chloride was freed from the last traces of potassium by a large number of fractional crystallizations. This was then converted into bicarbonate, of soda by saturating its aqueous solution successively with ammonium and carbon dioxide. The precipitated bicarbonate, after repeated washing, was converted into the normal carbonate by boiling its solution in water, and the crystals which separated on evaporation were freed from traces of chloride by repeated partial crystallization. The carbonate was next converted into fluoride by treatment with redistilled hydrofluoric acid originally prepared by distillation of hydrogen potassium fluoride, $HF \cdot KF$. The sodium fluoride thus obtained, after ignition at a red heat, was probably the purest specimen which has ever been obtained. As the result of five ignitions with sulphuric acid, the values obtained for the atomic weight of fluorine ranged from 19·04 to 19·08, when Na = 23·05 (Stas), S = 32·07 (Stas), and O = 16. The calcium and barium fluorides employed in the second and third series of determinations were obtained in microscopic crystals by precipitating potassium fluoride with calcium or barium chloride in dilute solutions of particular strengths. The values obtained in the case of four experiments with calcium fluoride varied from 19·02 to 19·08, and as the result of five determinations with barium fluoride, 19·05–19·09. As barium fluoride is not so regularly decomposed by sulphuric acid as sodium and calcium fluorides, M. Moissan considers that the nearest approximation to the truth is afforded by taking the mean of the experiments with the the two latter fluorides. This value is 19·05. Hence the atomic weight of fluorine may be considered, as has previously been supposed, to be practically represented by the whole number 19.

THE additions to the Zoological Society's Gardens during the past week include a Diana Monkey (*Cercopithecus diana* ♀) from West Africa, presented by Mr. Howard V. Henry; a Spotted Ichneumon (*Herpestes nepalensis*) from Nepal, presented by Mr. J. Percy Leith, F.Z.S.; a Polecat (*Mustela putorius*), British, presented by Mr. F. D. Lea Smith; two Laughing Kingfishers (*Dacelo gigantea*) from Australia, presented by Mr. W. B. Phillips; two Pomatorhine Skuas (*Stercorarius pomatorhinus*), British, presented by Mr. T. E. Gunn; a Cashmere Monkey (*Macacus pelops* ♀) from Cashmere, deposited; two Common Squirrels (*Sciurus vulgaris*), two Reed Buntings (*Emberiza schœniclus*), two Redpolls (*Linota rufescens*), British, purchased; an Angora Goat (*Capra hircus* ♀ var.), received in exchange; two Vinaceous Turtle Doves (*Turtur vinaceus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on October 30 = 0h. 36m. 39s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 105	—	—	0 34 23	+41 5
(2) G.C. 117	—	—	0 36 43	+40 16
(3) δ Piscium	4	Yellowish-red.	0 42 58	+ 6 59
(4) η Ceti	3	Yellowish-white.	1 2 31	−10 49
(5) θ Ceti	3	Whitish-yellow.	1 18 30	− 8 45
(6) α Andromedæ... ...	1	White.	0 2 42	+28 29
(7) 3 Schj.	8	Deep red.	0 14 5	+44 6
(8) R Vulpeculæ	Var.	Yellowish-red.	20 59 30	+23 23

Remarks.

(1) The spectrum of this bright nebula has not yet been recorded. The G.C. description is: "Very bright; very large; much extended in the direction 165°; very gradually very much brighter in the middle."

(2) This is the companion to the Great Nebula in Andromeda, which is described by Herschel as "exceptionally bright; large; round; pretty suddenly much brighter in the middle to a nucleus." With reference to the spectrum, Dr. Huggins notes: "This small but very bright companion of the Great Nebula of Andromeda presents a spectrum exactly similar to that of 31 M (the Great Nebula). The spectrum appears to end abruptly in the orange, and throughout its length is not uniform, but is evidently crossed either by lines of absorption or by bright lines." Referring to the Great Nebula, the same observer wrote: "The spectrum could be traced from about D to F. The light appeared to cease very abruptly in the orange; . . . no indications of the bright lines." A comparison of the two descriptions would lead one to suppose that the spectrum of the companion is the more discontinuous of the two, and, if this be the case, measurements of the positions of the brightnesses in the spectrum may teach us a good deal about the nebulæ which have so-called "continuous" spectra. After such a definite statement by Dr. Huggins as to the existence of irregularities, it is highly desirable that further observations and measurements should be made. Carbon comparisons (spirit-lamp flame) are suggested.

(3) A star of Group II., with bands 2, 3, 5, 7, 8 so narrow that Dunér describes them as "little more than lines." Narrow bands are common to both the early and late species of the

group, but in the former case the bright carbon flutings are strongly developed, whilst in the latter they are barely visible. Another characteristic of the later species is the addition of absorption-lines to the narrow bands. The observations of Mr. W. J. Lockyer and myself show that δ Piscium represents a late stage of the group, there being little or no carbon radiation.

(4, 5) Stars of the solar type. The usual differential observations are required.

(6) A star of Group IV. Observations of the relative thicknesses of the hydrogen and additional lines should be made, and the characters and positions of the latter noted. It appears that in some of these stars the added lines are similar to those seen in α Cygni, whilst others are solar.

(7) In this star of Group VI. the blue zone is very pale, the carbon band λ 564 is very wide and dark, and band 4 is suspected (Dunér).

(8) There will be a maximum of this variable on October 31. The period is about 138 days, and the range from 7·5–8·5 to 2·5–13·0. According to Dunér, the spectrum is one of Group I., but excessively weak. More details may possibly be obtained f the spectrum be observed at maximum. A. Fowler.

Spectroscopic Observations (Sawerthal's Comet 1881 I., and β Lyræ).—Dr. Nicolaus von Konkoly, the Director of the Astro-physical Observatory in O'Gyalla (Hungary) has issued the volumes containing his observations made during 1888-89. He notes, with respect to Sawerthal's comet (1881 I.):—

"I have observed the spectrum with a Merz universal spectroscope having one prism in position. This gave a dispersion of 8° (H to D), which was more than sufficient for my purpose. I was obliged to use this instrument, because the deviation it gave suited the focussing arrangement of the Kartaler refractor. The telescope of the spectroscope magnified seven times.

"The continuous spectrum was not very bright, and faded away slowly at each end. I thought that I could distinguish the D line (dark). It was so weak, however, that I could not fix its position with the micrometer. The continuous spectrum extended from 673 μ to 435 μ. Besides this I was able to recognize five hydrocarbon bands which I have located five times. From these measurements I have deduced the following mean values: I. 561·46 μ, II. 546·25 μ, III. 515·88 μ, IV. 513·26 μ, V. 472·56 μ.

"The lines were not sharply defined on either side, and were much widened near the continuous spectrum. The measurements given above are of the middle of the maximum light-intensity of the bands, which could be easily distinguished."

β Lyræ was spectroscopically observed on January 1, 1889, and it is recorded:—"The C line was bright and could be easily seen, and a dark band was visible at a slight distance from it. D_3 was distinguished in a similar manner. Near it, in the green, some fine lines could be perceived. F was suspected, but it was almost invisible."

Dr. Konkoly gives an extended account of his observations of Jupiter from 1885 to 1889, and accompanies it with a series of fourteen drawings of the planet. Several drawings of Sawerthal's comet are also given.

Spectroscopy at Paris Observatory.—M. Deslandres has charge of the spectroscopic section just created at Paris Observatory, and in the current number of *Comptes rendus* (October 20) he gives an account of the instruments to be used with the great equatorial (1·20 metres aperture). Those who have tried to obtain photographs of star spectra by means of a slit spectroscope on a large telescope, know how difficult it is to adjust a star on the slit, and, when there, to keep it in position for a sufficiently long time. To enable this to be done during a long exposure, M. Deslandres has arranged a total reflecting prism near the dark slide, so that the red end of the spectrum may be seen whilst the blue end is being photographed. In this way he has obtained many photographs of stellar spectra in juxtaposition with comparison spectra. To adjust the instrument for observing, the spectrum of a star, a small mirror, having a hole in the centre about the same diameter as the length of the slit, has been fixed in front of the slit at an inclination of 45°. The image of a star is thus reflected to the side of the instrument, and after another reflection reaches a small telescope fixed at the spectroscope. This telescope, therefore, gives the image of a star in the plane of the slit, and constitutes a veritable finder for use with the spectroscope.

ON THE LATER PHYSIOGRAPHICAL GEOLOGY OF THE ROCKY MOUNTAIN REGION IN CANADA, WITH SPECIAL REFERENCE TO CHANGES IN ELEVATION AND TO THE HISTORY OF THE GLACIAL PERIOD.[1]

Dr. G. M. Dawson has been engaged continuously for seventeen years in geological exploration of the Western Territories of Canada, including the country from Lake Superior to the Pacific; and in the paper above named he summarizes the history of the successive deposits and earth-movements which have built up the mountain ranges of the West, and the relations of these to the geology of the great plains to the eastward. He devotes special attention to the Glacial age, and concludes that the drift phenomena of the plains belong to a period of submergence, and that in the extreme period of glaciation there were great glaciers on the Cordillera on the west, and the Laurentian axis on the east, with a vast internal sea between. He is thus entirely opposed, as far as North America is concerned, to the idea of a Polar ice-cap or a great continental glacier flowing down the interior plateau of the continent, and he resolves the phenomena of the ice age into the operation of huge mountain glaciers and floating ice.

The leading points of the memoir may be summarized, with the aid of a few extracts, in such a manner as to convey a general view of the history of the great Cordilleran belt, which stretches along the west coast of America from Behring Straits to Cape Horn, and more especially to indicate that of its more northern portion.

The general structure of the country may be defined as follows:—

"At the present day, the western border region of the continent is formed by a series of more or less nearly parallel mountain-systems, with an average breadth in British Columbia of about 400 miles. The trend of these systems is north-west and south-east, or similar to that of the corresponding portion of the Pacific shore-line, the position of which, in fact, depends upon that of these orographic features. This generally mountainous zone of country is often referred to as the Rocky Mountain region, but is more appropriately named the Cordillera belt, the Rocky Mountains proper constituting only its north-eastern marginal range. In traversing it from east to west, in the southern part of the province of British Columbia, four distinct mountain-systems are crossed: (1) the Rocky Mountains proper, (2) mountains which may be classed together as the Gold Ranges, (3) the Coast Ranges, (4) an irregular mountain-system which in its unsubmerged parts constitutes Vancouver Island and the Queen Charlotte Islands, and which may be designated the Vancouver system. Between the second and third of these mountain-systems is a region without important mountain ranges, which is referred to as the Interior Plateau of British Columbia.

"To simplify our conception of the main features of this part of the Cordillera for our present purpose, we may, however, regard it broadly as being outlined on the north-east and south-west sides by the Rocky Mountains proper and by the Coast Mountains, as dominant ranges. This view is justified by the remarkable constancy of these two ranges and their relative importance. The intervening region may then be described as comprising the Interior Plateau together with the various ranges which have been grouped together under the name of the Gold Ranges, as well as other detached mountains and irregular mountainous tracts."

The geological history of British Columbia begins, like that of many other parts of the world, with that primitive crumpling of the earth's crust which produced the Laurentian gneisses. These exist principally in the Gold Ranges, and are in this region neither greatly extended nor of great elevation. In the Palæozoic age there were sea-bottoms receiving sediment, but apparently little mountain-making.

"Omitting, then, from consideration the imperfectly-known progress of events in the earlier stages of the geological history of the region, we may endeavour to picture to ourselves its condition in the Triassic or first stage in the Mesozoic division of geological time. The central region of the continent was at this time occupied by a very extensive, though shallow, mediterranean sea, which was either entirely cut off from the ocean

[1] By Dr. G. M. Dawson, F.G.S., Assistant Director of the Geological Survey of Canada. Transactions of the Royal Society of Canada, 1890. 73 pages quarto, with 4 maps.

or had only occasional and brief connection with it, and in which red beds with occasional layers of gypsum and salt were being deposited.[1] Rocks which represent a portion of the bed of this inland sea enter into the composition of the Rocky Mountain Range near the forty-ninth parallel, but are not known to occur to the north of that parallel for a distance of more than thirty or forty miles. To the west, they are not found in the Selkirk or Purcell Mountains. We appear, in fact, to discover in this vicinity the northern end of the inland Triassic sea. To the west of the Gold Ranges (under which term it will be remembered that the Selkirk, Purcell, and other mountains are grouped), deposits, also referable to the Triassic period, and more particularly to its upper part, are again found. These occur both on the mainland of what is now British Columbia and on Vancouver Island and the Queen Charlotte Islands. They contain truly marine fossils, and consist largely of materials of volcanic origin, which give evidence of contemporaneous volcanic activity on a great scale. To the north, in the Peace River country, and to the east of the present position of the Rocky Mountains, rocks holding the same marine forms are found, and they have quite recently again been discovered by Mr. McConnell in a similar position, still further north, on the Liard River.

"It would thus appear that in Triassic times the eastern border of the Pacific washed the western slopes of the Gold Ranges, and that where this mountain-system became interrupted, in its northern part, the sea was continued across its line, and covered a large tract of country to the east of the present position of the Cordillera belt.

"Precisely how far to the east the shore of this northern expansion of the Pacific was situated has not yet been determined. The region between it and the northern end of the inland sea previously referred to must have been a land area, which separated the open ocean of the north from the Mediterranean on the south. The Rocky Mountains proper had not yet been formed, nor is there any evidence of mountain ranges in the region of the Coast and Vancouver systems of to-day, though the volcanic action there in progress may have produced insular volcanic peaks. The deposits of the inland Triassic sea, including as they do beds of salt and gypsum, appear to prove the existence of a very dry climate in the area occupied by it; and as the land barrier separating it from the moisture-bearing westerly winds of the Pacific cannot have been wide, it must have been high. It is thus probable that the mountains of the Gold system formed at this time a lofty sierra, which was continued to the south of the forty-ninth parallel by the Cabinet, Cour D'Alaine, Bitter Root, and other mountains at least as far as the Wahsatch Range in Utah.

"The Triassic period was closed by one of those epochs of folding and dislocation of strata which are found to be recurrent in geological time, and which are generally attributed to the secular contraction of the earth's crust. The evidence of this time of change has been examined in greatest detail in the vicinity of the present coast-line, where it resulted apparently in outlining the Vancouver and Coast Ranges, and was accompanied by the production or extravasation of great masses of granitic rocks.[2] It is highly probable that some corrugation along the line of the Rocky Mountains occurred at the same period, as, in the next succeeding Earlier Cretaceous strata, without further evidence of disturbance, conglomerates are found to be composed of fragments of many varieties of the older rocks, which could scarcely otherwise have been rendered subject to denudation. Though much remains to be discovered respecting this post-Triassic epoch of disturbance, it was evidently an important one, and its results were wide-spread in the Cordillera region. It is quite possible that it was accompanied by, or resulted in producing, a general elevation of this entire region above the sea-level, as no rocks distinctly referable to the Jurassic or next succeeding period have yet been distinctly recognized either in British Columbia or in its bordering regions.[3] It must be borne in mind, however, that a portion of the red beds of the inland sea, described as Triassic, may extend upward into the Jurassic period, and that the marine Triassic fossils of the western and northern sea are referable to the later stages of the Triassic, or 'Alpine Trios' of the Cordillera region, comparable with the St. Cassian and Hallstadt beds of the Alps in Europe; while the beds of the Cretaceous next found are, according to European analogies, near the base of that formation."

"The next distinct record of the physical conditions of the region under discussion is afforded by the Earlier Cretaceous rocks. These, on the evidence of contained molluscan fossils, are regarded as about equivalent to the Gault of England, though the associated remains of plants are such as to admit their assignment to a somewhat older date. At this time, the immediately post-Triassic elevation had been followed by a subsidence of the land, resulting in the re-occupation by the open sea of the great area which had been similarly characterized in the Triassic. As in Triassic times, we find that this Earlier Cretaceous extension of the Pacific, to the north of the fifty-fourth parallel, spread eastward in a more or less connected manner completely across the present position of the Cordillera belt, while the Gold Ranges, and probably also other insular areas, continued to exist as dry land. In this case, as in that of the Triassic, it has not yet been found possible to outline exactly the eastern limit of the sea, in consequence of the want of sections cutting down to the base of the Cretaceous in the area of the Great Plains. There are, however, reasons for believing that it did not extend far beyond the line of the present foothills of the Rocky Mountains.

"In one important particular, the conditions in this Earlier Cretaceous period differed from those of the Triassic. There was at this time no isolated inland sea, and waters in connection with the main ocean stretched southward to the east of the Gold Ranges as far as the forty-ninth parallel and beyond it to a further distance which is as yet undetermined. This extension of the open sea thus actually overlapped, to a considerable extent, the area formerly occupied by the Triassic mediterranean."

This was followed, however, in Middle and Later Cretaceous times, by a great depression in which the marine beds of the Neobrava and Pierre Groups were deposited. This submergence was succeeded by some measure of elevation or folding, leading to the existence of vast swampy and lacustrine flats, in which the lacustrine and peat deposits of the Laramie formation of the great plains were formed. These deposits may be regarded as closing the Cretaceous era, or as transitional between it and the Eocene.

"This state of affairs was brought to a close by another of the recurrent epochs of folding and dislocation of the earth's crust, which was one of the greatest of those of which we find the results in the region under discussion, as well as the last of an important character to which this region was subjected. Under the influence of enormous pressure acting from the Pacific side, the nearly horizontal strata, which bordered the Gold Ranges on the north-east, were folded together and thrown up into a dominant ridge of Alps, which finally outlined the Cordilleran belt on this side. A similar folding and upthrust affected also the western marginal mountains which have been referred to as the Vancouver Range, but the action was there probably less violent and certainly affected a narrower zone. A portion of the crumpling to which the rocks of the Coast Ranges have been subjected was doubtless also produced at or about the same time, and certain granitic extrusions which cut the earlier Cretaceous rocks on its eastern flanks, as well as much of the flexure of these Cretaceous rocks, are also attributed to this period of disturbance.

"There is really no means of ascertaining what effect this disturbance produced in the region of the Gold Ranges, but it is more than probable that the whole width of the Cordillera then suffered changes and deformation of such a character that little if any trace of its surface contour of an older date can be found to-day.[1] It does not, however, necessarily follow that the

[1] Cf. "Note on the Triassic of the Rocky Mountains and British Columbia," Transactions of the Royal Society of Canada, vol. i., Section iv., p. 143.

[2] Cf. "Report of Progress, Geological Survey of Canada," 1878–79, pp. 46 B, 48 B; "Report of Progress, Geological Survey of Canada," 1886, p. 15 B.

[3] Certain rocks, from which fossils supposed at the time to be Jurassic were described, have since been found to belong to the Earlier Cretaceous. Cf. "Report of Progress, Geological Survey of Canada," 1876–77, p. 150; "Mesozoic Fossils," vol. i. p. 258.

[1] In respect to this great epoch of orographic movement, as evidenced particularly in the more southern part of the Cordillera, which has now been somewhat closely studied, Mr. S. F. Emmons may be quoted as follows:—"It is unquestionably one of the most important events in the orographic history of the entire Cordilleran system. With the exception of the great unconformity between the Archæan and all overlying sediments, which is a phenomenon *sui generis* and altogether exceptional, no movement has left such definite evidence as that which follows the deposition of the coal-bearing rocks to which the name Laramie has by universal consent been applied."—*Bulletin Geol. Soc. Amer.*, vol. i. p. 285.

general altitude of the Cordillera belt was at this time materially changed. The greater part of the accumulated pressure appears to have been relieved by folding along the lines of its two bordering ranges, and it seems to be not improbable, as a general proposition, that changes in elevation affecting wide areas are due to other causes than those producing mountain ranges.[1] We are warranted in assuming, however, that a certain movement in elevation was coincident, or nearly so, with that of the great disturbances above outlined, as no strata representative of the Eocene period proper have yet been found anywhere in the western part of Canada. The entire area of the Great Plains was thus sufficiently elevated to become dry land, as occurred at the same time in the Western States to the south of the international boundary."

The Eocene period thus witnessed the formation of the great interior table-land, which accordingly has present no aqueous formations of this age. In Miocene times, however, there were large interior lakes, with deposits rich in remains of plants and insects, and on the plains fluviatile gravels with mammalian bones.

The Pliocene period inaugurated another great continental elevation, which continued for a long period, and in which the fiords and cañons of the Cordillera were cut down by fluviatile action to the sea-level of the period. Many local illustrations are given in this memoir of the curious results in regard to denudation which this period of rest and elevation produced.

This leads to the glacial history of the region, the key to which is believed to be found in the unequal elevation whereby, while the great plains to the east remained under water, the Cordilleran Ranges became covered with a great glacier discharging north toward the Yukon Valley and the Arctic Sea, and south to Puget Sound, while glacial streams ran westward to the Pacific. At this time the Rocky Mountains produced but few and small glaciers on their eastern sides; but across the wide sea which covered the plains the Laurentian Mountains supported another *névé* discharging ice in all directions.

This was followed by what is usually called the inter-glacial period, when, as is believed, the plains were slightly elevated and the mountains depressed; and this was succeeded by the second glacial period, in which the mountain glaciers were comparatively small, and the depression of the plains was so great that water-borne boulders were deposited at elevations of 5000 feet or more on the foot-hills of the Rocky Mountains. It is to be noted here that the present eastward slope of the western plains had not yet been impressed on them. The series of events thus indicated is illustrated by the following table, which, however, the author regards as somewhat provisional:—

Scheme of Correlation of the Phenomena of the Glacial period in the Cordilleran region and the region of the Great Plains.

Cordilleran Region.	*Region of the Great Plains.*
Cordilleran zone at a high elevation. Period of most severe glaciation and maximum development of the great Cordilleran glacier.	Correlative subsidence and submergence of the Great Plains, with possible contemporaneous increased elevation of the Laurentian axis and maximum development of ice upon it. Deposition of the lower boulder-clay of the plains.
Gradual subsidence of the Cordilleran region and decay of the great glacier, with deposition of the boulder-clay of the Interior Plateau and the Yukon Basin, of the Lower boulder-clay of the littoral, and also at a later stage (and with greater submergence) of the inter-glacial silts of the same region.	Correlative elevation of the western part of the Great Plains, which was probably more or less irregular, and led to the production of extensive lakes in which inter-glacial deposits, including peat, were formed.
Re-elevation of the Cordilleran region to a level probably as high as or somewhat higher than the present. Maximum of second period of glaciation.	Correlative subsidence of the plains, which (at least in the western part of the region) exceeded the first subsidence, and extended submergence to the base of the Rocky Mountains near the forty-ninth parallel. Formation of second boulder-clay, and (at a later stage) dispersion of large erratics.
Partial subsidence of the Cordillera region to a level about 2500 feet lower than the present. Long stage of stability. Glaciers of the second period considerably reduced. Upper boulder-clay of the coast probably formed at this time, though perhaps in part during the last.	Correlative elevation of the plains, or at least of their western portion, resulting in a condition of equilibrium as between the plains and the Cordillera, their *relative* levels becoming nearly as at present. Probable formation of the Missouri Côteau along a shore-line during this period of rest.
Renewed elevation of the Cordillera region with one well-marked pause, during which the littoral stood about 200 feet lower than at present. Glaciers much reduced and diminishing, in consequence of general amelioration of climate toward the close of the Glacial period.	Simultaneous elevation of the Great Plains to about their present level, with final exclusion of waters in connection with the sea. Lake Agassiz formed and eventually drained toward the close of this period. This simultaneous movement in elevation of both great areas may probably be connected with the more general northern elevation of land at the close of the Glacial period.

[1] Cf. Le Conte, *American Journal of Science*, III. vol. xxxii. p. 178.

Among the evidences given of the partial submergences and differential elevations stated in this table, reference is made to the "White Silt formation" so extensively distributed in many parts of British Columbia, and indicating water action up to levels of about 2700 feet, to the high-level terraces; the peculiar distribution of boulders from the Laurentian highlands on the eastern slopes of the Rocky Mountains; the absence of glacial abrasion on the plains; the chemical character of the boulder-clay, leading to the inference that it was formed under water; the wide distribution and characters of the inter-glacial beds, the character and position of the Missouri Côteau, and a variety of other local facts. The objection that marine shells are not found in the Pleistocene strata is treated thus:—

"From what has already been said with respect to the Cordillera region, and more particularly in connection with the meaning which the White Silt formation appears to have in that region, it seems probable that the water by which the northern part of the Great Plains is supposed to have been flooded was in connection with that of the sea.[1] In discussing the results of my earlier investigations of the superficial deposits of this part of the plains, in reference to a theory of their submergence, I have stated that after a certain stage the waters entering from the north and south must have formed an open strait between the Arctic Ocean and the ocean to the south.[2] This was written, however, under an assumed limitation implying an equal subsidence of the continent; and at the time no satisfactory information was available respecting the position of the margin of the glacial deposits in the corresponding western part of the United States, such as has since been supplied by the work of Chamberlin, Salisbury, Todd, Wright, McGee, Upham, and others. The result of these new facts appears to show that, instead of opening broadly southward as well as to the north, any body of water covering the northern part of the Great Plains could have had only a tortuous and comparatively narrow communication with the sea to the eastward, round the front of the great confluent Laurentide glacier, and that even this communication was probably formed only at the time during which the plains stood at the lowest level indicated by the spread of the drift deposits. If such conditions may be assumed as probably

[1] It must still, however, be admitted as possible, that a great lake was in some manner produced, in the region of the plains, with a height somewhat exceeding that of the sea.

[2] "Geology and Resources of the Forty-ninth Parallel," p. 255.

representing the facts at the time, they go far toward explaining one of the greatest difficulties against the acceptance of the hypothesis that the waters by which the plains were flooded were in communication with those of the sea. The difficulty alluded to is the complete absence, so far as yet ascertained, of the remains of marine organisms from the glacial deposits. While prolonged weathering and the action of sub-aërial waters might result in the removal of calcareous organic remains from certain parts of these deposits, the condition of much of the boulder-clay, together with the occasional actual occurrences in it of fragments of Cretaceous or Laramie shells, is such as to show that any contemporaneous mollusks might have been preserved. If, however, the body of water in question, though communicating with the sea to the northward, was almost throughout closed to the south and in receipt of large quantities of fluvial water, it may well have been in great part brackish, if not almost entirely fresh. Adding to this the conception of its frigid temperature due to the great abundance of ice with which it must have been laden, and the vast amount of fine sediment which must have been carried into it by sub-glacier streams, it will be apparent that the conditions were singularly inimical to the existence of life of any kind, whether that characteristic of salt or fresh water. Somewhat similar conditions, though on a much smaller scale and without the adjunct of glacial waters or glaciers, occur in the southern extremity of Hudson Bay, where, as Mr. A. P. Low informs me, marine life is almost entirely absent, the water being nearly fresh and clouded with mud derived from the large entering rivers and from the action of the waves upon the shallow earthy shores."

Finally the climatal conditions deducible from the geological facts coincide with these facts in enforcing the probability that the great ice age of North America depended mainly on the existence of high mountains, surrounded by submerged areas, traversed by ice-laden currents, and that we are to imagine, not a continent covered with ice, but a submerged continent, with snow-clad mountains rising at its margins, and forming the gathering grounds of great local glaciers—the *Cordilleran*, the *Laurentide*, and probably the *Appalachian* glaciers. These were the favouring conditions, but the author does not venture to deny the co-operation of other and cosmical causes. He concludes as follows:—

"When the study of the superficial deposits of different parts of Europe and America was for the first time seriously begun, it was endeavoured to explain the phenomena entirely by diluvial action, and when the evidence of ice-action became insuperable, icebergs and floating ice only were at first admitted as factors. Since that time the pendulum of opinion appears to have swung to the opposite extreme, and the energies of the majority of investigators have been extended in endeavouring to account for the varied facts of what has become definitely known as the Glacial period, almost exclusively by the action of great confluent glaciers. From this extreme point, the pendulum may now be supposed to have returned so far, as to leave the hypothetical North Polar ice-cap almost without an advocate, but at what position it may eventually come to rest time alone can decide. I am aware that some of those who have accepted what I may perhaps be pardoned for characterizing as extreme views as to glacier action, have more or less completely, and to their own satisfaction at least, solved all difficulties opposed to the action of land ice, such as those presented by the facts met with over the Great Plains, by the application to these of their single universal menstruum. For myself I need only say that I have endeavoured to approach the subject of the glaciation of the north-western part of the continent, here reviewed, untrammelled by *a priori* theories, and with some personal familiarity in the field with nearly all parts of the region dealt with."

The above is only a hurried and imperfect summary. The paper itself, as containing the matured conclusions of long and thorough investigation by an able and earnest explorer, should be carefully read by all interested in the structure of the Great Cordilleran backbone of the American continent.

THE SCIENTIFIC INVESTIGATIONS OF THE FISHERY BOARD FOR SCOTLAND.[1]

THE results of the scientific investigations for 1889 are given in full detail with numerous tables and two charts. It was found that, during the year, no increase but rather a decrease, mainly in the migratory round fishes, occurred in the closed waters, and likewise in the open waters adjacent, the results of 1887 having been proved exceptional. The results of concurrent investigations carried on on board the *Garland* into the food of fishes, their spawning, and the distribution of the young, show that great and important differences—which must be taken into consideration both from a scientific and legislative point of view—prevail among the various food-fishes (Section A).

Dr. Wemyss Fulton, in his paper on the distribution of immature sea-fish and their capture by various modes of fishing, gives the results of the investigations into this important subject. The fundamental question as to what an immature fish is, has been determined for the first time by the examination and measurement of 13,000 fishes. The maximum size (as given in detailed tables) varies very much according to the species; any law regulating the legal sale of fish on the principle of size is therefore not based on scientific data. The distribution differs in the same way, but territorial waters serve as nurseries for the young fish. Tables given show the distribution of immature fishes at distances from the shore up to 22 miles and in various depths of water. Details are also given as to the proportion of immature food-fishes captured by the various modes of fishing. Dealing practically with the wasteful destruction of immature food-fishes, Dr. Fulton points out the difficulty of deciding among the different species, and shows how difficult it will be to save immature fish from capture and destruction by the beam-trawl, as that engine is now employed. The recommendations briefly are: that an inquiry should be made as to the retention of vitality by the various kinds of immature fish brought up in the trawl in order to ascertain the chances of survival if replaced in the sea; the protection of nurseries whose areas are capable of definition; the preservation of flat-fish under a certain size; and the establishment of hatcheries for sea-fish. A simple method is explained by which fishermen and trawlers might add to the fish-supply by fertilizing the ova of ripe fish when captured.

Regarding this Report, it should be noted that the importance of this question is not a thing which has dawned upon the Scottish Fishery Board since the conduct of scientific investigations was placed under new management (that of Dr. Fulton). In this paper, as in many others—indeed, everywhere in this year's Report—the willingness to ignore all that was done during the years that Prof. Ewart was convener of the Scientific Committee is very evident. In the Report of 1887 it is stated that "We have organized a series of extensive and systematic inquiries into the conditions of the reproductive organs of various kinds of fishes throughout the entire year, with particulars as to their size, &c., *which will help to clear up the hitherto obscure problems as to the minimum size of sexually mature individuals, &c.*" We have certain information that the original discoveries which led to this Report on immature fish were made by one who has done more than his share to redeem the work of the Fishery Board. And it is only fair that the credit which is Mr. T. Scott's due, and which is denied him there should be acknowledged here.

Dr. Fullarton furnishes a Report, with chart, on the cockle-beds of Barra (in the Hebrides), which furnishes the chief cockle-fishing in Scotland. It is desirable—and the wish of the fishermen themselves—that means should be taken to prevent the taking of undersized cockles, and to insure the working of the beds in rotation.

Dr. Fullarton also gives a paper on oyster-culture fauna in France and Holland. It is most desirable that scientific and practical measures should be taken to revive the Scotch oyster-fishing, which has gradually declined, and these should be planned on known and tried lines. The same must be said of the cultivation of mussels, and this should be urged even more strongly, as their scarcity or abundance are of the utmost importance to the line-fishermen. Lobsters also call for practical legislation and artificial cultivation. The Fishery Board are constructing a lobster inclosure at Brodwick, Arran, and hope that means will be provided for their carrying on operations at the recently established hatchery at Dunbar.

Section B contains the biological investigations. The food of fishes was dealt with in an elaborate Report by Mr. Ramsay Smith, based upon the examination of many thousand food-fishes which prove to live chiefly upon Crustacea, Annelids, Echinoderms, Mollusks, and upon one another. There are great differences, however, as to the proportion of the organisms selected as food by different fishes, and the proportions of the

[1] "Eighth Annual Report of the Fishery Board for Scotland." Part III. Scientific Investigations.

dietary vary to some extent at different places and at different seasons. These observations will ultimately demonstrate what organisms are valuable as fish-foods and what are not; the proportion in which the various vertebrates compose the dietary of fishes; and the possibility of introducing a valuable food-fish, such as the English sole, in places where it is absent or scarce. They will also show in what way the organisms forming the food of fishes may be protected and improved.

In a Report on the spawning and spawning-places of food-fishes, Dr. Fulton describes the results of the observations made during the year, many thousands of fishes having been examined on board the *Garland* all along the coast, and the duration of their spawning period in most instances determined. The duration of the spawning period varies much in different fish, and in some cases fully-grown adults appear not to spawn every year. The majority of the food-fishes congregate at the spawning time in immense shoals on the east coast at grounds lying from about eight to above twenty miles from shore in what may be termed the extra-territorial spawning zone. The young fishes are not, as a rule, found at the place of spawning, the floating pelagic eggs being carried by the currents chiefly shorewards. Dr. Fulton gives reasons for the belief that the selection of a particular offshore ground for spawning depends upon the set of the surface currents at the spawning season, these carrying the floating eggs during their development to the zones where food for the young fishes is abundant and shelter most readily secured.

Prof. W. C. McIntosh has made an elaborate study of the pelagic fauna of St. Andrews Bay, of which the second part, dealing with the distribution of the invertebrate organisms which form the food of many larval and other fishes, is now given.

Prof. McIntosh has also, in another paper, described the ova of the food-fishes and the larval and post-larval stages obtained in the *Garland's* tow-nets at various parts of the coast. These include the ova or larvæ of plaice, lemon-sole, flounders, dabs, cod, haddocks, ling, whiting, &c., and they constitute an indispensable part of the general study of the reproduction of the food-fishes.

Mr. Thomas Scott, in his valuable additions to the fauna of the Firth of Forth, gives a list of 80 species of organisms, not previously recognized as belonging to that locality. Some of these are for the first time recorded from the east of Scotland; some are new to Britain, and a few new to science. This paper is illustrated by two plates.

Mr. Scott, in his Report on the invertebrate fauna of inland waters, gives the result of the first investigations into the invertebrate organisms present in Scottish lochs and inland waters ever carried on in this country.

Dr. Fullarton's paper on the development of the clam is one which would hardly have found acceptance in any scientific journal. Had text and plates been submitted to the judgment of a skilled investigator, there would have been little or nothing of either for publication, as both display gross inaccuracy. There are many very remarkable statements in this paper, and the author naïvely describes as normal, phenomena whose pathological nature the merest tyro ought to be able to recognize.

Dr. John Beard, in his paper (illustrated by three plates) on the development of the common skate, gives the result of the study of this subject, on which very little has been written, though the skate is one of the most common elasmobranch fishes of our seas. The development of the embryo as it lies within its "purse" at the bottom of the seas occupies probably nine or ten months, being more rapid in summer than in winter. The eggs may be deposited throughout the year, but chiefly in March and April. Dr. Beard furnishes minute descriptions of the egg-cases or "purses" of the various species of skates and rays, and of the various stages in the development of the embryo. He discusses the function of the temporary external gills, so characteristic in advanced stages of development; and, in opposition to other authorities, he gives good reason for the belief that they are purely respiratory in function, and are adapted to the special conditions under which the developing embryo is placed.

Dr. Fulton, in his paper on the proportional numbers and sizes of the sexes among sea-fishes, gives the results of his inquiries, based upon the examination of 12,666 fishes. Females are, as a rule, more numerous than males; the female is also as a rule larger, but the male is the larger among the cod, haddock, and a few other fishes.

Among the "Notes and Memoranda" will be found Mr. Scott's hybridism among fish, the account of ingenious and interesting experiments made on board the *Garland* on the artificial fertilizing of the ova of certain species of sea-fishes with the milt of other species sometimes widely separated zoologically. Dr. Fulton sends interesting notes on the reproduction and migrations of the common eel, and on the presence of anchovies in Scotch waters. Regarding the former paper, it should be remembered that a German zoologist recently obtained a conger-eel at Zanzibar with eggs ten times the size of those here described. There is nothing really remarkable in the reproductive organs of the eel obtained at Howietoun, eels with eggs as large being very often caught. It has usually been estimated that the eel produces five millions of ova. The number is here increased to upwards of ten millions, and the method by which this was counted is not given. There is a lamentable looseness in quoting literature, even that of British zoology. We are told that *Myxine* is a protandric hermaphrodite, and that this was discovered by Nansen. As a matter of fact, the discovery was made by a Scotch naturalist (Cunningham), and within a short distance of Edinburgh. If Nansen's paper had been read as well as quoted, this misstatement would not have been made.

Section C contains notes on contemporary work relating to fisheries in this and other countries. We note that no mention is made, however, of the very important "Plankton" expedition of Germany of last year, which is the more to be wondered at as interesting accounts of the expedition have been published in Germany.

It is much to be regretted that the Fishery Board Bill of last year did not become law. The conduct of scientific investigations might then have been placed in different hands, with the result, among other things, that properly-organized scientific work would have been carried on by a thoroughly competent scientific staff, and the Government grant of £2000 a year usefully and judiciously expended, instead of being, as at present, frittered away because the dominant clique of the Fishery Board do not know what to do with it. The Scientific Department of the Fishery Board needs reorganization quite as much as the Fishery Board itself. Under the control and direction of the leading Scottish biologists, some adequate return ought to be made for the nation's money. To do this, however, the work must be properly planned and directed, and moreover the working of the different investigations given only to men who really understand their subject. Government has been asked, and is asked in the present Report, to furnish increased funds. We hope and believe that the authorities will be wise enough to stay their hand till they can reorganize everything connected with the Fishery Board.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Mr. Love, St. John's, and Mr. Coates, Queens', have been appointed Moderators, and Mr. Wallis, Corpus, and Mr. Burnside, Pembroke, Examiners for the next Mathematical Tripos, Part I.

Mr. L. Fletcher, Keeper of Minerals at the British Museum, and Mr. H. P. Gurney, of Clare College, are nominated Examiners in Mineralogy for the Natural Sciences Tripos.

Mr. H. M. Stanley was, on October 23, admitted to the honorary degree of LL.D.

The following communications were made to the Cambridge Philosophical Society at the annual general meeting on October 27:—The President, the origin and early years of the Society; Mr. C. Chree, on the vibrations of some simple systems; Dr. A. Gamgee, on the principle on which Fahrenheit constructed his thermometrical scale; Mr. H. J. Sharpe, on liquid jets.

The Harkness Scholarship for Women, tenable at either Girton College or Newnham College, Cambridge, is to be awarded triennially to the best candidate in an examination in geology and palæontology, provided that sufficient proficiency is shown. The candidates must be resident members of Girton or Newnham College, in their first or second term. The Scholarship will be of the value of about £35 a year for three years. The next award will be made in 1891. The examination will be held at Cambridge in the Michaelmas Term, and the award will be made on or before November 15, 1891. The intended range of examination is indicated by the following schedule:—General physical geography; such geological phenomena as are matters of common observation; the principal agencies which change or modify the earth's surface and the life on it; outlines

of the stratigraphy of the United Kingdom; outlines of the classification of organized beings, existing and extinct; the commoner rocks and rock-forming minerals, and the commonest and most characteristic British fossils. Candidates must send in their names, on or before October 12, 1891, to Miss A. Johnson, Llandaff House, Cambridge, from whom further information may be obtained.

SCIENTIFIC SERIALS.

American Journal of Science, October 1890.—A description of the "Bernardston Series" of metamorphic Upper Devonian rocks, by Prof. Ben. K. Emerson. With respect to this paper, Prof. J. D. Dana remarks:—"Prof. Emerson has given the region a thorough investigation, in which he has removed the doubts as to the relations of the beds, made out, as far as possible, the system of faults and flexures, studied the rocks as to their kinds and transitions, and determined the age of the series to be Upper Devonian. The paper will be accepted in America, and should be elsewhere, as putting the facts beyond doubt that gneiss, diorite, granite, and the other crystalline rocks described are not always of Archæan or pre-Cambrian make; that granite and diorite are not always of igneous origin; and these conclusions are made sure on the well-established criterion of age, that is, fossils—Crinoids, Corals, Brachiopods."—On the circular polarization of certain tartraté solutions, by J. H. Long. The author describes certain peculiarities of solutions of potassium antimony tartrate, when mixed with potassium or sodium carbonate, acetate, or phosphate in amounts insufficient to produce immediate precipitation. A decrease of specific rotation took place in the case of each of the mixtures. It is probable, therefore, that a temporarily stable antimony salt is formed with a corresponding amount of alkali tartrate. The observed rotation is due to this in conjunction with that of the potassium antimony tartrate which remained unchanged.—A rapid method for the detection of iodine, bromine, and chlorine, in presence of one another, by F. A. Gooch and F. T. Brooks.—Metacinnabarite from New Almaden, California, by W. H. Melville.—On the Keokuk Beds at Keokuk, Iowa, by C. H. Gordon.—Note on the vapour-tension of sulphuric acid, with the description of an accurate cathetometer microscope, by Dr. Chas. A. Perkins. The author finds that the vapour-tension is not greater than about 0·01 mm. at ordinary temperatures.—Experiments upon the constitution of the natural silicates, by F. W. Clarke and E. A. Schneider.—On five new American meteorites, by George F. Kunz. Descriptions and analyses are given of the group of meteorites recently discovered in Brenham Township, Kiowa County, Kansas; the Winnebago County, Iowa, meteorite; the meteoric stone from Ferguson, Haywood County, North Carolina; the meteoric iron from Bridgewater, Burke County, North Carolina; and the meteoric iron from Summit, Blount County, Alabama.—On the determination of the coefficient of cubical expansion of a solid from the observation of the temperature at which water, in a vessel made of thin solid, has the same apparent volume as it has at 0° C.; and on the coefficient of cubical expansion of a substance determined by means of a hydrometer made of this substance, by Alfred M. Mayer.

THE *American Meteorological Journal* for October contains articles:—On cyclical periodicity in meteorological phenomena, by E. D. Archibald, in which he advocates investigations as to the possible connection between weather and other physical agencies, on the following plan: (1) the collection and analysis of all previous investigations which bear traces of any value, and their distribution under the head of the particular element dealt with; (2) the arrangement of the periods in the matter of length; (3) the choice of the particular working hypothesis intended to be employed, and the working out of its supposed effects in different parts of the world; (4) the reduction and comparison of the data representing the various elements, and their comparison with the deductions from the hypothesis; (5) the investigation of the causes of apparent exceptions, and the exhibition of the final results, both in tabular and graphic form.—On accessory phenomena of cyclones, by H. Faye. The author draws attention to the theories of Redfield and Reid, and to the contradictory theories of Espy and Bache, from which he argues that only one conclusion could be drawn, viz. that there were two entirely different kinds of storms and tornadoes; and he refers to the advance made by the study of synoptic charts, both as regards the movements of cyclones and thunderstorms. The article contains an illustration of what he assumes to be a typical figure of a cyclone.—On temperatures in and near forests, by Prof. M. W. Harrington. The author shows that this subject admits of a much less satisfactory solution than that of soil temperatures discussed in an earlier paper. The observations used are from several sources over Central Europe, and refer in this part of the discussion to differences of temperature extremes. They show that the forest cuts off the mean daily maxima on the yearly average to the extent of 2° or 3°; the effect is most marked in summer and least in winter. The action of the forest on the minima of temperature is also a moderating one: the temperature does not on the average fall as low in forests as outside. With long-continued unchanging weather the peculiarities of forest climate tend to disappear.—On the Meteorological Section of the French Association for the Advancement of Science, held at Limoges in August last, by A. L. Rotch. The attendance of meteorologists was not large, but some important matters were discussed, among which may be mentioned the use of self-recording instruments on mountain stations, the subject being introduced by M. Teisserenc de Bort, and a paper on the recent seismic activity of Japan, by M. Y. Wada, of Tokio.

IN the *Journal of Botany* for October is a very interesting biographical sketch, accompanied by an excellent portrait, of the late Mr. John Ralfs, of Penzance, whose classical work, "The British Desmidieæ," one of the most valuable monographs ever published, was brought out as long ago as 1848. The value of this work may be judged from the fact that before its publication the number of species of Desmids recorded as British was four. An interesting note is given on the fertilization of the sugar-cane, by Dr. Fressanges, President of the Medical Society of Mauritius.

THE greater part of the number of the *Nuovo Giornale Botanico Italiano* for October is occupied by the completion of Signor L. Nicotra's interesting and important paper on the flora of Sicily. Going through the natural orders successively, he describes in general terms the representation of the order in the flora of the island, and points out the contrast between the flora of its north-eastern and that of its south-western portion, due to geological causes, the former having more of a European, the latter more of a North African character. Some particulars are added with regard to the flora of the small islands adjacent to Sicily. The remaining articles in the number are of special interest to Italian botanists.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 20.—M. Duchartre in the chair.—Study of the movement of a double cone which appears to rise, though it really descends, on an inclined plane, by M. H. Resal. A double cone placed on two guides inclined to the horizon, and nearer to one another at the lower than at the upper end, appears to ascend. The author has studied the mechanics of this movement.—Note on lightning-flashes which meet one another, by M. A. Trécul. On September 29, M. Trouvelot presented a paper on the identity in the structure of lightning and discharges from an induction machine. The author calls attention to the fact that he made similar observations ten years ago.—Observations of Brookes's comet (March 19, 1890), made with the great equatorial of Bordeaux Observatory, by MM. G. Rayet, L. Picart, and Courty. Seventy-one observations for position are given, extending from June 21 to October 12.—Remarks relative to a cause of variation of latitudes, by M. R. Radau. The movements of the sea, as well as certain meteorological phenomena (avalanches, &c.), may give rise to small deviations of the axis of our globe. It is shown that a mass of water 2000 cubic kilometres in size could produce an effect large enough to be observed.—On the established variations in the observations of the latitude of the same place, by M. A. Gaillot. Observations made at Berlin, Potsdam, and Prague, indicate that the latitude of a place is subject to a periodic variation, the maximum occurring in the summer, and the minimum in the winter, the amplitude of the oscillation about the mean value being $\pm 0''\cdot25$. M. Gaillot gives two hypotheses to account for this variation, and points out the means of testing each of them.

They are: (1) the axis of rotation is changed in the interior of the earth, the poles describing a circumference about the mean position, of which the radius is 0"·25 (7 or 8 metres); (2) the accepted periodic variation in observations of latitude is due to refraction phenomena.—Organization of spectroscopic researches with the great telescope of Paris Observatory, by M. Deslandres. (See Our Astronomical Column.)—Two solar prominences observed at the Haynald Observatory, Kalocsa (Hungary), by M. Jules Fényi. On August 15, at 9h. 39m., Paris mean time, a prominence reaching a height of 323", was observed on the western edge of the sun. Its base extended from $+37° 4'$ to $+44° 58'$ heliographic latitude. Another prominence was seen on August 18, at 11h. 45m., between $-41° 29'$ and $-55°$. This attained a height of 418", but was of a much more broken character than the preceding one.—On certain kinds of surfaces, by M. Lelieuvre.—Researches on the atomic weight of fluorine, by M. Henri Moissan. (See Notes, p. 649.)—Action of aromatic amines and of phenylhydrazine upon the β-ketonic nitriles, by M. L. Bouveault. The author establishes the generality of the reaction—

$$R—CO—C\begin{matrix}\diagup R' \\ \diagdown R''\end{matrix}—CN + R'''NH_2 = H_2O + R—\underset{\underset{NR'''}{\|}}{C}—C\begin{matrix}\diagup R' \\ \diagdown R''\end{matrix}—CN.$$

—On the mode of combination of sulphuric acid in plastered wines, and on a method of analysis permitting the distinction between the amount of the plastering and the acidification of the wine by sulphuric acid, by MM. L. Roos and E. Thomas. It is shown, by experimental means, that the sulphuric acid introduced by the plastering exists in the wine as K_2SO_4, and not as $KHSO_4$; hence, on precipitation of the H_2SO_4 by $BaCl_2$, the whole of the HCl will remain in combination, thus:—$K_2SO_4 + BaCl_2 = BaSO_4 + 2KCl$; and so the titrations of Cl by standard $AgNO_3$, taken (*a*) in filtrate from the $BaSO_4$ made up to a definite volume, and (*b*) in a fraction of the same filtrate evaporated to dryness, and then made up to the same fraction of the definite volume, should be exactly the same if no free H_2SO_4 be present; if free H_2SO_4 be present, a corresponding quantity of HCl will be lost to titration (*b*).—The saccharine matters in mushrooms, by M. Em. Bourquelot.—On the excretory apparatus of *Palinurus vulgaris*, *Gebia deltura*, and *Crangon vulgaris*, by M. Paul Marchal.—On the primitive conformation of the kidney of Pelecypodæ, by M. Paul Pelseneer.

STOCKHOLM.

Royal Academy of Sciences, October 8.—On the spectrum of absorption of bromium, by Prof. Hasselberg.—On the development of the Orthagoriscæ, by Prof. Smitt.—A report on entomological researches in the south of Sweden and Denmark, by Prof. Aurivillius.—Microscopical structures represented in coloured figures, which had been photographed by the firm Lumière, at Lyons, exhibited by Prof. Gyldén.—On the properties of a combination between nitrogen and hydrogen (HN_3) (discovered by Prof. Curtius, in Kiel), which in its free state, as well as in its combinations, has a most remarkable analogy with the hydrogen combinations of the haloids, and in consequence thereof has been named hydrazoic acid, reported upon by Prof. Nilson.—Studies of the crystal form of the arsenopyrite, by Dr. Weibutt.—Studies of naphthalene derivatives, by Dr. Paul Hellström.—Some observations on the anatomy of the subterranean elongations of the Gramineæ, by the same.—On the occurrence of *Dictyophyllum Nilssoni*, Brongn., in the coal-bearing strata of China, by Prof. Nathorst.—On Ribaucour's cyclic system, by Prof. Bäcklund.—Derivatives of ethyliden-disulphon-acids 1, and on 1, 4 fluor-naphthalin-sulphon-acid, by Herr Mauzelius.—Contributions to the knowledge of the moss flora of Canada, by Lector N. C. Kinberg.—Contributions to the theory of infinite determinants, by Herr H. von Kock.—On the conductibility of electricity through hot, saline vapours, by Dr. S. Arrhenius.

DIARY OF SOCIETIES.

LONDON.

SUNDAY, NOVEMBER 2.

SUNDAY LECTURE SOCIETY, at 4.—The Order of Nature—its Relation to Human Life and Happiness: A. Elley Finch.

MONDAY, NOVEMBER 3.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

TUESDAY, NOVEMBER 4.

ZOOLOGICAL SOCIETY, at 8.30.—On the Indian Gaur and its Allies: W. T. Blanford, F.R.S.—Description of a New Squirrel from the Philippine Islands: Dr. A. B. Meyer.—On a Cervine Jaw from Algeria: R. Lydekker.—Note on the Skull of the East African Reed-buck (*Cervicapra bohor*): Dr. A. Günther, F.R.S.

WEDNESDAY, NOVEMBER 5.

ENTOMOLOGICAL SOCIETY, at 7.—African Micro-Lepidoptera: Right Hon. Lord Walsingham, F.R.S.—A Monograph of British Braconidæ, Part IV.: Rev. T. A. Marshall.—New Species of Moths from Southern India: Colonel Charles Swinhoe.

THURSDAY, NOVEMBER 6.

LINNEAN SOCIETY, at 8.—A Contribution to the Study of the Relative Effects of different parts of the Solar Spectrum on the Assimilation of Plants: Rev. Prof. Henslow.

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